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Fading out effect or long lasting nudge? The impact of a Conditional Cash Transfer Program beyond starting the school year in Argentina[*]

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Abstract

We estimate the impact on education outcomes of the Universal Child Allowance (AUH), a massive conditional cash transfer program targeted at young children of unemployed and informal workers launched in Argentina in late 2009. Evidence from previous works suggests that the AUH has had a significant positive impact on attendance rates at the beginning of the school year, but concentrated on boys in upper-secondary school. In this paper we study the effects on other education outcomes: intra-year dropout rates and primary school completion rates. We find that the AUH may be held responsible for significant improvements in both outcomes while the analysis highlights heterogeneous effects across age groups and gender. In particular, the AUH seems to have contributed to reduce intra-year dropout rates of eligible girls aged 12 to 14 (almost 4 p.p.) and 15 to 17 (7 p.p.) while no effects were found for children aged 6 to 11 or for boys, irrespective of age. The program seems to have also increased the probability of graduating from primary school of over-aged eligible children (1.4 p.p. for boys aged 12 to 14, almost 3 p.p. for girls in that age range and 2 p.p. for boys in the 15-17 age group). These results suggest that beyond the effects on school access indicators, the AUH may also contribute to the improvement of final outcomes in education. Nevertheless, the evidence also indicates that there is room for improvements in the design of the program aimed at enhancing these long term effects.

*JEL Code***:** I2, I3

Keywords: conditional cash transfers, education, gender, Argentina, AUH.

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1. Introduction

The vast majority of social protection schemes in Latin America are based on Conditional Cash Transfer (CCT) programs, which consist of monetary transfers usually focused on vulnerable children and conditioned upon the fulfillment of certain education and health requirements. One of the main aims of such programs is to encourage the accumulation of human capital and therefore contribute to the breakdown of the intergenerational transmission of poverty. Therefore CCT programs articulate short and long term objectives: on the one hand, monetary transfers seek economic protection; on the other hand, conditionalities pursue social promotion (Fiszbein et al., 2009).

CCT programs have spread throughout Latin America since the 1990s: *Oportunidades* in Mexico, *Bolsa Família* in Brazil, *Familias en Acción* in Colombia, *Chile Solidario* in Chile, *Tekopora* in Paraguay, *Superémonos* in Costa Rica, *Bono de Desarrollo Humano* in Ecuador and *PANES* in Uruguay, among others. In Argentina, the Universal Child Allowance for Social Protection (AUH for its acronym in Spanish) was launched in late 2009. Beyond certain specificities, the AUH was designed as a CCT focused on children of unemployed, inactive or informal workers. The beneficiaries receive 80 percent of the transfers on a monthly basis, while the remaining 20 percent is transferred annually upon fulfillment of health controls and school attendance. Non-compliance with conditionalities implies the automatic loss of the 20 percent accumulated during the previous year and the risk of being suspended from the program. The AUH is the largest social program in Argentina: it benefits over 3.7 million children across the country and the monthly transfer implies a 50 percent increase in the household income of an average poor family. These figures suggest that both the income effect and the incentives to comply with the conditionalities could be potentially large.

In a recent work (Edo, Marchionni and Garganta, 2017), we studied the impact of the AUH on education outcomes, finding a positive and significant effect on secondary school net attendance rates at the beginning of the school year. Specifically, we found that the program may be held responsible for an almost 6-percentage-point increase in the probability that eligible boys aged 15 to 17 start the school year, while net attendance rates of girls in the same age group do not seem to have been affected by the AUH. Additionally, we showed that the effect on attendance rates decreases for younger children aged 12 to 14 and is virtually zero for primary-school-age children (6 through 11 years old). Based on these results and on the international evidence, we argued that these heterogeneities may be driven by differences in the baseline levels of attendance: effects are larger among those groups with lower initial net attendance rates. This is the case for boys aged 15 to 17, i.e. the expected age in upper secondary school in Argentina.

In the present paper we aim to address two core issues: whether the AUH affected other education outcomes beyond attendance rates, and whether the effects on these outcomes are heterogeneous across

gender and age groups. We focus the analysis on two education outcomes: (i) intra-year dropout rates and (ii) primary school completion rates. The incentives introduced by the AUH are designed to boost school attendance. As expected, this occurs during the first months of the school year, as shown in Edo et al. (2017) for certain groups. It remains unclear, however, whether the AUH incentives are enough to avoid dropout across the school year, given that this requires greater commitment and the possibility to overcome the restrictions that children and families face at school and at home. Therefore, the analysis of the first outcome aims at assessing whether the AUH contributes to sustain attendance beyond the beginning of the school year. The second outcome, instead, points at evaluating whether higher attendance and lower intra-year dropout eventually lead to improvements in primary school completion rates. Unfortunately, data limitations prevent us from assessing the AUH impact on secondary school graduation, as will become clearer later on.

We also explore the possibility that the impact of the AUH on each of these outcomes varies by gender and age group. In particular, we evaluate whether girls may have also benefitted from the AUH in terms of education or if, on the contrary, effects are still restricted to boys as we found in our previous work (Edo et al., 2017). To this end, we carry out a separate analysis for each of the following six groups: girls and boys aged 6 to 11, 12 to 14 and 15 to 17, where the three age groups considered correspond to the expected age for students in primary, lower-secondary and upper-secondary education, respectively.

Given the non-random assignment of the program as well as the absence of information for beneficiaries before the intervention, we evaluate the intention-to-treat effect through a difference-in-difference strategy based on data on the eligible and non-eligible population available from the Argentinean Permanent Household Survey (EPH). According to our results, the AUH had a positive impact on education results beyond school attendance, with heterogeneous effects across gender and age groups. In particular, the AUH contributed to reduce intra-year dropout rates of eligible girls: almost 4 percentage points for those aged 12 to 14 and 7 percentage points for the eldest (15 to 17 years old). These effects are very large when compared to the baseline dropout rates, since they imply a 94-percent (44-percent) reduction in the probability of intra-year dropout for eligible girls between 12 and 14 (15 and 17) years old. Instead, we find no effects on dropout rates for eligible boys or for younger children (6 to 11 years old) irrespective of gender. Regarding the impact on primary school completion rates, we find a mild positive effect for both girls and boys aged 12 through 14 (1.4 percentage points for eligible boys and almost 3 percentage points for eligible girls), while for the eldest children (15 to 17) the impact is statistically significant only for boys (2 percentage points). All these results hold across different specifications and robustness analysis.

This work contributes to the growing literature on the evaluation of CCT programs. First of all, we provide a new piece of evidence on the effects of this kind of programs on education outcomes beyond

school attendance. In particular, we study the impact of the Argentinean AUH in reducing intra-year dropout rates as well as increasing primary school completion rates. It is worth noting that improvements in these dimensions may contribute in the long run to enhancing 'final indicators'. In fact, inasmuch as reducing dropout rates and school-age gaps contribute to increase permanence in school, promotion to higher educational levels is fostered. In the long run, this may improve 'final outcomes' such as total years of formal education. In addition, this work aims at contributing to the literature that explores the heterogeneity of CCT programs' impact on education results providing new insights for further adjustments in the design of these programs.

The rest of the paper is organized as follows. The next section extends on the international evidence regarding the educational impact of CCT programs and describes the AUH program. The third section presents the data and methodology used for the empirical analysis while the following section discusses the results. Finally, section 5 concludes with some final remarks.

2. Impact of Conditional Cash Transfer Programs on Education Results

CCT programs aim to address simultaneously two different, yet interrelated, objectives. In the short term, monetary transfers are thought to protect the vulnerable population from economic shocks. In the long run, social promotion is pursued through human capital accumulation fostered by the conditionalities imposed (Fiszbein et al., 2009). Therefore, education outcomes are expected to improve as a result of CCT programs through two different channels: (i) by relaxing the budget constraints of poor households families may increase the demand for different goods and services, including education, i.e. the 'pure income' effect, and (ii) by incorporating an additional incentive to consume education (and health) through compliance with the conditionalities (Fiszbein et al., 2009).

