ASOCIACION ARGENTINA
DE ECONOMIA POLITICA

ANALES | ASOCIACION ARGENTINA DE ECONOMIA POLITICA

LI Reunión Anual

Noviembre de 2016

ISSN 1852-0022
ISBN 978-987-28590-4-6

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Compulsory education laws or incentives from CCT programs? Explaining the rise in secondary school attendance rate in Argentina

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Abstract

The last decade shows a sizeable increase in school attendance rates for children aged 15 through 17 in Argentina. This could be related to the 2006 National Education Law that made upper-secondary education compulsory. In this paper, instead, we claim that the *Asignación Universal por Hijo* may be mostly responsible for this improvement. Using a difference-in-difference strategy we estimate that the program accounts for a 3.9 percentage point increase in the probability of attending secondary school among eligible children aged 15 through 17.

Resumen

La última década muestra una mejora considerable de las tasas de asistencia escolar entre jóvenes de 15 a 17 años en Argentina. Esto podría estar relacionado con la Ley de Educación Nacional de 2006 que transformó en obligatoria la educación secundaria superior. En este trabajo, en cambio, afirmamos que la Asignación universal por Hijo puede ser la principal responsable de esta mejora. Utilizando una estrategia de diferencias en diferencias estimamos que el programa generó un aumento de 3,9 puntos porcentuales en la probabilidad de asistir a la escuela secundaria de los jóvenes elegibles entre 15 y 17 años de edad.

JEL Code: I2, I3

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

* We are grateful to Leonardo Gasparini, Walter Sosa Escudero, Paula Razquin, Joaquín Coleff, Emamnuel Vázquez, Leopoldo Tornarolli and seminar participants at Universidad de San Andrés and Universidad Nacional de La Plata for valuable comments and suggestions. Any errors are our responsibility. Corresponding author María Edo: mariaedo@gmail.com

1. Introduction

Argentina has traditionally stood out within Latin America in terms of education. Since the very creation of the National Education System in 1884, primary education has been mandatory in Argentina. This and the free public provision of educational services have allowed to reach almost perfect rates of primary school attendance, which have remained relatively stable above 97% since the 1980s and are comparable and even higher than those of developed countries (Marchionni and Alejo, 2015).

In contrast, secondary education has not always been mandatory in Argentina. By the early 1990s, only the seven years of primary education were compulsory. In 1993, the Federal Education Law 24,195 (*Ley Federal de Educación*) increased compulsory education from 7 to 10 years, thus including the first stage of secondary education. The National Education Law 26,206 (*Ley Nacional de Educación*) passed in December 2006 extended compulsory education by three more years, making mandatory also the upper-secondary education level.

Secondary education indicators improved markedly since the mid-1990s, and some argue that these improvements are a consequence of the successive expansions of compulsory education (DiNIECE, 2011). For the case of the 1993 Federal Education Law, Alzúa *et al.* (2015) find a positive effect on school enrollment and attainment, but the mechanisms remain unclear since the 1993 reform combined an expansion in compulsory education with deep institutional and curricular modifications, among other changes.

Over the last decade, the net school attendance rate for the group aged 15 to 17 – the upper-school age range – rose by almost 4 percentage points, from 82.9% in 2004 to 86.6% in 2014.[1] Our first hypothesis is that this improvement was not caused by the 2006 law. First, neither the law nor accompanying policies had enforcement mechanisms embedded in their design. Therefore, it is unclear through which channels the law may have affected school attendance. Second, three years after the law was passed, attendance rates for the group aged 15 to17 remained virtually unchanged. Only since 2010 school attendance for individuals in this age group started to show clear signs of growth.

But if the 2006 National Education Law showed no impact on net attendance of those aged 15-17, what is driving the increase in those rates as of 2010? What is bringing children aged 15 to 17 – especially those most poor – to stay in school? In this paper we claim that the *Asignación Universal por Hijo* (Universal Child Allowance, AUH) a program implemented in Argentina in late 2009 may be driving this increase in attendance rates.

The AUH is a massive conditional cash transfer program (CCT) targeted at children under 18 years old living in poor families with no registered workers in the formal employment sector. The program currently benefits 29% of all children and approximately 15% of total households in the country (ANSES, 2014). As any typical CCT program, the reception of the transfer is conditional on complying with children's health requirements and school attendance at all compulsory levels. The high economic incentives introduced by the AUH and their conditionalities may probably reduce the probability of dropping out of secondary school compared to the counterfactual situation in absence of the program.

Estimating the causal effect of the AUH on school attendance, however, represents a rough task. The AUH was not assigned randomly nor was it accompanied by a publicly available comprehensive dataset that allows for assessing the program. We thus resort to the Permanent National Household Survey (*Encuesta Permanente de Hogares,* EPH*)*

[1] Net school attendance rate is the percentage of children in a given age group that attend the educational level that officially corresponds to that age (UNESCO).

carried out in Argentina. We classify children in upper-secondary age-range (15 to 17) as potential beneficiaries according to whether their parents comply with the program's eligibility requirements. We compare the probability of secondary school attendance of both groups (eligible and not eligible) over time following a difference-in-difference approach.

Our estimates suggest that the AUH increased the probability of attending secondary school among eligible children aged 15-17 by 3.9 percentage points. The impact seems to be led by boys and is more relevant for children living in larger families where the head of household has lower education levels. The effect on younger children is statistically significant yet very small: 0.4 percentage points for those in primary school age range (6 through 11) and 0.8 percentage points for those in lower secondary (12 through 14 years old). The results hold across different specifications and robustness analysis.

This paper intends to make contributions in several realms. First of all, it adds to the literature on the impact of CCT programs on educational outcomes. Secondly, it provides evidence of the effects of the *Asignación Universal por Hijo*, thus generating input for future improvements of the program. Finally, this work also seeks to highlight the fact that compulsory education laws by themselves are not enough to affect schooling.

The rest of the paper is organized as follows. Section 2 expands on compulsory education legislation in Argentina while presenting evidence on the evolution of net attendance rates over the last decade. Section 3 describes the AUH and discusses the channels that may affect schooling decisions. Section 4 presents the data and methodology. Section 5 and 6 discuss the main results while Section 7 concludes and points to further research.

