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Conditional Cash Transfer Programs and Enforcement of Compulsory Education Laws. The case of Asignación Universal por Hijo in Argentina

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Conditional cash transfer programs and enforcement of compulsory education laws.

The case of *Asignación Universal por Hijo* in Argentina[1]

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Abstract

Argentina has traditionally stood out in terms of educational outcomes among its Latin American counterparts. Schooling among older children, however, still shows room for improvement especially among the more vulnerable. Fortunately, the last decade witnessed a sizeable improvement in attendance rates for children aged 15 through 17. 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 (AUH)* a massive conditional cash transfer program implemented in 2009 in Argentina may be partly responsible for this improvement. Using a difference in difference model we estimate that the program accounts for a 3.9 percentage point increase in attendance rates among those eligible children aged 15 through 17.

JEL Code: J13, H52, I21,

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

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1. Introduction

Argentina has traditionally stood out within Latin America in terms of its educational standards. In particular, enrollment and attendance rates are the largest among the region. While primary education rates are almost perfect secondary education rates are far from being ideal (85% according to SITEAL) but still represent the highest standard in the region.

There is, however, much room for improvement. These high average rates hide wide disparities among different groups: older and more vulnerable children probabilities of attending school are much lower. Opportunity costs rise driving upper secondary rates below the secondary attendance average. The issue becomes even more pressing among those belonging to poor families.

Fortunately, both enrollment and attendance rates of secondary school have experienced a sizeable increase during the last decade in Argentina. This has been specially driven by improvements in attendance of those in the upper secondary age-range and among the most vulnerable.

This improvement could be related to the National Education Law. Passed in December 2006, its main regulation implied the extension of compulsory education to upper secondary. We find, however, that neither the law nor accompanying policies had enforcement mechanisms embedded in their design. In fact, three years after the law was passed, attendance rates for those in the upper secondary age-range (15-17) were almost exactly the same.

But if the 2006 National Education Law has no enforcement mechanisms, what encouraged children aged 15 to 17 (especially those most poor) to start attending school during the last decade? In this paper we claim that the *Asignación Universal por Hijo (AUH)* program implemented in 2009 may be driving this increase in attendance rates.

Launched in Argentina in November 2009, it represents a massive conditional cash transfer program that currently benefits 29% of all children in the country. It is targeted at children living in poor families not registered in the formal employment

sector. Compliance with school attendance for children under 18 years old is one of the conditionalities imposed by the program in order to fully access the benefit. In this sense, the AUH may be working as an enforcement mechanism of the National Education Law.

Estimating the causal effect of the AUH on school attendance rates, however, represents a rough task. The AUH was not assigned randomly nor it was 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)* carried out in Argentina. We classify children in upper-secondary age-range (15 to 17) either as eligible or not eligible according to whether their parents comply with the program's requirements. We compare their attendance rates across time following a difference in difference approach.

Our estimates suggest that the AUH increased the secondary attendance rate for those aged 15 to 17 by 3.9 percentage points among those eligible. The impact seems to be higher for boys than for girls and among single-parent households where the head has lower education levels. The results are robust to different specifications and checks.

This work intends to make a contribution in several realms. First of all, it adds to the literature on the impact of conditional cash transfers programs on educational outcomes. Secondly, and not least considering the relative scarce studies, it attempts to provide evidence of the effects of the *Asignación Universal por Hijo*, thus generating input for future improvements of the program. Finally, it also points out to the potential effectiveness of conditionalities as enforcement mechanisms of compulsory schooling.