The impact of CCT programs on different education outcomes has been extensively studied in the literature. Albeit strong differences across the findings, a broad conclusion points to a positive and significant impact on school access indicators, typically enrollment and attendance rates. The positive effects of CCT programs on enrollment and attendance rates are particularly important among the poorest children, whose enrollment rates are the lowest. Effects are also large during upper high school and in the transition years (i.e. from primary to secondary school), which are two key periods in terms of dropout. The generosity of transfers is another key factor in enhancing the positive impact of programs on school access indicators. Additionally, the imposition of conditionalities on school achievement beyond standard attendance conditions (e.g., school progression) is associated with higher enrollment and attendance levels (Saavedra and García, 2017). In general, however, impact assessments have not been able to

identify which channel predominates, i.e. what part of the impact is due to a pure income effect and which part responds to the incentives introduced by conditionalities (Fiszbein et al., 2009). Moreover, there is little evidence of improvements in 'final' education outcomes such as completed years of schooling or test scores (Saavedra and García, 2017). This suggests that in order to maximize the potential effects on human capital accumulation, CCT programs should be combined with other programs that improve the quality of the supply of education services, as well as implement conditions that focus on results rather than the mere use of educational services (Fiszbein et al., 2009).[1]

In Argentina, the Universal Child Allowance for Social Protection (AUH), launched in late 2009, was designed as a CCT program focused on children of unemployed, inactive and informal workers earning less than the minimum wage. Each beneficiary household can perceive a monthly transfer per child under 18 years old up to a maximum of five dependent children, while transfers for disabled children have no age limit. The AUH represents the largest social program in Argentina: it benefits over 3.7 million children across the country while the monthly transfer per child is equivalent to 15 percent of the legal minimum wage (ANSES, 2017). For a typical poor family with 3 children, the AUH represents an increase of around 50 percent in total household income.

The conditionalities within the program are defined in the following way. Beneficiaries receive 80 percent of the transfers on a monthly basis. The remaining 20 percent is retained each month and only transferred once a year, upon certification of compliance with health controls and school attendance. The latter condition applies to children between 5 and 18 years old, i.e. children in compulsory education age. Failure to comply with the certification procedure within a given schedule implies the automatic loss of the 20 percent accumulated during the previous, and could also lead to suspension from the program.

Evidence regarding the impact of the AUH on education outcomes is scarce. In a recent work (Edo, Marchionni and Garganta, 2017), we studied the effects of the program on net attendance rates at the beginning of the school year. By resorting to a difference-in-difference methodology based on microdata from the Argentinean National Household Survey for the 2004-2014 period, we found that the AUH rose secondary school net attendance of eligible boys aged 15 to 17 by almost 6 percentage points, while net attendance rates of girls in the same age group do not seem to have been affected by the AUH. We also showed that the effect on net attendance rates is smaller for lower-secondary-school-age children (12 through 14 years old) and negligible for primary-school-age children (6 through 11 years old). However, since at that time we focused on upper-secondary school, we did not explore heterogeneous effects by gender for the younger groups. Therefore, to provide some insight on this issue, we replicate here our

[1] Evidence is a little more encouraging with regard to the impact of CCT programs on early childhood cognitive development (Macours, Schady and Vakis, 2008). The results suggest that a very early intervention could produce larger returns than would be expected.

analysis in Edo et al. (2017) to assess the impact of the AUH on the probability of attending *any* school level for children aged 6 to11 and 12 to 14, by gender (see Table A.1 in Appendix A). Again, the impact of AUH on school attendance at the beginning of the school year is restricted to boys, although, as mentioned before, the effects are considerably smaller than those of the eldest group (less than 1 percentage point for boys aged 12 to 14 and even smaller for the youngest group).

Beyond methodological and data differences, other works find similar results, i.e. a positive impact on school attendance, but they do not deepen into the analysis of heterogeneities across gender while using data from a much narrower time window (Jiménez and Jiménez, 2016; Cigliutti et al., 2015; Paz and Golovanevsky, 2014). Conversely, D'Elia and Navarro (2013) find preliminary evidence that the program may have widened the schooling gap of children aged 6 to 13 but not of older children (14 to 17), even though the analysis compares only years 2009 and 2010.

The present work provides new evidence regarding the impact of the AUH on education results beyond school attendance, namely intra-year dropout rates and primary school completion rates. Furthermore, we focus on unraveling the heterogeneous effects of the AUH on different gender and age groups. In the following section we describe the data and methodology used.

3. Data and empirical strategy

Assessing the impact of the AUH remains elusive for several reasons. In the first place, the program was not randomly assigned thus imposing severe limitations to the definition of an adequate control group. Secondly, there is no publicly available dataset containing information on the beneficiaries *before* the implementation of the program. We thus base the analysis on data from the *Encuesta Permanente de Hogares* (EPH), a national household survey carried out by the National Statistics Office in Argentina (INDEC). Even though the EPH does not allow for identifying the actual beneficiaries of the program, it is still possible to identify eligible children in the survey by checking whether the program's eligibility conditions are met. Therefore, we perform an intention-to-treat analysis.[2]

The treatment group is defined as girls and boys aged 6 to 17 years old, whose parents are inactive, unemployed, informal or self-employed workers.[3] As for the control group, it includes all children aged 6 to 17 for whom at least one of their parents is employed in the formal sector. We restrict both groups to

[2] This is the same empirical strategy we follow in Edo, Marchionni and Garganta (2017).

[3] The Special Social Security Scheme for Domestic Service Employees (Law 25,239, Title XVIII) states that children whose parents are registered employees working in the domestic service are also eligible for the AUH and hence are included in the treatment group.

children belonging to the first four deciles of the per capita family income distribution.[4] We focus on the 2004-2014 period for intra-year dropout rates; for primary school completion rates the period observed includes the first semester of 2015.[5] Since the AUH was launched in November 2009 we take years 2004 through 2009 as the pre-intervention period.

We resort to a difference-in-difference methodology to estimate the intention-to-treat impact on our two results of interest: (i) intra-year dropout rates and (ii) primary school completion rates. We define intra-year dropout rate as the percentage of children dropping out of school during a given school year out of the total number of children attending school at the beginning of the same school year. As for the definition of primary school completion rate we take the percentage of children in a given age group that completed the primary education level.[6] In particular, we compare the differences in the probability of dropping out of school throughout the year as well as of graduating from primary school of the treatment and control groups, before and after the inception of the program. This requires two identification assumptions. On the one hand, we need to assume that there was no other contemporaneous event to the implementation of the AUH that could have caused differences in the evolution of these results between the treatment and control groups. This does not appear to be a strong assumption considering no major initiatives affecting education outcomes took place in late 2009. The other assumption that needs to hold is that trends of both indicators (intra-year dropout rates and primary school completion rates) for the treatment and control groups would have evolved similarly in the absence of the program. Even though this assumption may not be proven, we provide evidence in its favor further on through common trends tests and placebo experiments.

As for the difference-in-difference model, we use the standard linear specification in equation (1).

$$Education\ Outcome_i = \propto + \beta_1 Treat_i + \beta_2 After_i + \gamma(Treat_i . After_i) + \theta\ X_i + u_i \quad (1)$$

The dependent variable *Education Outcome* represents one of our two results of interest. To account for intra-year dropout, we focus on children who attend school at the beginning of the school year (first quarter) and define the dependent variable as a binary indicator that takes the value 1 if the child is not

[4] Even though eligibility also requires that parents' earnings be below the minimum legal wage, earnings are almost impossible to monitor for unregistered workers. Nevertheless, qualitative and quantitative evidence suggests that middle and high-income informal workers opt out of the program due to social responsibility and stigma, and hence the inclusion error is presumably small. For instance, information from the national expenditure survey for 2012 (ENGHo) reveals that beneficiary children are strongly concentrated on the poorest income deciles, despite the absence of income checks when assigning the program. See Garganta et al. (2017) for further details.