2. Compulsory education laws and school attendance in Argentina

Compulsory education laws are motivated by the potential social benefits and positive externalities coming from an expansion of the overall education attainment which promotes economic development (Oreopoulos, 2006a). These laws may affect attendance rates through different channels. In the first place, the human capital model of school choice perceives education as an investment (Becker, 1975) and hence depends on intertemporal benefits and costs of schooling. Consequently, compulsory education may prevent a probably optimal decision of leaving school. However, compulsory attendance laws may rise lifetime welfare if they generate positive externalities or under the presence of suboptimal school attainment (Oreopoulos, 2006a; Eckstein and Zilcha, 1994), which is likely among the more vulnerable children in developing countries like Argentina. Secondly, these legislations may trigger implicit enforcement mechanisms, by imposing social stigma to those who fail to comply with the rule. Fulfillment of mandatory schooling may also affect future opportunities in the labor market if, for instance, legal educational requirements are set as a condition to enter the formal employment sector (Alzúa *et al*., 2015). Finally, other public policies accompanying the launch of these legislations may have an impact on attendance rates by affecting the direct costs of education (abolition of tuition fees), the quality of education (increase in educational budget, drastic changes in the curricula) or the availability of nearby educational facilities (large-scale infrastructure programs), among others.

Unfortunately, evidence of the impact on attendance rates of changes in compulsory education laws is relatively scarce. Most studies concentrate on the effects regarding labor market outcomes (Angrist and Kruger, 1991; Acemoglu and Angrist, 2000; Oreopoulos, 2006a and 2006b). Even though some studies document the improvement of attendance rates following mandatory education laws (Goldin and Katz, 2008; Lleras Muney, 2002;

Oreopoulos, 2006a), the mechanism through which the effect operates is not entirely clear. Compulsory education laws are usually launched together with other policies aiming at increasing school attendance. Therefore, some or all of the abovementioned channels operate at the same time, hindering the possibility of isolating the impact of the expansion of compulsory education by itself.

Regarding Argentina, while primary education has always been mandatory, it was only in the early 1990s that compulsory schooling expanded to secondary education. The Federal Education Law, passed in 1993, increased mandatory education from 7 to 10 years of schooling, thus including the first stage of secondary education (children up to 14 years old). Later, in 2006, the National Education Law added three more years of compulsory education, covering also the upper-secondary level (youths between 15 and 17 years old).[2] Table 1 summarizes the timing and scope of these reforms.

Table 1. Extension of compulsory education in Argentina

Age	Common Education Law Year: 1884	Federal Education Law Year: 1993	National Education Law Year: 2006	Modification to National Education Law Year: 2015
4				
5				
6				
7				
8				
9				
10				
11				
12				
13				
14				
15				
16				
17				
Compulsory Years	7	10	13	14

Indicates compulsory age

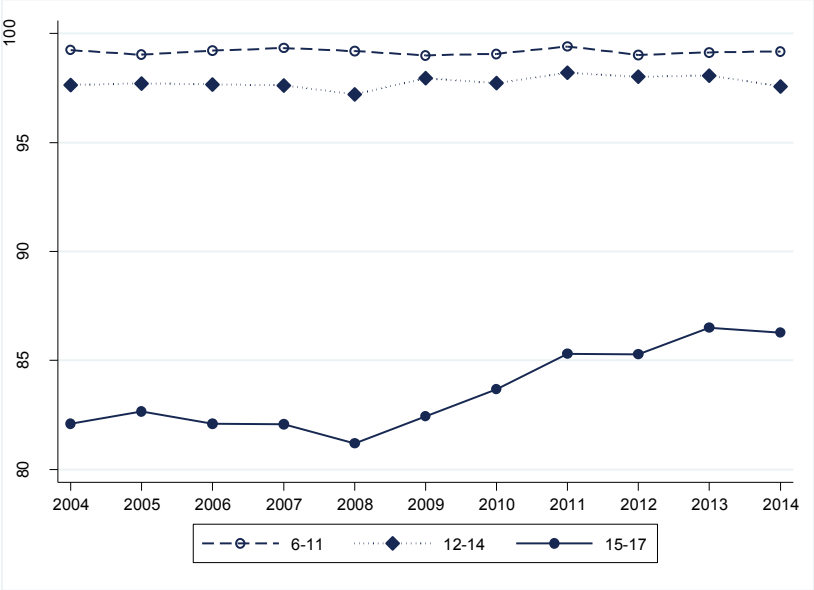
Sources: Common Education Law (1884), Federal Education Law (1993), National Education Law (2006).

[2] Only four other Latin American countries have passed equivalent legislation (i.e. mandatory schooling for both primary and secondary education): Uruguay in 2008, Chile and Brazil in 2009 and Mexico in 2013 (Ruiz and Schoo, 2014).

Some argue that these successive expansions in mandatory schooling are responsible for the observed improvements in secondary education indicators since the early 1990s in Argentina (DiNIECE, 2011). However, the evidence is not so clear. Alzúa *et al*. (2015) evaluate the impact of the 1993 law by taking advantage of the different timing in the implementation of the reform. They find that the 1993 law was followed by a notable increase in gross enrollment rates and had a positive impact on years of schooling for children aged 13-14. However, as stated by the authors, the main mechanism driving the effect is hard to identify since the new legislation was accompanied by changes in the curricula and a strong expansion of the education budget to finance investment in school infrastructure as well as teacher's training.

Figure 1 shows that by 2004, net attendance rates for children aged 6 to 11 (primary school age) and 12 to 14 (lower-secondary school age) were above 97% and remained rather stable over the following decade. Compared to these younger children, those aged 15 to 17 exhibit markedly lower attendance rates (82% in 2004). Even though for this latter group education became compulsory in 2006, net attendance rates remained mostly unchanged over the following three years.[3] Only after 2009 net attendance rates started to significantly grow for 15-17 year-olds, from 82.9% in 2009 to 86.6% in 2014, i.e. an almost 4-percentage-point increase.[4]

Figure 1. Net school attendance rates by age group



Source: own estimations based on EPH.
Note: Net school attendance rate is the percentage of children in a given age group that attend the educational level that officially corresponds to that age (UNESCO). Ages 6-11 correspond to primary school; ages 12-14 and 15-17 correspond to lower and upper secondary school, respectively.