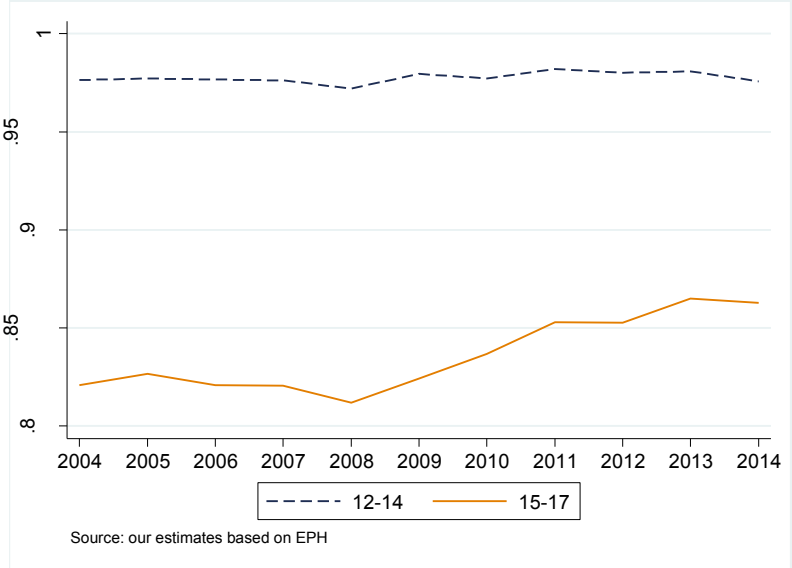
The rest of the paper is organized as follows. Section 2 expands on the link between attendance rates in Argentina and compulsory education legislation. Section 3 concentrates on the main characteristics of the AUH program while briefly presents a review of related literature. The following section explains in detail the data and methodology used. The main results are presented in Section 5. The next two sections lay out robustness checks and explore heterogeneous effects respectively. The last section concludes and points to further research.

2. Attendance Rates and Compulsory Education in Argentina

School enrollment and attendance rates have traditionally been quite high in Argentina relative to other Latin American countries. Primary net enrollment rates are almost perfect while secondary enrollment is approximately 85% (SITEAL), the highest level of the region.

Despite this encouraging panorama, relevant challenges remain to be faced in the middle level. Drop-out rates are still large, especially among children belonging to vulnerable families. As of 2009, secondary enrollment rates between the richest 40% and the poorest 30% differed by 20 percentage points (SITEAL). The issue becomes even more pressing among those in the last years of secondary education for whom the opportunity cost of attending school is much larger. As of 2009, according to our estimates attendance rates among children aged 15 to 17 belonging to the first quintile barely reached 76% as compared to 95% of rich children (and the gap was even larger in previous years). As expected, the group consistent of poor older boys showed the worst attendance rates: 71%. Furthermore, a comparison of the 2001 and 2010 census reveals secondary education rates had remained practically the same during the decade especially among those aged 15 to 17: 79.4% and 81.6% respectively (INDEC 2010).

Figure 1 – Attendance Rates by Age Groups[2]

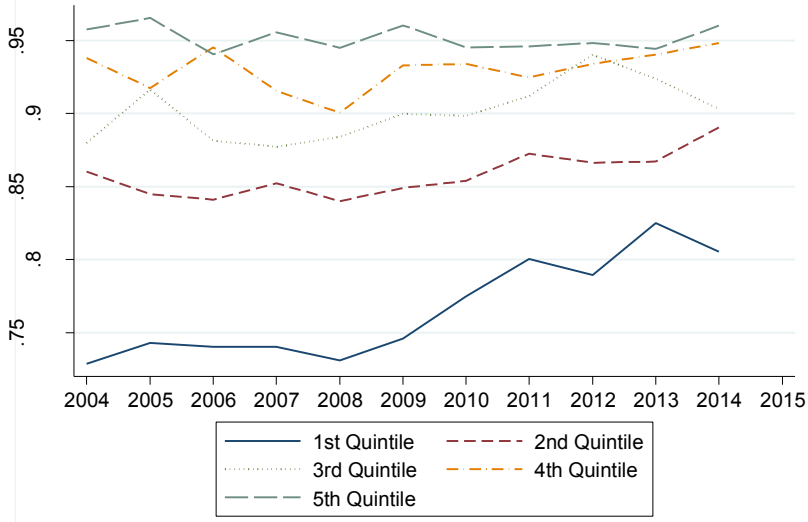


[2]Administrative data shows a very similar pattern for secondary enrollment (DINIECE: http://portales.educacion.gov.ar/diniece/).

Nevertheless, both enrollment and attendance rates have improved during the last decade. This is true not only for children in secondary school as a whole but especially among those aged 15 to 17 for whom the larger opportunity costs of attending school were reflected in quite lower attendance rates.