[5] Even though both outcomes of interest follow the empirical strategy outlined in this section, each of them requires a specific treatment of the data given the particular definitions employed. In particular, intra-year dropout rates rely on the rotating panel scheme of the EPH which limits the time span of observation. For further details refer to Section 4 and Appendix B.

[6] The analysis of this result is restricted to children aged 12 to 17, given that 12 is the expected age to finish primary school in Argentina and that only children under 18 years of age are eligible for the AUH.

attending school by the end of the school year (last quarter), and 0 otherwise. Regarding primary school completion, we focus on children older than 12 years old (i.e. the theoretical age to finish primary school) and define the dependent variable as a binary indicator that takes the value 1 if the child graduated from the primary level and 0 otherwise. Unfortunately, data limitations prevent us from assessing the impact on secondary school completion, since that by the age children finish secondary school in Argentina (17 or higher) most of them are no longer eligible for the AUH.[7] As for the right hand side of equation (1), *Treat* is an indicator variable identifying the treatment group; *After* tags years after the AUH implementation (2010-2014), and X includes a set of controls at the child and head of household level (child's age and squared age; head of household's gender, age, squared age, educational level and employment status) as well as other household characteristics (household size, per capita family income, single parent household, female headed household, number of children under 18). We also control for time (year and quarter) and regional fixed effects, as well as for regional trends.[8]

Assuming that the unobserved characteristics that remain after controlling for all these variables do not have a differential impact on the dependent variables across groups (treatment and control) between the two periods (before and after the implementation of the AUH), we may claim that the γ parameter represents the causal effect of the program.

4. Results

a. Intra-year Dropout Rates

To compute the intra-year dropout indicator, we resort to the rotation scheme of the EPH that allows us to track the same individual and household over time. Specifically, if a household enters the sample in quarter t (which could be any quarter of the year) it would also be surveyed in quarters t+1, t+4 and t+5.[9] Since the school year in Argentina goes from March to December, we can use the rotating panel that entered the EPH sample in the last quarter of a given year (t) to follow the children attending school at the beginning of the following year (t+1) and check whether they are still attending school by the end of the

[7] This analysis would be possible if retrospective data of young people aged 18 or older containing information on their eligibility conditions when they were at most 17 years old were available. Moreover, in this case we would need to assume that there was no other event later in their lives that could have affected their chances to complete secondary school. Such an assumption would not be valid since in 2014 the *Progresar* program was implemented, focused on helping young people aged 18 through 24 to continue their studies even though they can no longer benefit from the AUH.

[8] Evaluating the effect of the AUH on each outcome of interest (intra-year dropout and primary school completion rates) requires some differentiation of this common empirical strategy, driven by the nature of the data on which the analysis is based. These differences are detailed in the following sections.

[9] For a detailed explanation of the rotational scheme of the EPH see Appendix B.

same year (t+5).[10] Therefore, our variable of interest, *dropout*, is a binary indicator variable that takes the value 1 if the child was attending school in *t+1* but was not in *t+5*¸ and it takes the value 0 if he/she was attending school in *t+1* and continued to do so in *t+5*.[11] It is important to note that the sample that we obtain in this way, which we use to estimate the impact of the AUH on intra-year dropout rates, may be quite small, especially as we are interested in evaluating heterogeneous effects: each rotating panel represents 25 percent of the total sample and we further restrict the analysis to children of a given age and gender attending school at the beginning of the school year.[12]

Table 1 displays intra-year dropout rates for the treatment and control groups both before and after the program implementation. The (unconditional) diff-in-diff results are heterogeneous by age and gender. For the youngest group aged 6 to 11 (the theoretical primary school age range), and irrespective of gender, we find no significant change after the AUH in the difference between the treatment and control groups. It is important to note, however, that baseline intra-year dropout rates were very low for that age-range (less than 1 percent), even though we are focusing on poor children (first four deciles). Intra-year dropout rates are higher for those aged 12 to 14 (i.e., the transition age from primary to secondary school), especially for boys in the treatment group where about 6 percent of those attending school at the beginning of the year dropout before the school year ends. However, when comparing eligible and non-eligible boys before and after the implementation of the AUH we find virtually no changes in the gap between the two groups over time. Instead, dropout rates fell by around 2 percentage points after the AUH inception for eligible girls aged 12 to 14, which compared to the 1.3-percentage-point increase for the control group implies a significant diff-in-diff contraction of 3.4 percentage points. The results for the group between 15 and 17 years old are qualitatively similar to the group aged 12-14 but of a larger magnitude. While intra-year dropout rates for eligible girls significantly fell compared to the control group (the difference between the two groups decreased by almost 6 percentage points after the AUH was implemented), the diff-in-diff for boys is 2 percentage points in favor of the treatment group but not

[10] Given the rotational scheme of the EPH intra-year dropout rates may be evaluated in two different ways: (i) by assessing child's attendance during the first and last quarter of the year –as we do in this paper-; (ii) by checking child's attendance in the second and third quarters, i.e. selecting households that are sampled for the first time during the second quarter of the year (Figure B.1 in Appendix B may be useful for visualizing both strategies). Results based on (i) or (ii) do not differ significantly. We opted for presenting results based on (i) because it specifically captures child attendance over the whole school year. Results using the alternative sample (ii) are available upon request.

[11] By the time this research was carried out, the latest microdata available of the EPH included the first two quarters of 2015. Given that this definition of intra-year dropout rates requires observing the same child on the first and last quarter of the year, 2014 was the last year for which this indicator could be computed. Figure B.1 in Appendix B may be helpful in understanding this limitation.

[12] A back of the envelope calculation can illustrate this point. Around 87.000,000 children between 6 and 17 years old were interviewed during the period under review (2004-2014). When restricting the sample to fit our criteria (children attending school and entering the sample during the last quarter of the year and belonging to households in the first 4 deciles of the income distribution) the sample is reduced to around 15,000 observations. Estimation samples get even smaller when we further partition the sample into age and gender groups (6 to 11, 12 to 14 and 15 to 17 years old; girls and boys).

statistically significant. Even though preliminary, this evidence suggests a positive effect of the AUH in terms of reducing intra-year dropout rates, at least for eligible girls aged 12 and older.

Table 1. Intra-year Dropout Rates

By age and gender

Boys	6 to 11			12 to 14			15 to 17		
	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)
Before AUH	0.4	1.0	***0.6***	2.2	5.7	***3.5***	16.1	20.0	***3.9***
After AUH	0.7	1.0	***0.3***	2.7	6.3	***3.6***	13.3	15.2	***1.9***
Difference (After-Before)	***0.3***	***0.0***	***-0.3***	***0.5***	***0.6***	***0.1***	***-2.8***	***-4.8***	***-2.0***

Girls	6 to 11			12 to 14			15 to 17		
	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)
Before AUH	0.5	0.6	***0.1***	1.9	4.1	***2.2***	8.1	15.8	***7.7***
After AUH	0.7	1.6	***0.9***	3.2	2.0	***-1.2***	11.0	13.0	***2.0***
Difference (After-Before)	***0.2***	***1.0***	***0.8***	***1.3***	***-2.1***	***-3.4*****	***2.9***	***-2.8***	***-5.7****

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: our sample is based on the rotating panel that entered the EPH in the last quarter of the previous year (see Appendix B for more detail) and includes all children from the first four deciles of the per capita family income distribution who attend school at the beginning of the school year. Intra-year dropout takes the value of 1 if the child does not attend school in the last quarter of the year; 0 if he/she continues attending. *Treatment Group* includes children whose parents are either inactive, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children for whom at least one of their parents is employed in the formal sector. *Before AUH* includes years 2004-2009 while *After AUH* includes years 2010-2014. Robust standard errors clustered by region; * p<0.10, ** p<0.05, *** p<0.01.