The preliminary evidence in Figure 1 suggests that the 2006 National Education Law had no impact on net attendance rates on the first three years after its implementation, which is

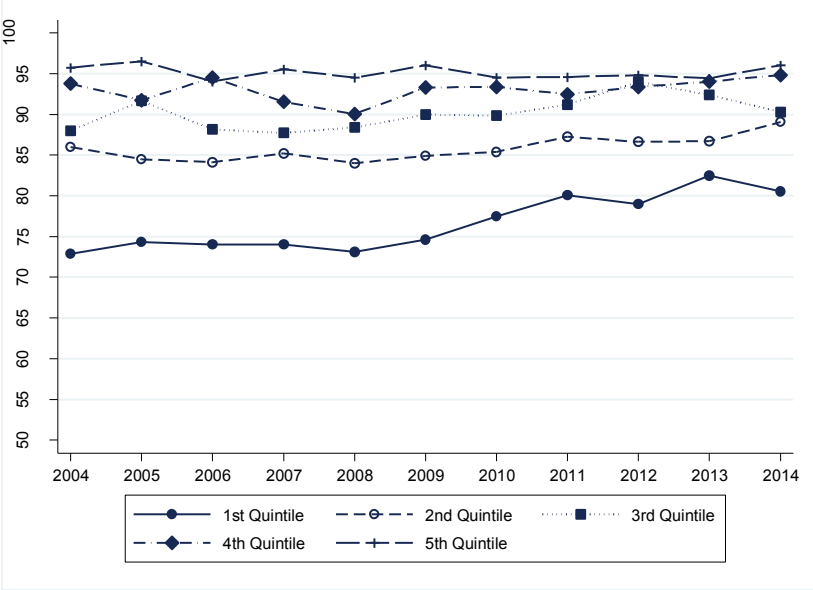
[3] In fact, attendance rates for the group of 15-17 year-olds follow a similar pattern to the 12-14 year-old group over the 2004-2009 period, even though the latter group was not affected by the law. This is confirmed by a difference-in-difference estimation. These results are available upon request.

[4] Administrative data shows a very similar pattern for secondary school enrollment (DiNIECE: http://portales.educacion.gov.ar/diniece/). However, administrative data is not available by age group.

not surprising given that there were no companion measures that could have encouraged school attendance. In fact, even though there was a large expansion of the educational budget, new funds were almost entirely absorbed by salaries, with no investment in training or systematic infrastructure development, and only quite limited changes in the curricula.[5] Moreover, despite some specific programs were developed to complement the new education law they were more focused on establishing an adequate normative framework and on improving institutional arrangements than in providing direct or indirect incentives to school attendance (UNICEF, 2012). [6]

But if the 2006 National Education Law had no impact on attendance rates for those aged 15-17 three years after its implementation, what is driving the increase as of 2010 shown in Figure 1? In this paper we claim that the *Asignación Universal por Hijo* (Universal Child Allowance, AUH) program implemented in Argentina in late 2009 is responsible for encouraging children aged 15 through 17, especially poor children, to stay at (or return to) school. In fact, Figure 2 shows that the improvement of upper-secondary net attendance rates after 2009 was driven by the most vulnerable children, i.e. the target group of the AUH. Net attendance rates for youths aged 15 to 17 in the first quintile of the income distribution increased 8 percentage points in the last decade: almost 3 percentage points between 2004 and 2009 (from 72.8% to 74.6%) but more than 5 percentage points between 2009 and 2014 (from 75% to 80.5%). Net attendance rates for those in the top quintiles have remained mostly unchanged over the last decade.

Figure 2. Net attendance rates for 15-17 year olds by income quintile.



Source: own estimations based on EPH.
Note: quintiles of the distribution of per capita family income.

[5] The 2005 Education Funding Law 24,075 (*Ley de Financiamiento Educativo*) introduced a gradual expansion of the educational budget, with the aim of reaching 6 percent of GDP by 2010. This implied an increase in per-student expenditure in Argentina, but the country lacked improvements in terms of the efficiency of this investment, in particular the pedagogical and organizational transformations to facilitate the improvement of education results (Auguste, 2012).

[6] For instance, the *Plan Nacional de Educación Secundaria Obligatoria* (2009-2011 and 2012-2016).

3. The AUH program and the incentives to school attendance

The AUH was launched in November 2009 and represents a massive conditional cash transfer (CCT) program that focuses on children under 18 years old living in poor and informal households. It was designed to extend the social protection network in Argentina, which used to be tied to the formal employment sector, to the more vulnerable groups of the population. The magnitude of the benefit as well as the expansion in the number of beneficiaries have no precedents in the Argentinian social policy, formerly characterized by small scale and targeted programs.

The AUH awards a monetary transfer to households with children where neither parent is registered in the formal sector. This includes not active, unemployed or informal workers earning less than the minimum wage.[7] Each beneficiary household can perceive a transfer per child under 18 years old up to a maximum of five dependent children.[8] Currently, more than 3.5 million children and youths benefit from this program, representing almost 29% of all individuals under 18 years old and approximately 15% of total households in the country (ANSES, 2014). Regarding its budget, the AUH is one of the largest CCT programs in Latin America, with resources representing almost 0.8% of the country's GDP (Stampini and Tornarolli, 2013).

CCT programs may impact on school enrollment and attendance by relaxing family's budget constraints but also through the conditionalities they impose. As education may be regarded as a normal good its consumption could increase with household income. The conditionalities set an additional incentive to bias this increase in consumption towards investment in education. In particular, the AUH imposes sanitary and educational conditionalities in terms of periodical health controls and vaccination for children under 5 and pregnant women, and school attendance at all compulsory levels from ages 5 through 18. For this purpose, the program sets a particular payment mechanism: 80% of the subsidy is automatically received by beneficiary families on a monthly basis, and the remaining 20% is paid annually, once compliance with the conditionalities is proven.[9] If the conditions are not met, not only the 20% is not perceived but also the beneficiary is suspended from future participation in the program.

The amount of the AUH transfer has been modified several times to cope with inflation.[10] As of June 2014, the monthly transfer for each child -i.e. 80% of the total transfer- was ARS 515 which represented almost 15% of the minimum legal wage in Argentina. For a typical poor family with three children, this implied an almost 30% increase of total monthly family income. The 20% remaining amounted to ARS 1,400 per child per year, *i.e.* 62% of total family income for the same typical family and almost 100% of the

[7] It is important to note, however, that monitoring this condition is not feasible in practice. This implies that informal workers earning more than the minimum wage could become beneficiaries. Nevertheless, as shown later on, both quantitative and qualitative evidence suggest that these situations are scarce, probably due to social responsibility or stigma.