Most importantly, it seems attendance rates for those aged 15-17 have increased the most among the more vulnerable children (Figure 2). The first quintile attendance rate for that age group shows an 8 percentage points increase between 2004 and 2014, while it has remained unchanged among the richest children for whom rates were almost perfect already.

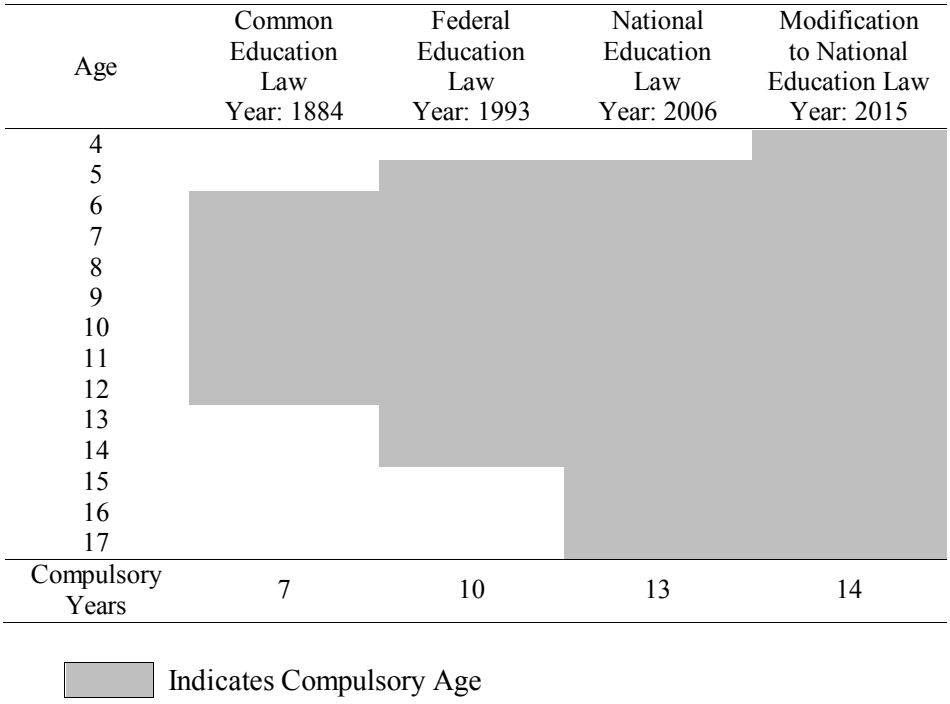
Figure 2. 15-17 Secondary Attendance Rates by Quintiles



Source: our estimates from EPH

This improvement could be related to the National Education Law (N°26.206) passed in December 2006. Among other regulations, the law extended compulsory education to the last three years of secondary education (upper secondary). Primary education had been mandatory since the very creation of the National Education System (1884) while in 1993 the Federal Education Law had extended compulsory education from 7 to ten years.

Figure 3. Extension of Compulsory Education in Argentina



Within the Latin American context, Argentina's public free education system has traditionally stood out. Only five 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 only recently in 2013 (Ruiz and Schoo 2014).

In the more developed countries, legislation regarding extension of compulsory education to the middle school was passed earlier. In OECD countries, secondary level reforms started right after the Second World War. A first wave of legislation was passed between 1950 and 1960 concerning lower secondary. The second wave of expansion included upper secondary education and took place between 1980s and 1990s (Briseid and Caillods 2004).

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Evidence on the impact of these changes in regulations on attendance rates is relatively scarce. While many studies concentrate on the effects regarding labor market outcomes (Angrist and Kruger 1991; Acemoglu and Angrist 2000; Oreopoulos 2006a, 2006ba) the evidence of the impact on schooling outcomes is hard to find. Even though in some of the mentioned studies improvement of attendance rates is documented the mechanisms through which this was achieved

are unclear. In many countries, these changes in legislation were accompanied by the reduction or even abolition of fees, the massive construction of schools and in some cases even established fines for those not complying with the law.