This preliminary evidence, however, also highlights the fact that treatment and control groups differ in their initial dropout rates. Indeed, potential beneficiaries of the AUH show systematically higher rates of intra-year dropout than children in the control group. This is not surprising given the fact that both groups are different by construction; the program was assigned non-randomly and focused on the more vulnerable population, and thus we expect systematic differences between eligible and non-eligible children in terms of demographic, economic and social characteristics. Table A.2 in Appendix A corroborates this presumption. Indeed, regardless of age or gender, treatment and control groups differ in almost all of the characteristics considered. Children in the treatment group belong to households where the head is more likely to be a woman and/or a single parent, less likely to be employed and has lower educational attainment. Furthermore, children in the treatment group generally belong to larger households with a larger number of young children. Finally, as expected, eligible children are significantly poorer than children of formal workers. Similar results hold with slight variations across gender-age groups, before and after the implementation of the AUH. These differences certainly explain to some extent the gap between both groups in terms of baseline levels of intra-year dropout rates.

Nevertheless, it is important to note that despite of this gap in levels, the pre-intervention trends for the treatment and control groups are similar. Indeed, according to a series of pre-program common-trend tests that we performed for each gender-age group, there is not enough evidence to reject the null hypothesis that the pre-intervention trends for the treatment and control groups were equal, which reinforces the confidence in our identification assumption.[13]

We now evaluate whether the preliminary results presented in Table 1 hold in a multivariate diff-in-diff framework and are robust to several types of controls. Table 2 shows the results of estimating the linear model of the probability of intra-year dropout from equation (1), for each gender and age groups, using the whole set of controls described in the previous section.[14] In general, the results from the unconditional diff-in-diff in Table 1 still hold in this conditional setting. In particular, we find no evidence that the AUH had any effect on the probability of intra-year dropout of girls or boys aged 6 to 11 (the coefficients in columns (1) and (2) are close to zero and not significant). Furthermore, we also find no evidence of effect for boys in any of the age groups (columns (1), (3) and (5)). Even though for the eldest boys aged 15 to 17 the coefficient is negative and relevant in magnitude (3.28 percentage points), it is not statistically significant (column (5)).

Girls, however, show clear signs of having benefitted from the program: our results suggest that the AUH had a positive effect in terms of preventing girls from dropping out of school during the school year. Indeed, for eligible girls aged 12 to 14, the program may be held responsible for reducing intra-year dropout rates by almost 4 percentage points (column (4)). Most strikingly, for the eligible eldest girls aged 15 to 17, the impact almost doubles, reaching 7 percentage points (column (6)). These effects are considerable if we take the pre-intervention levels as a reference: they imply a 94-percent (44-percent) reduction in the probability of intra-year dropout for eligible girls between 12 and 14 (15 and 17) years old. Still, it is also worth noting that we find no effect among the youngest girls: for those aged 6 to 11, the coefficient is virtually zero and not statistically significant.

It is interesting to relate these results to those in Edo et al. (2017), where we found a positive effect of the AUH on secondary school net attendance rates at the beginning of the school year for boys aged 15 to 17 but not for girls. Additionally, we showed that for younger children the impact on attendance rates is still restricted to boys, although smaller than for the eldest group.[15] The results from the present paper

[13] We run a model of our outcome of interest (intra-year dropout rates) on a constant, the treatment dummy, year dummies and the interactions between these latter variables including only pre-intervention years. We then apply an *F* test in which the null hypothesis states that all the coefficients for the interaction terms are jointly equal to zero. We repeat the same procedure for each gender-age group and find no evidence to reject the null for any of the groups. Table A.3 in Appendix A displays the p-values associated to the F-Statistics.

[14] The results are robust to the inclusion of different subsets of controls. Results are available upon request.

[15] See the discussion in Section 2 and Table A.1 in Appendix A, where we replicated the analysis from our previous work in Edo et al. (2017) to assess the impact of the AUH on the probability of attending school by gender for children aged 6 to 11 and 12 to 14.

complement those previous findings. On the one hand, they suggest that although the AUH did not increase attendance rates for girls, it did help prevent those attending school at the beginning of the year from dropping out during that school year. On the other hand, the fact that attendance increased for eligible boys but dropout rates remained unchanged could be indicative of a composition effect: even if the AUH had reduced male dropout rates among those already in school (similar to the impact found for girls), the effect may be offset by the higher dropout probabilities among those "new" boys who started school due to the program.

Table 2 – Diff-in-diff Estimation Results (in percentage points): Linear Probability Model of Intra-Year Dropout

By age and gender

Age Range	6 to 11		12 to 14		15 to 17	
	Boys (1)	Girls (2)	Boys (3)	Girls (4)	Boys (5)	Girls (6)
Treatment*After	***-0.272***	***0.610***	***-0.383***	***-3.869****	***-3.282***	***-7.047*****
	(0.454)	***(0.720)***	***(1.634)***	***(1.837)***	***(3.837)***	***(2.560)***
Treatment	0.745***	0.105	2.516*	1.725	2.799	6.150**
	(0.146)	(0.543)	(1.290)	(1.298)	(2.233)	(2.252)
After	4.240***	-8.380***	-6.459	-1.084	5.991	60.38***
	(0.716)	(0.688)	(12.78)	(2.090)	(12.80)	(19.68)
Child and Head of HH characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Other HH characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Regional and Time dummies, Regional Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,863	3,517	1,948	1,842	1,553	1,573

Source: own estimations based on *Encuesta Permanente de Hogares*.

Note: our sample is based on the rotating panel that entered the EPH in the last quarter of the previous year (see Appendix B for more detail) and includes all children from the first four deciles of the per capita family income distribution who attend school at the beginning of the school year. OLS Estimations. Dependent binary variable: *Dropout*, equals 1 if the child attended school in the first quarter of the year but was not doing so by the end; it equals 0 for those attending school in the first and last quarter of the year. *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* equals 1 in the period 2010-2014 and 0 for the period 2004-2009; child´s and/or head of household´s characteristics (child's age and squared age, head of household's gender, age, squared age, educational level and employment status), other household characteristics (household size, per capita income, single parent household, female headed household, number of children under 18), region fixed effects (6 regions), time fixed effects (year and quarter) and regional time trends. Robust standard errors clustered by region in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Placebo Experiments

We perform a series of false experiments or placebo exercises on those groups for which we claim the AUH may be held responsible for reducing the intra-year dropout rates, that is, girls aged 12-14 and 15-17. The results provide evidence that allows for gaining more confidence in the validity of the identification assumption. The experiments consist of running the same linear probability model but using only pre-treatment observations while pretending that the program took place in any year previous to 2009 – the actual implementation date of the AUH. Table 3 shows the results for five alternative fake

dates: 2004, 2005, 2006, 2007 and 2008. In all cases the coefficient accompanying the interaction term is not statistically significant, suggesting that it was only after 2009 that some event shifted the intra-year dropout rates of eligible girls between 12 and 17 years old, but clearly not before.

Table 3. Diff-in-diff Estimation Results (in percentage points): Linear Probability Model of Intra-year Dropout

Placebo Experiments

	Girls 12 to 14					Girls 15 to 17				
	Intervention in					Intervention in				
	2004	2005	2006	2007	2008	2004	2005	2006	2007	2008
Treatment*After	1.947	0.223	1.717	4.023	4.023	3.680	-2.696	-2.560	-4.953	-6.922
	(1.414)	(2.654)	(2.261)	(3.070)	(3.070)	(2.499)	(4.205)	(3.781)	(4.313)	(5.543)
Child and HH head's characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	938	938	938	938	938	777	777	777	777	777

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: our sample is based on the rotating panel that entered the EPH in the last quarter of the previous year (see Appendix B for more detail) and includes all children from the first four deciles of the per capita family income distribution who attend school at the beginning of the school year. OLS estimations. Dependent binary variable: *Dropout*, equals 1 if the child attended school in the first quarter of the year but was not doing so by the end; it equals 0 for those attending school in the first and last quarter of the year; *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* is defined ad-hoc for each year (for example in 2006 it equals 0 in the period 2004 to 2006 and 1 in the period 2007-2009). For a description of control variables included, refer to Table 2. Robust standard errors clustered by region in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

b. Primary School Completion Rates

To estimate the impact of the AUH on primary school completion rates we do not need to resort to the rotating panels of the EPH; it is enough to observe the proportion of children of at least 12 years of age -the expected age to finish primary education- who graduated from that level.[16] Consequently, the sample size expands considerably compared to the previous analysis. Furthermore, we are also able to extend the time span to include the first semester of 2015.