[8] Transfers for disabled children have no age limit.

[9] Concerning the condition on school attendance, the program originally required that the child must be enrolled in a public school. This clause, however, was never made effective given the large public opposition that claimed for a considerable fraction of vulnerable children who attends publicly subsidized private schools. In fact, 16% of all primary school students belonging to the first two quintiles of the equivalized income distribution were attending a private school in 2010. The corresponding figure for secondary school students is 14% (SEDLAC, 2015).

[10] The nominal monthly benefit per child, initially set at ARS $180, has increased on average more than 20% per year and hence its real value has remained relatively constant since 2009 (Garganta *et al.*, 2014).

minimum legal wage. Besides, noncompliance with the conditionalities leads to suspension from the program, implying the loss of the future transfers until the child turns 18. Since the AUH was launched as a permanent program with a wide support of all political parties, the transfers should be perceived as permanent income and the expected present value of the transfers should be large, thus reinforcing the commitment of beneficiaries with conditionalities.

According to the literature on the impact of CCT programs, the effects on 'access to school' indicators such as enrollment and attendance are usually positive (Fiszbein *et al.*, 2009; Cecchini, 2014), even though the size of the effect varies with other factors: it is larger for groups with low attendance rates, among the most vulnerable families and in programs with more generous transfers (Saavedra and García, 2012). Besides these general findings, some particular results are worth noticing. Typically, the size of the effect is larger in the secondary school level than in the primary level. For instance, both the *Oportunidades* program in Mexico (formerly known as *PROGRESA*) and *Familias en Acción* in Colombia significantly contributed to increase attendance rates, especially among secondary school children (Attanasio *et al.*, 2008; Schulz, 2004; De Brauw and Hoddinott, 2008). Also, even when focusing on secondary education, the size of the effect exhibits considerable variation: from a 2-percentage-point increase in the case of *Ingreso Ciudadano* in Uruguay to a 12-percentage-point increase in the case of *Oportunidades* in Mexico and *Bolsa Escola* in Brazil (Saavedra and García, 2012).[11] In summary, even though the impact differs across programs and population groups, in general CCT programs improve the so-called 'intermediate objectives': better access to school, higher enrollment rates and higher attendance (Cecchini, 2014; Bastagli, 2008).

Given this evidence and the importance of the AUH – both in terms of coverage and generosity of the benefits – it is likely that it contributed to the improvement of attendance rates documented in section 2, which took place precisely after the program's inception in late 2009. Evidence of the impact of the AUH on education results is still scarce. Among a large set of wellbeing indicators, Paz and Golovanevsky (2014) find large and positive effects in attendance rates – around 7 percentage points – of the AUH for eligible children aged 13-17 when comparing the years 2009 and 2010 through a difference-in-difference methodology. In a recent working paper based on aggregate data from administrative sources, Cigliutti *et al.* (2015) find that secondary gross enrollment rates in Argentina rose by 2.25 percentage points due to the AUH compared to a synthetic control that consists of a linear combination of other Latin American countries.[12] The present work provides new evidence regarding the impact of the AUH on eligible's secondary school attendance. By using micro-data and following a difference-in-difference approach we extend the period of analysis to cover 6 years before and 5 years after the AUH implementation. Furthermore, we zoom into the group aged 15-17 which allows for relating our findings to the extension of mandatory schooling while deepening the analysis of the nature of the effect by exploring heterogeneities across different sub-groups.

[11] Additional evidence from the Mexican *PROGRESA/Oportunidades*, the oldest and most studied program in Latin America, shows also a significant reduction in drop-outs (SEDESOL, 2008), a fall of the gender gap in secondary enrollment (Parker, 2003) and an increase in indigenous children attendance (Escobar and De la Rocha, 2008).

[12] D'Elia *et al.* (2014) provide evidence of the AUH impact on education quality indicators.

4. Data and empirical strategy

The AUH was neither randomly assigned nor accompanied by a publicly available comprehensive dataset that may allow for follow-ups of the beneficiary population. The absence of these features greatly determines both the data and the empirical strategy for assessing the program's impact on any outcome.

We use microdata from the Permanent National Household Survey (EPH) carried out by the Argentinian national statistical office (INDEC). The EPH gathers data on demographic, education, income and employment issues and covers 31 large urban conglomerates, representing 62% of the total population of the country. We focus on the 2004-2014 decade. The pre-intervention period (before) includes years 2004 through 2009 – the AUH was launched in November 2009 – while the post-intervention period (after) covers years 2010 through 2014.

Our sample includes children aged 15-17, *i.e*. in the upper secondary age range. Since the EPH does not include information to identify AUH beneficiaries, we aim at determining if the child is a potential beneficiary of the program by checking whether he/she meets the AUH eligibility criteria – intention to treat. Particularly, we define the 'treatment' and 'control' groups based on children's eligibility according to their parents' labor status. A child is classified as belonging to the treatment group whenever his/her parents are either not active, unemployed, informal or self-employed workers. Because of a special regulation, 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.[13] As for the control group, it includes all children aged 15-17 for whom at least one of their parents is employed in the formal sector.

As an additional requirement for eligibility, the AUH imposes that earnings are below the minimum legal wage. Even though this condition is not verifiable for informal workers, 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 small.[14] Therefore, we further restrict the sample to only include children from poor households, defined as those in the first four deciles of the per capita income distribution.[15]

In order to estimate the intention-to-treat impact of the AUH on secondary school attendance of eligible children we follow a difference-in-difference methodology by comparing the differences in the probability of secondary school attendance of the treatment and control groups, before and after the inception of the program. The identification assumptions are that secondary attendance rates of treatment and control groups would have evolved similarly in the absence of the program and that there was no other contemporaneous event to the implementation of the AUH that could have caused differences in the evolution of school attendance between the treatment and control groups. The latter does not appear to be a strong assumption considering no major initiatives affecting educational outcomes took place in 2009 (infrastructure expansion,

[13] Special Social Security Scheme for Domestic Service Employees (Law 25,239, Title XVIII).

[14] From the experience of public officials in charge of the registration to the AUH, non-poor individuals –yet not belonging to the formal sector- tend to opt out of the program either by not even starting the procedure or by not complaining when they are suspended from the benefit following audits (Pautassi *et al.*, 2013). Evidence from the last National Consumption Survey (ENGHo 2012) points in the same direction: very few children belonging to the upper income deciles – less than 2% in the two top deciles – receive benefits from the AUH (Gasparini and Cruces, 2015).

[15] Results are robust to other income measures as well as other cut-offs.

teacher's training, school meals, etc.). Regarding the first assumption, it cannot be proven but we provide evidence in its favor in the next section.

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

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

The output variable *Attends* is a binary indicator that takes the value 1 for children attending secondary school and 0 otherwise[16]; $Treat$ is an indicator variable for the treatment group; *After* tags years after the AUH implementation (2010-2014), and X includes a set of child and household level controls (child's gender, 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 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.[17] If the unobserved characteristics that remain after adding all these controls do not have a differential impact on attendance between both groups before and after the implementation of the AUH, we may claim that the γ parameter represents the causal effect of the program (Angrist and Pischke, 2009).

5. Results

Table 2 shows average net attendance rates for treatment and control groups before and after the inception of the AUH. Even though attendance rose for both groups, the increase was considerably larger among eligible children: 5.1 percentage points as compared to 1.9 for the control group. This preliminary unconditional evidence suggests that the AUH may have had the effect of rising secondary school attendance of eligible children aged 15-17 by 3.2 percentage points.

Table 2. Net Secondary School Attendance Rates

Children between 15 and 17 years old

	Treatment (i)	Control (ii)	(i)-(ii)
Before AUH	75.1	87.0	-11.9
After AUH	80.2	88.9	-8.7
Difference (After-Before)	***5.1***	***1.9***	***3.2***

Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: *Treatment Group* includes children whose parents are either not active, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children aged 15-17 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.

[16] Unfortunately, even though the EPH includes information on the education level being attended, it does not inform the specific school-year.

[17] We use data for the first semester of each year by combining EPH's samples from the first two quarters and control for quarter fixed effects.

It is worth noting, however, that given the very nature of the program – non-random assignment –, treatment and control groups differ by construction. Table 3 shows that even though the two groups share on average some features (gender, age, household's size), potential AUH beneficiaries belong to poorer households and exhibit a larger proportion of single-parent and female headed households where the head of household has lower educational attainment and is more likely to be unemployed, both in pre and post-intervention periods.

Table 3. Descriptive statistics
Children between 15 and 17 years old

	Variables	Before				After			
		Treatment Group	Control Group	Difference	P-Value	Treatment Group	Control Group	Difference	P-Value
Child	**Male**	51.5	51.0	0.5	0.6	50.1	51.4	-1.3	0.9
	Age	15.9	15.9	0.0	0.5	16.0	15.9	0.1	0.0
Head of HH	**Single-Parent**	34.7	14.4	20.3	0.0	36.9	16.6	20.3	0.0
	Female	36.6	18.2	18.4	0.0	42.0	22.7	19.3	0.0
	Age	46.4	45.4	1.0	0.0	46.1	45.4	0.7	0.0
	Years of Education	7.9	9.0	-1.1	0.0	8.4	9.5	-1.1	0.0
	Employed	73.5	89.9	-16.4	0.0	71.3	89.4	-18.1	0.0
HH	**Household Size**	5.8	5.8	0.0	0.4	5.7	5.7	0.0	0.1
	Number of Children	3.2	3.1	0.1	0.0	3.1	3.0	0.1	0.0
	Per Capita Income	184.3	285.4	-101.1	0.0	741.7	1012.5	-270.8	0.0
	Observations	***12,466***	***6,363***			***10,002***	***6,171***		

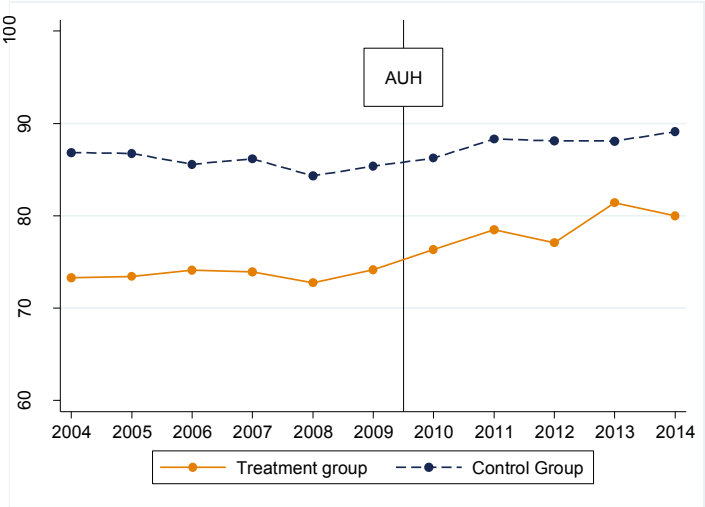
*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: *Treatment Group* includes children whose parents are either not active, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes all children aged 15-17 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.

In fact, as Figure 3 shows, treatment and control groups differ in their school attendance rates prior to the program, which is in part due to those differences in characteristics. Nevertheless, albeit attendance rates *levels* differ before the inception of the AUH, the *time patterns* are similar. This is confirmed by a pre-program common trends test: we do not find enough evidence to reject the null hypothesis that the pre-treatment trends were equal, thus reinforcing the confidence in our identification assumption.[18] However, since

[18] We run a model of our outcome of interest (attendance) 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 (*Ho*) states that all the coefficients for the interaction terms are jointly equal to zero. We find no evidence to reject the null: *Ho*: *F*(5, 18,817)=0.47, Prob>*F*=0.80. We then run a new model

2010 just after the AUH implementation, the school attendance gap between groups started to shrink because the attendance rate of eligible children grew faster than that of the control group.

Figure 3. Net attendance rates for 15-17 year olds. Treatment and control groups.



Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: *Treatment Group* includes children whose parents are either not active, unemployed, informal or self-employed workers (or are registered employees working in the domestic service). *Control Group* includes children for whom at least one of their parents is employed in the formal sector. Children in both groups are aged 15 through 17 and belong to the first four deciles of the per capita family income distribution.