Table 4 shows primary school completion rates for the treatment and control groups, before and after the program implementation. In all cases, the proportion of primary school graduates is higher in the control group, but the improvement in this indicator between the pre- and post-intervention periods is greater among eligible the children although the magnitude differs by age and gender. The unconditional diff-in-diff effect for the group aged 12 to 14 is 1.1 percentage points for boys and almost doubles (2.1 percentage points) for girls. Regarding the eldest children (15 through 17 years old), primary completion rates for eligible boys increased by around 2 percentage points more than those of the control group. For

[16] In Argentina, primary school starts at the age of 6 and consists of 6 or 7 years of schooling, depending on the district (province). This implies that by the age of 12 children are expected to have graduated from that level.

the eldest girls, instead, the improvement in graduation rates among the treatment and control groups was virtually the same. These results, although certainly preliminary, suggest a positive effect on primary completion rates that may be attributed to the AUH.

Table 4. Primary School Completion Rates.

By age and gender

Boys	12 to 14			15 to 17		
	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)
Before AUH	65.4	56.7	-8.7	93.7	87.3	***-6.4***
After AUH	77.9	70.3	-7.7	96.4	91.8	***-4.6***
Difference (After-Before)	***12.6***	***13.6***	***1.1***	***2.7***	***4.5***	***1.9***

Girls	12 to 14			15 to 17		
	Control (i)	Treatment (ii)	(ii)-(i)	Control (i)	Treatment (ii)	(ii)-(i)
Before AUH	71.8	64.0	-7.8	96.2	93.1	***-3.1***
After AUH	81.5	75.7	-5.8	97.8	94.9	***-2.9***
Difference (After-Before)	***9.7***	***11.7***	***2.1***	***1.6***	***1.9***	***0.2***

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: the sample includes all children aged 12 to 17. *Treatment Group* includes children whose parents are either inactive, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children for whom at least one of their parents is employed in the formal sector. *Before AUH* includes years 2004-2009 while *After AUH* includes years 2010-2015. Robust standard errors clustered by region; * p<0.10, ** p<0.05, *** p<0.01.

Once again, the systematic differences in the baseline graduation rates in favor of the control group is a consequence of the non-random assignment of the program, which is focused on the most vulnerable population. Indeed, children belonging to the treatment and control groups differ not only in educational characteristics but also in many other demographic and social dimensions as we discussed previously (see Table A.4 in Appendix A). Despite these differences, which are likely to explain part of the initial gap in graduation rates between the two groups, the pre-intervention trends in primary school completion rates of treatment and control groups seem to be parallel according to a series of pre-treatment common trends tests. This is true for all age-gender groups.[17]

We now evaluate whether the preliminary results presented in Table 4 hold in a multivariate difference-in-difference framework and are robust to the inclusion of controls. Table 5 shows the results of estimating the linear model of the probability of graduating from primary school from equation (1), for

[17] We run a model of our outcome of interest (primary school graduation rates) on a constant, the treatment dummy, year dummies and the interactions between these latter variables including only pre-intervention years. We then apply an *F* test in which the null hypothesis states that all the coefficients for the interaction terms are jointly equal to zero. This is repeated for each gender-age group. We find no evidence to reject the null hypothesis of pre-intervention parallel trends in any of the tests. The p-values associated to the F-Statistics are displayed in Table A.5 in Appendix A.

each gender and age group, while including all the controls described in the previous section.[18] In general, these results are similar to those from the unconditional analysis in Table 4.

Our results suggest a positive impact of the AUH on the probability that eligible children complete primary school, although again the magnitude of the effect varies with age and gender. For eligible children aged 12 through 14, the effect is always significant: the AUH may be held responsible for an increase of almost 3 percentage points in the probability that eligible girls graduate from primary school (column (2)), while this effect is less than half for eligible boys (1.4 percentage points, column (1)). Compared to the pre-intervention levels, these figures imply a 4.4-percent (2.5-percent) increase in the probability of graduating from primary education for eligible girls (boys) between 12 and 14 years old. Regarding children aged 15 to 17, the effect is relevant and statistically significant only for boys: the AUH may be held responsible for a 2-percentage-point increase in the probability that eligible boys complete primary education (column (3)), which represents a 2.4-percent increase compared to the pre-intervention levels. Instead, for girls aged 15 to 17 the estimated impact is negligible and not statistically significant (column (4)). The latter result, however, should not be interpreted as evidence on the lack of effect of the AUH on education attainment of the oldest group of girls. As Table 4 shows, most eligible girls aged 15 to 17 have a primary-complete education level, even before the intervention (93 percent). Moreover, their net attendance rates at the beginning of the school year are high (80.3 percent) and, according to the results of the previous subsection, their intra-year dropout rates decreased because of the AUH. Putting all together, it is very likely that the AUH had increased the chances of successfully completing secondary school for eligible girls aged 15 to 17. Unfortunately, as we mentioned before, data limitations prevent us from providing evidence to support this hypothesis.

[18] The results presented in Table 5 are robust to the inclusion of different subsets of these controls. Results are available upon request

Table 5. Diff-in-diff Estimation Results (in percentage points): Linear Model of the Probability of Graduating from Primary School

By age and gender

Age Range	12 to 14		15 to 17	
	Boys (1)	Girls (2)	Boys (3)	Girls (4)
Treatment*After	***1.371****	***2.836****	***2.055*****	***0.563***
	(0.674)	***(1.137)***	***(0.443)***	***(0.562)***
Treatment	-4.634***	-4.304**	-3.852***	-1.682***
	(0.780)	(1.182)	(0.435)	(0.251)
After	-0.126	5.679***	-2.650***	-0.638
	(0.516)	(0.675)	(0.300)	(0.491)
Child and Head of HH characteristics	Yes	Yes	Yes	Yes
Other HH characteristics	Yes	Yes	Yes	Yes
Regional and Time dummies, Regional Trends	Yes	Yes	Yes	Yes
Observations	47,580	45,900	45,652	43,622

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: the sample includes all children aged 12 to 17. OLS estimations. Dependent binary variable: *Completion*, equals 1 if the child graduated from primary school and 0 otherwise. *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* equals 1 in the period 2010-2015 and 0 for the period 2004-2009; child´s and/or head of household´s characteristics (child's age and squared age, head of household's gender, age, squared age, educational level and employment status), other household characteristics (household size, per capita income, single parent household, female headed household, number of children under 18), region fixed effects (6 regions), time fixed effects (year and quarter) and regional time trends. Robust standard errors clustered by region in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Placebo Experiments

We carry out the same placebo experiments described for the intra-year dropout rates in order to gain confidence in the validity of the identification assumption. We focus on those groups for which we claim the AUH may be held responsible for increasing the primary school completion rates: eligible boys and girls between 12-14 and boys aged 15 through 17 years old. Results are displayed in Table 6: in all cases the coefficient accompanying the interaction term is not statistically significant. Once again, the evidence suggests that it was only after 2009 that some event had a differential impact on the primary school completion rates of eligible and non-eligible children, but clearly not before.

Table 6. Diff-in-diff Estimation Results (in percentage points): Linear Model of the Probability of Graduating from Primary School

Placebo Experiments

	Boys 12 to 14					Girls 12 to 14					Boys 15 to 17				
	Intervention in					Intervention in					Intervention in				
	2004	2005	2006	2007	2008	2004	2005	2006	2007	2008	2004	2005	2006	2007	2008
Treatment*After	1.536	0.614	0.990	1.710	2.046	2.022	0.459	1.013	0.441	0.856	-0.801	0.705	1.095	0.229	1.544
	(2.225)	(1.523)	(1.767)	(1.572)	(1.045)	(1.318)	(1.176)	(1.116)	(1.087)	(1.179)	(1.455)	(1.325)	(1.127)	(0.605)	(1.160)
Child and HH head's characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,765	24,765	24,765	24,765	24,765	23,767	23,767	23,767	23,767	23,767	23,018	23,018	23,018	23,018	23,018

Source: own estimations based on *Encuesta Permanente de Hogares*.