We now assess whether this result holds in a multivariate difference-in-difference framework and is robust to several types of controls. Table 4 shows the results of estimating the linear model of school attendance in equation 1. Models 1, 2 and 3 in the table progressively control for child´s and head of household's characteristics (child's gender, age and squared age; head of household's gender, age, squared age, educational level, employment status), other household features (household size, per capita income, single-parent household, female headed household, number of children under 18), region and time fixed effects (year and quarter), as well as regional trends. The coefficient of the interaction term is positive and statistically significant across specifications, suggesting a positive impact of the AUH on school attendance of eligible children aged 15-17 of almost 4 percentage points (3.9 p.p.).

The size of the effect is certainly non-trivial. According to our estimates, the 3.9 percentage-point impact in secondary school net attendance implies that the AUH helped around 20,000 eligible children aged 15-17 to stay at secondary school over the period 2010-2014. In terms of education gaps, it represents a 20% closure of the net attendance rate gap between the treatment group and those belonging to the richest quintile. Moreover, compared to other Latin American CCT programs, the impact we find for the AUH is between the 2-percentage-point effects of the Brazilian *Bolsa Escola* and the

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that includes both pre and post-program years. The null hypothesis is now easily rejected: *Ho*: *F*(10, 34,980)=2.19, Prob>*F*=0.015.

Uruguayan *Ingreso Ciudadano*, and the 12-percentage-point effects of *Familias en Acción* in Colombia and *Oportunidades* in Mexico (Saavedra and García, 2012). [19]

Table 4. Probability of attending secondary school
Children between 15 and 17 years old

	(1)	(2)	(3)
Treatment*After	***0.0320****	***0.0392****	***0.0388****
	(0.00817)	***(0.00890)***	***(0.00885)***
Treatment	*-0.119****	*-0.0771****	*-0.0757****
	(0.00728)	*(0.00623)*	*(0.00622)*
After	*0.0195****	*0.000711*	*0.0309*
	(0.00655)	*(0.00700)*	*(0.0433)*
Child and HH head's characteristics	Yes	Yes	Yes
Other HH Characteristics	No	Yes	Yes
Regional and Time Dummies, Regional Trends	No	No	Yes
Observations	35,002	35,002	35,002

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: OLS estimations. Dependent binary variable: *Attends*, equals 1 if the child is 15-17 years old and attends secondary 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.

Placebo experiments

We perform a series of false experiments or placebo exercises to gain more confidence in the validity of the identification assumption. In this regard, we run the same linear model using only pre-treatment observations and pretending that the program took place in any year previous to 2009 – the actual implementation date of the AUH. Table 5 shows the results for five alternative fake dates: 2004, 2005, 2006, 2007 and 2008. In all cases the coefficient accompanying the interaction term is small and not statistically significant. Only after 2009 some event shifted the attendance rates for the treatment group, but clearly not before.

[19] Some additional clarifications must be made in order to assess a fair comparison. Firstly, the average baseline of secondary school attendance in most Latin American countries was considerably lower than that of Argentina. Furthermore, we focus on upper-secondary attendance rates while the evidence presented above corresponds to the whole secondary school level.

Table 5. Probability of attending secondary school

Placebo regressions

	Intervention in				
	2004	**2005**	**2006**	**2007**	**2008**
Treatment*After	***0.0192***	***0.0252***	***0.0179***	***0.0157***	***0.0155***
	(0.0184)	***(0.0167)***	***(0.0140)***	***(0.0128)***	***(0.0151)***
Treatment	*-0.0856****	*-0.0872****	*-0.0796****	*-0.0751****	*-0.0719****
	(0.0166)	*(0.0147)*	*(0.0105)*	*(0.00831)*	*(0.00753)*
After	*-0.0525***	*0.0437*	*0.0489*	*0.0507*	*0.0759**
	(0.0249)	*(0.0392)*	*(0.0393)*	*(0.0386)*	*(0.0421)*
Child and HH head's characteristics	Yes	Yes	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes	Yes	Yes
Observations	18,829	18,829	18,829	18,829	18,829

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*

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

Note: OLS estimations. Dependent binary variable: *Attends*, equals 1 if the child attends upper secondary level; *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 4.

Alternative definition of the pre-intervention period

As discussed in Section 2, the National Education Law of 2006 extended compulsory schooling for children aged 15-17. Therefore, if this legislation altered schooling incentives differently for the treatment and control groups, then the effects we find cannot be adjudicated solely to the AUH. The results of the placebo experiment with 2006 as the false intervention date – column 3 in Table 5 – represent evidence against this possibility. However, to reinforce the rejection of the incidence of this law we additionally assess the AUH impact on secondary attendance by establishing an alternative shorter pre-intervention period: from 2007 to 2009, rather than 2004 to 2009. Column 1 in Table 6 shows the original results – the same results reported in Table 4, column 3 – while column 2 presents the estimated results when restricting the sample to years 2007-2014 and defining 2007-2009 as the pre-intervention period. Coefficients are quite similar in terms of size and statistical significance, reinforcing the hypothesis that it was the AUH in 2009 what caused the increase in attendance rates of poor children living in informal households.

Table 6. Probability of attending secondary school

Alternative pre-intervention periods

	Pre-intervention period	
	2004-2009	**2007-2009**
Treatment*After	***0.0388****	***0.0328****
	(0.00885)	***(0.00975)***
Treatment	*-0.0757****	*-0.0707****
	(0.00622)	*(0.00831)*
After	*0.0309*	*-0.100****
	(0.0433)	*(0.0358)*
Child and HH head's characteristics	Yes	Yes
Other HH Characteristics	Yes	Yes
Regional and Time Dummies; Regional Trends	Yes	Yes
Observations	35,002	27,035

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*

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

Note: OLS estimations. Dependent binary variable: Attends, equals 1 if the child attends upper secondary level; *Treatment* equals 1 for eligible children and 0 for non-eligible children; *After* is defined ad-hoc in each model (for column 1 it equals 1 in the period 2010-2014 and 0 for the period 2004-2009, for column 2 it equals 1 for the same period but 0 for 2007-2009). For a description of control variables included, refer to Table 4.