Note: the sample includes all children aged 12 to 17. OLS Estimations. Dependent binary variable: *Completion*, equals 1 if the child graduated from primary school and 0 otherwise. *Treatment* equals 1 for eligible children and 0 for non-eligible children; *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* is defined ad-hoc for each year (for example in 2006 it equals 0 in the period 2004 to 2006 and 1 in the period 2007-2009). For a description of control variables included, refer to Table 5. Robust standard errors clustered by region in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

5. Concluding remarks

Increasing human capital accumulation in order to reduce the intergenerational transmission of poverty is one of the core aims of CCT programs. This ambitious goal is embodied in the very design of these programs, which requires beneficiaries to comply with certain conditionalities. In Argentina, a massive CCT program was launched in late 2009: the Universal Child Allowance for Social Protection (AUH for its acronym in Spanish). The AUH is focused on children under 18 years old whose parents are inactive, unemployed or working in the informal sector. As any typical CCT program, the AUH requires compliance with education and health conditions: vaccination and health checks for children under age 4 and for pregnant women, and school attendance for children aged 5 through 18. To enforce these conditions the program sets a particular payment mechanism: 80 percent of the subsidy is automatically received by beneficiary families on a monthly basis, and the remaining 20 percent is paid annually, once compliance with the conditionalities is proven. If the conditions are not met, not only the accumulated 20 percent is not perceived but also the beneficiary is likely to be suspended from future participation in the program.

In terms of education results, we showed in a recent work (Edo et al., 2017) that the AUH may be held responsible for increasing the probability of school attendance of eligible children. The effect is mainly concentrated in boys aged 15 through 17, for whom the probability of attending secondary school increased by almost 6 percentage points thanks to the AUH. For younger children the impact is smaller, although still significant and concentrated on boys, while the AUH does not seem to have improved girls attendance rates for any age-group.

Following a similar methodology based on a difference-in-difference analysis, in this paper we study the effects of AUH on intra-year dropout rates and primary school completion rates. In general, we find that the AUH may be held responsible for positive impacts on these education results, while the effects vary across gender and age groups. In particular, we find a sizeable impact of the AUH on reducing intra-year dropout of eligible girls: the estimated effect is almost 4 percentage points for girls aged 12 to 14 and 7 percentage points for the eldest group aged 15 to 17. Compared to the pre-intervention levels, these effects are very large: they imply a 94-percent (44-percent) reduction in the probability of intra-year dropout for eligible girls between 12 and 14 (15 and 17) years old. Nonetheless, we find no evidence of effects on the probability of intra-year dropout for children aged 6 to 11 or boys irrespective of their age. Regarding the latter result, however, we argue that it is possible that the AUH did contribute to reduce dropout rates of eligible boys but that this effect remains concealed because of a composition effect. Indeed, even if the AUH helped boys already in school to sustain attendance throughout the year –an impact similar to the one found for girls-, the effect may be offset by the higher dropout probabilities among those "new" boys who started school due to the program -a result not present among girls-.

Concerning the effects on primary school completion rates, our results suggest a positive impact of the AUH for eligible children, although again the magnitude of the effect varies with age and gender. The effect is significant for almost all the gender and age groups: 1.4 percentage points for eligible boys aged 12 to 14, 2.1 for boys aged 15 to 17, and 2.8 percentage points for girls aged 12 to 14. Compared to the pre-intervention levels, these effects imply that the probability of graduating from primary education increased about 2.5 percent for eligible boys and 4.4 percent for eligible girls between 12 and 14 years old. Instead, for girls aged 15 to 17 the estimated impact on primary school completion is negligible and not statistically significant. Nonetheless, we call for a cautious interpretation of this latter result, which should not be interpreted as evidence of the lack of effect of the AUH on education attainment of the oldest group of girls. On the contrary, based on all available evidence, we believe that it is very likely that the AUH had increased the chances that eligible girls aged 15 to 17 can, eventually, graduate from secondary school. Unfortunately, data limitations prevent us from evaluating this hypothesis.

Interpreting these results in light of our previous work (Edo et al., 2017) depicts a fairly consistent panorama of the AUH's impact on education results. In the first place, it seems that the program is not affecting any education outcome for children aged between 6 and 11 years old: while attendance rates show a negligible positive impact concentrated on boys, we find no effects at all for intra-year dropout rates of neither boys or girls in that age group. Although this is clearly related to the very high baseline levels of both indicators, it is also indicative of the fact that the AUH may not be enough to attract the few but very vulnerable children in primary-school age that still remain out of the educational system. Secondly, all our results indicate that the AUH is indeed affecting education outcomes for those aged 12 to 17. Regarding girls, even though the AUH does not seem to be enough to attract the most vulnerable who are still out of the educational system, it is contributing to improve the educational trajectories of those who attend school, reducing intra-year dropout and increasing the chances of completing primary school at ages closer to the theoretical upper limit. As for boys, the AUH is improving their chances to start or return to school, which may also explain part of the increase we find in their chances of completing primary education.

Further research should focus in disentangling the mechanisms through which the results are being achieved. As discussed in Section 2, CCT programs may affect education results through two channels: transfers and conditionalities. So far, it remains unclear which of them (or whether a combination of both) is responsible for the positive results found for the AUH in terms of attendance, intra-year dropout and primary school completion. Exploring these issues would certainly yield benefits in terms of further adjustments of the AUH design.

Finally, regarding public policy recommendations, it is important to note that even though our results emphasize the positive effects of the AUH on education, they also draw attention to the limitations of the

program in attracting to (or maintaining in) school some small, yet very vulnerable, groups of children. This highlights the need of strengthening or implementing well designed complementary policies that focus on these specific groups. This surely requires a much greater and fine-tuned effort, with policies that complement monetary transfers with other accompanying measures and support for children and their families. In this sense, the failure to fulfill the requirements imposed by the conditionalities could be useful in identifying these groups of vulnerable children. Furthermore, it may be worthwhile to review the design of the program in order to strengthen the incentives to achieve education outcomes beyond school attendance, such as promotion to higher grades and graduation from the different levels.

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Appendix A.

Table A.1. The impact of AUH on the probability of attending school

Age Range	6 to 11		12 to 14	
	Boys (1)	Girls (2)	Boys (3)	Girls (4)
Treatment*After	0.693***	0.194	0.911*	0.742
	(0.176)	(0.279)	(0.467)	(0.473)
Treatment	-0.599***	-0.139	-1.815***	-1.199***
	(0.144)	(0.165)	(0.381)	(0.279)
After	-0.414	-3.052**	-2.001	-3.382
	(0.408)	(1.375)	(2.301)	(2.132)
Child and Head of HH characteristics	Yes	Yes	Yes	Yes
Other HH characteristics	Yes	Yes	Yes	Yes
Regional and Time dummies, Regional Trends	Yes	Yes	Yes	Yes
Observations	35,616	33,716	17,672	17,232

Source: own estimations based on Table 8 in Edo et al. (2017).