Alternative samples

Since the EPH does not include information to identify AUH beneficiaries we relied on children's eligibility based on their parents' labor status. However, some limitations of the survey may lead to classification errors. To start with, we do not have information on one or both parents when they do not live within the household.[20] Furthermore, even if parents live with their child it is not always straightforward to identify this relationship given the fact that the EPH collects information on the family linkage of each household member only in terms of the head of household.[21]

To assess the extent to which these limitations may affect results, we define three alternative nested samples that account for different possible situations: *(i)* a first sample that only contains those children for whom both parents live in the household; *(ii)* an alternative larger sample that includes children for whom least one parent is present; and finally *(iii)* one that also incorporates those children living in households where neither parent is present. Considering our universe is composed by all children aged 15-17

[20] The latter generally includes households where grandparents are in charge of their grandchildren.

[21] For instance, suppose a family is composed by the head of household, two of his daughters, two sons in law and a grandson between 15 and 17 years old. In such a case we would not be able to identify who the father and mother of the child are.

belonging to the first four income deciles, then sample *(i)* represents 64.4% of that target population, sample *(ii)* adds up a considerable fraction of children leading to a total coverage of 94.1%, while sample *(iii)*, by construction, holds the total universe. In all three samples, whenever more than one adult could be identified as the mother or father of the child, the child was only considered eligible if all of the 'potential' parents met the eligibility conditions.[22]

Table 7. Probability of attending secondary school
Alternative samples

	Sample		
	(i)	***(ii)***	***(iii)***
Treatment*After	***0.0358******	***0.0388******	***0.0376******
	(0.00984)	***(0.00885)***	***(0.00865)***
Treatment	*-0.0761****	*-0.0757****	*-0.0792****
	(0.00750)	*(0.00622)*	*(0.00594)*
After	*0.0287*	*0.0309*	*-0.00799*
	(0.0459)	*(0.0433)*	*(0.0134)*
Child and HH head's characteristics	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes
Observations	23,953	35,002	37,207

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: sample *(i)* includes children aged 15-17 for whom both parents live in the household; **sample *(ii)*** includes children aged 15-17 for whom at least one parent is present**; sample *(iii)*** includes all children aged 15-17, irrespective of whether both, one or neither parent in present in the household. *S*ee Table 4 for a description of the variables included.

Table 7 shows that the estimated effects of the program are not altered when using these alternative samples, neither in magnitude nor in terms of statistical significance. Given the robustness of the main result to different samples, we choose to conduct the analysis on the basis of sample *(ii).* Indeed, all the results shown previously relied on this last group of children. The choice is grounded on conceptual reasons. On the one hand, it extends sample *(i)* by including many single-parent households, mostly female headed households, where poverty rates are usually higher and are thus possibly more prone to belong to the treatment group. On the other hand, sample *(ii)* excludes those children for whom we have no information on neither of their parents working conditions – sample *(iii)*. The chosen sample, of course, suffers from the risk of including in the treatment group children that should belong to the control group: when the parent living with the children meets the

[22] In the example set in the previous note, this would imply that both daughters and both sons-in-law should meet the requirements. These cases, however, only represented 0.8% of sample *(i)* and 1.8% of sample *(ii)*.

program's eligibility conditions but the parent not living within the household does not. Nevertheless, even making very pessimistic assumptions, we estimate that only 9% of sample *(ii)* could be wrongly classified in the treatment group.[23]

6. Heterogeneous effects

Our estimates show that the AUH increased net secondary attendance rates for those eligible children aged 15 to 17 years old by almost 4 percentage points, but heterogeneities may be hidden behind this average effect. In this section we explore whether the impact of AUH on attendance rates varies across groups. Firstly, we look for heterogeneous effects by age and gender of children. Secondly, we assess whether the impact is related to household characteristics: number of children and education level of the head of household.

Heterogeneities by age

Table 8 shows that the effect varies considerably across age groups. Compared to the almost 4-percentage- point increase for the group aged 15-17, the effect is only 0.8 for the 12-14 age group – lower secondary. For children aged 6-11 – primary school age – the effect is even smaller but still significant (0.4 percentage points) while for the youths between 18 and 20 years old the estimated effect of the AUH is not statistically significant.

The latter result is consistent with the fact that individuals older than 18 years old are not eligible for the program, so no effect of the AUH is expected in terms of their schooling. Regarding the age groups covered by the program (6 to 11, 12 to 14 and 15 to 17), the results are consistent with the existing international evidence on the impact of CCT programs on schooling: the effect of the AUH is larger for higher levels of education, where baseline attendance rates are lower (Saavedra and García, 2012; Fiszbein *et al.*, 2009). Indeed, even though the explicit cost of attending school may be similar at all educational levels, the opportunity costs certainly increase with age: older children may work in the labor market or allow for other adults in the household to do so by taking care of younger siblings or performing other household chores.[24] Therefore, it is plausible that the economic incentives introduced by the AUH may have lower or even insignificant effects for younger school-aged children whose educational decisions are less sensitive to economic changes, thus explaining the larger impact for the oldest eligible children.

We also explore whether there are age heterogeneities among 15-17 year olds. Table 9 shows the results when zooming into this group. We find an unclear pattern: the impact of the AUH seems to be concentrated in those aged 15 and 17.

[23] This is based on the assumption that all non-present parents live and are recognized as such. Also, we assume that their formality rate is similar to that of parents living with their children – around 36%.

[24] The legal minimum working age in Argentina is 16 years old (*Ley de empleo infantil 26,390*).

Table 8. Probability of attending school

Heterogeneities by age range

	Age Range			
	6-11	**12-14**	**15-17**	**18-20**
Treatment*After	***0.00422******	***0.00809****	***0.0388******	***0.0170***
	(0.00153)	***(0.00315)***	***(0.00885)***	***(0.0151)***
Treatment	*-0.00383****	*-0.0153****	*-0.0757****	*-0.0867****
	(0.00113)	*(0.00256)*	*(0.00622)*	*(0.00941)*
After	*-0.0229**	*-0.00251*	*0.0309*	*-0.109*
	(0.0120)	*(0.0172)*	*(0.0433)*	*(0.0695)*
Child and HH head's characteristics	Yes	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes	Yes
Observations	69,332	34,904	35,002	28,792

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*

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

Note: OLS estimations. Dependent binary variable: *Attends*, equals 1 if the child attends the corresponding 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. For a description of the variables included, see Table 4.