Note: our sample is based on the rotating panel that entered the EPH in the last quarter of the previous year (see Appendix B for more detail) and includes all children from the first four deciles of the per capita family income distribution who attend school at the beginning of the school year. OLS estimations. Dependent binary variable: *Attends*, equals 1 if the child attends any school level; *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* equals 1 in the period 2010-2014 and 0 for the period 2004-2009; child´s and/or head of household´s characteristics (child's gender, age and squared age, head of household's gender, age, squared age, educational level and employment status), other household characteristics (household size, per capita income, single parent household, female headed household, number of children under 18), region fixed effects (6 regions), time fixed effects (year and quarter) and regional time trends. Robust standard errors clustered at regional level in parenthesis; * p<0.10, ** p<0.05, *** p<0.01. For more details, see Edo et al. (2017)

Table A.2. Descriptive Statistics – Sample used in the estimation of intra-year dropout

		6 to 11 - Boys							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	**Age**	8.5	8.7	-0.2	0.10	8.6	8.5	0.1	0.32
Head of HH	**Age**	41.0	42.1	-1.0	0.05	42.0	41.3	0.7	0.15
	Female head (%)	41.8	17.6	24.2	0.00	39.2	22.6	16.6	0.00
	Single parent (%)	36.2	13.9	22.2	0.00	32.6	16.5	16.1	0.00
	Education (Years)	7.9	9.3	-1.4	0.00	8.5	9.9	-1.5	0.00
	Employed (%)	72.9	90.4	-17.5	0.00	70.2	88.3	-18.1	0.00
HH	**HH Size**	6.0	5.9	0.1	0.57	5.9	5.6	0.3	0.01
	Number of Children	3.5	3.4	0.2	0.03	3.3	3.0	0.3	0.00
	Per Capita Income ($)	167.7	279.0	-111.3	0.00	583.3	838.2	-254.8	0.00
	Observations	*1297*	*717*			*1116*	*733*		

		6 to 11 - Girls							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	**Age**	8.6	8.5	0.1	0.38	8.5	8.6	-0.1	0.24
Head of HH	**Age**	41.3	41.1	0.2	0.69	42.1	41.8	0.3	0.57
	Female head (%)	42.1	17.8	24.3	0.00	40.8	22.4	18.4	0.00
	Single parent (%)	34.4	14.4	20.0	0.00	33.0	16.3	16.6	0.00
	Education (Low-High)	7.9	9.5	-1.5	0.00	8.3	10.0	-1.7	0.00
	Employed (%)	72.4	90.5	-18.1	0.00	72.0	88.3	-16.2	0.00
HH	**HH Size**	6.1	6.0	0.1	0.31	5.8	5.7	0.1	0.47
	Number of Children	3.6	3.4	0.2	0.05	3.2	3.1	0.1	0.17
	Per Capita Income ($)	163.5	266.4	-103.0	0.00	590.8	898.2	-307.4	0.00
	Observations	*1188*	*652*			*1004*	*673*		

		12 to 14 - Boys							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	**Age**	13.0	12.9	0.0	0.75	13.0	13.0	0.0	0.73
Head of HH	**Age**	44.9	43.1	1.8	0.01	44.5	43.6	0.9	0.17
	Female head (%)	40.3	15.7	24.7	0.00	42.1	24.7	17.5	0.00
	Single parent (%)	37.4	13.8	23.6	0.00	37.1	16.9	20.2	0.00
	Education (Years)	7.9	9.1	-1.3	0.00	8.4	9.8	-1.3	0.00
	Employed (%)	74.1	91.6	-17.5	0.00	70.3	87.4	-17.1	0.00
HH	**HH Size**	6.1	6.1	0.0	0.87	5.9	5.7	0.2	0.16
	Number of Children	3.5	3.5	0.0	0.68	3.3	3.1	0.2	0.08
	Per Capita Income ($)	175.7	271.1	-95.4	0.00	610.5	876.2	-265.7	0.00
	Observations	*652*	*370*			*553*	*373*		

Table A.2 (continued). Descriptive Statistics - Sample used in the estimation of intra-year dropout

		12 to 14 - Girls							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	**Age**	13.0	13.0	0.0	0.92	13.0	13.0	0.0	0.95
Head of HH	**Age**	44.0	44.1	-0.1	0.91	44.6	43.8	0.8	0.25
	Female head (%)	44.0	16.5	27.5	0.00	41.6	26.0	15.6	0.00
	Single parent (%)	37.5	12.7	24.8	0.00	38.0	19.1	18.9	0.00
	Education (Years)	8.0	9.0	-1.1	0.00	8.2	9.9	-1.8	0.00
	Employed (%)	73.5	91.6	-18.1	0.00	74.0	88.7	-14.7	0.00
HH	**HH Size**	6.1	6.2	-0.1	0.54	6.0	5.7	0.3	0.02
	Number of Children	3.5	3.4	0.1	0.36	3.4	3.1	0.3	0.01
	Per Capita Income ($)	169.6	264.5	-94.9	0.00	583.2	862.2	-279.1	0.00
	Observations	*616*	*322*			*558*	*346*		

		15 to 17 Boys							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
	Age	15.9	15.9	0.0	0.79	16.0	16.0	0.0	0.65
Head of HH	**Age**	46.9	45.5	1.4	0.04	47.4	45.6	1.8	0.01
	Female head (%)	39.3	22.4	17.0	0.00	41.0	24.0	17.0	0.00
	Single parent (%)	38.3	20.2	18.1	0.00	39.1	19.5	19.6	0.00
	Education (Years)	8.2	9.2	-0.9	0.00	8.6	10.0	-0.9	0.00
	Employed (%)	75.2	93.4	-18.2	0.00	71.6	90.6	-19.0	0.00
HH	**HH Size**	5.7	5.9	-0.2	0.22	5.6	5.7	-0.1	0.33
	Number of Children	3.1	3.1	0.0	0.99	2.9	2.9	0.0	0.78
	Per Capita Income ($)	200.2	277.4	-77.2	0.00	670.4	882.7	-212.4	0.00
	Observations	*460*	*317*			*468*	*308*		

		15 to 17 Girls							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	**Age**	15.9	15.9	0.0	0.58	15.9	15.9	0.0	0.78
Head of HH	**Age**	46.8	45.7	1.1	0.13	46.0	45.2	0.8	0.27
	Female head (%)	40.1	21.8	18.3	0.00	46.7	26.0	20.7	0.00
	Single parent (%)	36.4	18.8	17.5	0.00	41.8	19.9	21.9	0.00
	Education (Years)	8.3	9.7	-1.3	0.00	8.6	9.4	-0.8	0.00
	Employed (%)	74.1	93.4	-19.2	0.00	68.0	90.2	-22.2	0.00
HH	**HH Size**	5.7	6.1	-0.3	0.03	5.7	5.7	0.0	0.93
	Number of Children	3.1	3.2	-0.1	0.27	3.0	3.0	0.0	0.81
	Per Capita Income ($)	196.2	283.4	-87.2	0.00	639.5	886.7	-247.1	0.00
	Observations	*506*	*271*			*469*	*327*		

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: the sample includes all children attending school in the first quarter of the year who belong to households interviewed for the first time during the last quarter of the previous year. *Treatment Group* includes children whose parents are either inactive, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children for whom at least one of

their parents is employed in the formal sector. *Before AUH* includes years 2004-2009 while *After AUH* includes years 2010-2014. *Number of Children* is the total number of children under 18 living in the household. *HH* stands for household.

Table A.3 Pre-Treatment Common Trends Tests. P-values associated to the F-Statistic

Intra-year dropout rates

	6 to 11	12 to 14	15 to 17
Boys	0.7362	0.4112	0.7287
Girls	0.5312	0.4091	0.9548

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: P-Values associated to the F-Statistic of running a model of our outcome of interest (intra-year dropout rates) on a constant, the treatment dummy, year dummies and the interactions between these latter variables including only pre-intervention years. We then apply an F test in which the null hypothesis states that all the coefficients for the interaction terms are jointly equal to zero. This is repeated for each age-gender group.

Table A.4. Descriptive Statistics - Sample used in the estimation of Primary School Completion

		12 to 14 Boys							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
Child	Age	13.5	13.5	0.0	0.31	13.5	13.4	0.0	0.02
Head of HH	Age	45.0	44.3	0.7	0.00	45.0	44.5	0.5	0.00
	Female head (%)	37.8	18.1	19.7	0.00	43.3	23.4	19.9	0.00
	Single parent (%)	35.7	14.5	21.2	0.00	38.5	17.5	21.0	0.00
	Education (Years)	7.8	9.3	-1.5	0.00	8.3	9.9	-1.5	0.00
	Employed (%)	72.8	89.7	-16.9	0.00	70.4	88.0	-17.6	0.00
HH	HH Size	6.1	6.0	0.1	0.00	5.8	5.7	0.1	0.00
	Number of Children	3.6	3.4	0.2	0.00	3.3	3.1	0.3	0.00
	Per Capita Income ($)	180.7	297.6	-117.0	0.00	780.1	1151.0	-371.0	0.00
	Observations	*16082*	*8683*			*13727*	*9088*		

		12 to 14 - Girls							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
	Age	13.5	13.5	0.0	0.81	13.5	13.5	0.0	0.71
Head of HH	Age	44.9	44.2	0.7	0.00	45.2	44.3	0.8	0.00
	Female head (%)	39.5	19.0	20.5	0.00	44.0	24.8	19.2	0.00
	Single parent (%)	36.3	16.4	19.9	0.00	38.9	17.7	21.2	0.00
	Education (Years)	7.7	9.3	-1.5	0.00	8.3	9.9	-1.6	0.00
	Employed (%)	73.2	90.0	-16.8	0.00	69.7	87.5	-17.8	0.00
HH	HH Size	6.1	6.0	0.1	0.00	5.9	5.7	0.1	0.00
	Number of Children	3.6	3.4	0.2	0.00	3.4	3.1	0.2	0.00
	Per Capita Income ($)	180.0	296.9	-116.9	0.00	781.4	1145.7	-364.3	0.00
		15591	*8176*			*13271*	*8862*		

		15 to 17 - Boys							
		Before				After			
	Age	16.5	16.4	0.0	0.08	16.4	16.4	0.0	0.25
Head of HH	Age	47.5	46.3	1.2	0.00	47.4	46.4	0.9	0.00
	Female head (%)	39.4	19.3	20.1	0.00	44.6	25.2	19.4	0.00
	Single parent (%)	38.1	15.8	22.3	0.00	40.4	19.9	20.5	0.00
	Education (Years)	7.7	9.2	-1.5	0.00	8.2	9.8	-1.6	0.00
	Employed (%)	69.7	90.5	-20.8	0.00	67.8	88.4	-20.5	0.00
HH	HH Size	6.0	6.0	0.0	0.13	5.8	5.7	0.1	0.05
	Number of Children	3.3	3.2	0.1	0.00	3.1	3.0	0.2	0.00
	Per Capita Income ($)	198.9	308.2	-109.4	0.00	832.9	1174.6	-341.7	0.00
		15072	*7946*			*13922*	*8712*		

		15 to 17 - Girls							
		Before				After			
		Treatment group	Control group	Difference	P-value	Treatment group	Control group	Difference	P-value
	Age	16.4	16.4	0.0	0.90	16.4	16.4	0.0	0.25
Head of HH	Age	47.3	46.2	1.1	0.00	47.2	46.3	0.8	0.00
	Female head (%)	39.9	20.7	19.2	0.00	44.7	26.1	18.6	0.00
	Single parent (%)	37.7	17.8	19.9	0.00	41.5	20.2	21.4	0.00
	Education (Years)	7.9	9.3	-1.4	0.00	8.3	9.8	-1.5	0.00
	Employed (%)	72.2	90.5	-18.3	0.00	69.2	89.1	-19.9	0.00
HH	HH Size	6.0	6.0	0.0	0.90	5.9	5.8	0.1	0.00
	Number of Children	3.3	3.2	0.1	0.00	3.2	3.0	0.2	0.00
	Per Capita Income ($)	196.3	303.7	-107.3	0.00	811.4	1171.8	-360.4	0.00
		14738	*7481*			*13065*	*8338*		

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: *Treatment Group* includes children whose parents are either inactive, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children for whom at least one of their parents is employed in the formal

sector. *Before AUH* includes years 2004-2009 while *After AUH* includes years 2010-2014. *Number of Children* is the total number of children under 18 living in the household. *HH* stands for household.

Table A.5 Pre-Treatment Common Trends Tests. P-values associated to the F-Statistic

Primary School Graduation Rates

	12 to 14	15 to 17
Boys	0.3983	0.3488
Girls	0.5555	0.5617

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: P-Values associated to the F-Statistic of running a model of our outcome of interest (primary school graduation rates) on a constant, the treatment dummy, year dummies and the interactions between these latter variables including only pre-intervention years. We then apply an F test in which the null hypothesis states that all the coefficients for the interaction terms are jointly equal to zero. This is repeated for each age-gender group.

Appendix B. Rotational Scheme of the *Encuesta Permanente de Hogares* (EPH)

The EPH periodically renews its sample following a rotational scheme that allows for tracking each household over a period of a year and a half. The scheme has been called 2-2-2, since it operates in the following way: (i) households in a given area enter the sample to be surveyed in two consecutive quarters; (ii) they retire for the following two consecutive quarters; (iii) they return to the sample to be surveyed in the following two consecutive quarters. This scheme guarantees that a household that is surveyed for the first time in quarter *t* is again surveyed in *t+1*, it stays out of the sample in *t+2* and *t+3* and is interviewed again in *t+4* and *t+5*. This implies that the same household is observed in the same two quarters of two different years, as may be appreciated in Figure B.1.

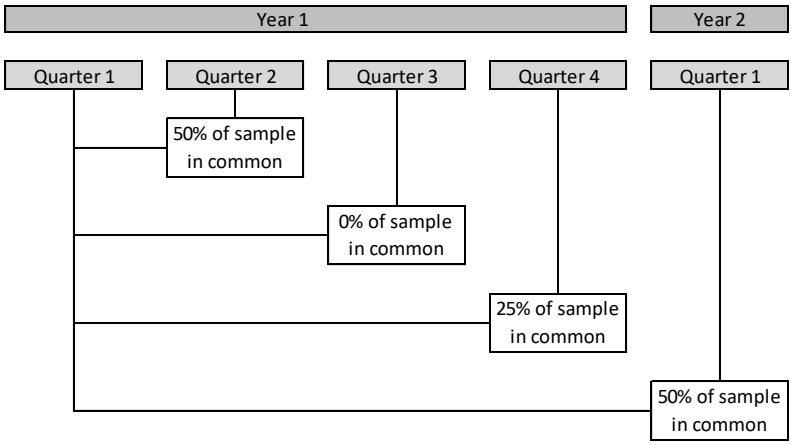
Figure B.1. Rotational Scheme EPH

YEAR 1				YEAR 2				YEAR 3			
Quarter				Quarter				Quarter			
1	2	3	4	1	2	3	4	1	2	3	4
A_2			A_3	A_4		H_1	H_2			H_3	H_4
B_1	B_2			B_3	B_4		I_1	I_2			I_3
	C_1	C_2			C_3	C_4		J_1	J_2		
N_4		D_1	D_2			D_3	D_4		K_1	K_2	
O_3	O_4		E_2	E_2			E_3	E_4		L_1	L_2
	P_3	P_4		F_1	F_2			F_3	F_4		M_1
		Q_2	Q_4		G_1	G_2			G_3	G_4	

Source: own elaboration based on INDEC (2003).
Note: each letter denotes a group of households that entered the sample in the same year and quarter. The number in the subscript indicates the interview round for that particular group. For instance, group B is interviewed for the first time in Year 1, Quarter 1 and it is also sampled in the following quarter. In Quarters 3 and 4 of Year 1 group B remains out of the sample. In Year 2, Quarters 1 and 2 the household is interviewed again.

The overlap in the continuous survey is as follows: (i) two consecutive quarters share 50 percent of sample; (ii) the same quarter in two consecutive years share 50 percent of sample; (iii) between one quarter and another, separated by an intermediate quarter, there is no common sample; (iv) between one quarter and another, separated by two intermediate quarters, there is a 25 percent of sample in common. This scheme is presented in Figure B.2. Given that the analysis of the intra-year dropout rates is based on Quarters 1 and 4 -as in (iv)- , 25 percent of the original sample is used.

Figure B.2. Sample overlap in the EPH rotational scheme



Source: INDEC (2003)