Table 9. Probability of attending secondary school

Heterogeneities by age

	Age		
	15 years old	***16 years old***	***17 years old***
Treatment*After	***0.0425******	***0.0252***	***0.0518******
	(0.0132)	***(0.0165)***	***(0.0162)***
Treatment	*-0.0718****	*-0.0537****	*-0.103****
	(0.00942)	*(0.0100)*	*(0.00944)*
After	*-0.0286*	*-0.0442*	*0.0593*
	(0.0372)	*(0.0490)*	*(0.0506)*
Child and HH head's characteristics	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes
Observations	12,481	11,354	11,167

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: for a description of the variables included, see Table 4.

Heterogeneities by gender

Table 10 shows that the increase in attendance rates was mostly driven by improvements in boys' attendance: the estimated impact for boys is well above 5 percentage points while that of girls is below 2 percentage points and not statistically significant.

Once again, more than one mechanism may explain these results. As stated before, different baseline levels of attendance may be in part responsible. In fact, initial attendance rates were lower for boys: around 70% as compared to 80% for girls among the treatment group. Also, according to the literature, family decisions on girls' schooling seem to be more tied to cultural factors which are less affected – at least in the short term – by changes in household income. For instance, previous evidence for Argentina (Sosa Escudero and Marchionni, 1999) suggests that girls' attendance is rather inelastic as compared to boys'.

Table 10. Probability of attending secondary school
Heterogeneities by gender

	Boys	Girls
Treatment*After	***0.0583****	***0.0165***
	(0.0108)	***(0.0122)***
Treatment	*-0.100****	*-0.0499****
	(0.00813)	*(0.00731)*
After	*0.0263*	*0.00823*
	(0.0393)	*(0.0696)*
Child and HH head's characteristics	Yes	Yes
Other HH Characteristics	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes
Observations	17,822	17,180

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: for a description of the variables included, see Table 4.

Heterogeneities by household characteristics

Table 11 shows the AUH effect on net school attendance among families which differ in the number of children. Despite the fact that the impact is statistically significant for all groups, it increases with the number of children. In particular, the effect for larger households (5 or more children) almost doubles that of families with one or two children. This result is consistent with the fact that more eligible children in the household imply higher benefits and a potentially larger income effect of the AUH. Thus, larger families may show more commitment with the conditionalities of the program.

Table 11. Probability of attending secondary school
Heterogeneities by number of children in the household

	Number of Children		
	1 or 2	***3 or 4***	***5 or more***
Treatment*After	***0.0267****	***0.0326****	***0.0506****
	(0.0130)	***(0.0143)***	***(0.0218)***
Treatment	*-0.0612****	*-0.0686****	*-0.104****
	(0.0106)	*(0.00872)*	*(0.0127)*
After	*-0.0760**	*0.0652*	*0.0535*
	(0.0390)	*(0.0451)*	*(0.0700)*
Child and HH head's characteristics	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes
Observations	13,799	14,301	6,902

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: For a description of the variables included, see Table 4.

Finally, table 12 explores whether the effect varies with household structure –two-parent or single-parent families– and with the education level of the head of household. We find a positive impact of AUH on school attendance for all groups, and the effect appears to be slightly larger among children with low educated head of household.

Table 12. Probability of attending secondary school
Heterogeneities by characteristics of the head of household

	Single-Parent	Two-Parent	Low Education	High Education
Treatment*After	***0.0369****	***0.0398*****	***0.0360*****	***0.0240***
	(0.0157)	***(0.0105)***	***(0.0104)***	***(0.0142)***
Treatment	*-0.0685****	*-0.0765****	*-0.0841****	*-0.0435****
	(0.00970)	*(0.00758)*	*(0.00752)*	*(0.0100)*
After	*-0.00686*	*-0.00582*	*0.121***	*0.0269**
	(0.0300)	*(0.0128)*	*(0.0549)*	*(0.0153)*
Child and HH head's characteristics	Yes	Yes	Yes	Yes
Other HH Characteristics	Yes	Yes	Yes	Yes
Regional and Time Dummies, Regional Trends	Yes	Yes	Yes	Yes
Observations	10,994	24,008	25,505	9,497

*Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.*
Source: own estimations based on *Encuesta Permanente de Hogares*.
Note: OLS estimations. "Low Education" includes household which head has less than secondary school education, "High Education" refers to households where the head completed secondary education. For a description of the variables included, see Table 4.

7. Concluding remarks and further research

Argentina has traditionally stood out in terms of educational outcomes among its Latin American counterparts. Schooling of older children, however, still shows room for improvement especially among the more vulnerable school-age children. Fortunately, during the last years a sizeable improvement in attendance rates for children aged 15 through 17 took place. This could be related to the 2006 National Education Law that made upper-secondary education compulsory. In this paper, instead, we show that the *Asignación Universal por Hijo* (Universal Child Allowance, AUH), a massive conditional cash transfer program implemented in 2009 in Argentina, may be mostly responsible for this improvement.

Using a difference-in-difference strategy based on data from the Argentinian National Permanent Household Survey we estimate that the program accounts for a 3.9 percentage point increase in secondary school attendance among eligible children aged 15 through 17. This effect is robust to different specifications and a large set of checks. Also, we present evidence suggesting that this effect is not related to the expansion of compulsory education that took place in Argentina in 2006. Moreover, the positive impact of the AUH in attendance rates is not homogenous: the effect seems to be driven particularly by boys and is higher for children living in larger households where the head has low educational attainment.

Further research should point in several directions. A first relevant issue would be to unravel which mechanisms within the AUH are responsible for the increase in attendance rates. The effect may be driven by the monthly benefit itself or by the conditionality, or both mechanisms could be operating simultaneously. A deep understanding of these alternative channels is indeed relevant in terms of improving the design of CCT programs. Secondly, it would be interesting to explore if the AUH has not only increased secondary school attendance among eligible children but also affected other educational results, such as intra-annual dropouts or secondary school completion rates. Thirdly, it would also be relevant to disentangle if this increase in attendance rates is matched by a similar result in the employment realm. It could be expected that an increase in attendance rates may contribute to a reduction in labor participation among the 15-17 age group. It could also be the case, however, that those upper-secondary school aged children were not working in the labor market before the AUH, but in charge of household chores such as taking care of their siblings. In that case the AUH may be altering instead other members' labor participation. Although household decision processes are certainly difficult to assess, exploring these hypothesis would shed light on the mechanisms that are at work and thus further refine the AUH's design.

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