Regarding Argentina, taking advantage of the different timing in the implementation of the Federal Education Law passed in 1993 Alzúa *et al*. (2010) show that extending compulsory education to those aged 12-14 had a positive impact on the number of years of education completed. The effect, however, seems to be small among poor children (an extra half year of education). Furthermore, the paper also points to several institutional changes that accompanied the law: changes in the curricula and an extended budget (around US$ 3,000 million was invested in educational infrastructure and training). Thus, the mechanism through which schooling outcomes improved following the 1993 Federal Law remains unclear.

The simultaneous implementation of the 2006 National Education Law across all provinces prevents to perform a similar causal impact analysis for the expansion of mandatory education to those aged 15-17. Furthermore, while the 1993 Federal Education Law was followed by sharp increases in 13-14 enrollment rates (the age group affected by the 1993 law), the National Education Law of 2006 had virtually no impact on 15-17 age range enrollment rates (the group for which compulsory education was extended in 2006). Indeed, even though departing levels were similar, while 13-14 rates augmented from 84% to 92% approximately between 1993 and 1997 (Alzúa et al 2010), 15-17 enrollment rates remained virtually the same three years after 2006:

Table 1. Attendance Rates 15-17 age-group.

	Attendance Rates 15-17
2004	81.1
2005	81.2
2006	82.3
2007	82.7
2008	80.1
2009	82.5

Source: our estimates from EPH

This is consistent with the fact that the 2006 National Education Law was passed rather as an "aspirational" law that aims at guaranteeing the universal access to education services for all children aged 4 through 17. Indeed, the legislation has not relied on effective enforcement mechanisms to achieve its ends. Public education has traditionally been free in Argentina and virtually no punishments exist for those not complying with the law. It was, nonetheless, accompanied by some policies aimed at fostering school enrollment. In particular, a National Secondary Education Plan was launched simultaneously, but it was more focused on establishing an adequate normative framework and on improving institutional arrangements than in intervening directly on school attendance (UNICEF 2012).

But if the 2006 National Education Law has no enforcement mechanism embedded and does not seem to have had an impact on attendance rates for those aged 15-17 four years after its implementation, what is driving the documented increase as of 2010? (See Figure 1) What is encouraging children aged 15 through 17, especially poor children, to start attending school?

3. The AUH Program

By 2009, attendance rates for those aged 15 through 17 were at a standstill. As shown in the previous section, in spite of the 2006 National Education Law that made attendance compulsory for that age range no relevant increases had taken place in over a decade (INDEC 2010). In that context, the *Asignación Universal por Hijo* (AUH) program was launched.

Implemented in November 2009, the AUH represents a massive conditional cash transfer program that focuses on children living in vulnerable households. It was designed to extend the social protection network –generally tied to the formal sector- to the more vulnerable sectors of the population. The magnitude of the benefit as well as the expansion in the number of beneficiaries have no precedents in social policy in Argentina. In fact, the AUH was designed so as to replace many social programs which will be gradually eliminated. Therefore, it was made not compatible with transfers coming from any other social plan.

The AUH awards a monetary contribution per child to households where neither parents is registered in the formal sector. This includes all those inactive, unemployed, or informal (but earning less than the minimum wage)[3]. A special consideration is made for those parents working in the domestic service: even for those registered in the formal sector, applications to the program are allowed. It consists of a monthly payment for each child under 18 and for handicapped children with no age limit. Each household can perceive the AUH for a maximum of five children.

Since one of the aims of the AUH is to encourage human capital accumulation, the payment is divided in two parts. 80% of the transfer is received on a monthly basis, regardless of compliance with the program sanitary and educational requirements. In contrast, the remaining 20% is paid annually, once the requisites have been accomplished: school attendance, vaccination and periodical health controls. Originally, the program required that the child be enrolled in a public school. This clause, however, was never made effective given the large public opposition that claimed that many potential beneficiaries attended private schools with very low fees, generally subsidized with public resources.

The amount of the transfer has been modified since 2009, to cope with inflation rates. As of June 2015, the monthly transfer for each child is of $837 which represents 15% of the minimum wage in Argentina. Considering a family made of three children (the average among the first four deciles of the income distribution), this implies almost a 50% increase in income for a family earning the minimum wage. The annual payment of the retained 20% implies that if the educational (and health) conditionalities are met, the family receives for each child around $2000 at the beginning of the year.

Currently, more than 3.600.000 children benefit from these transfers, representing almost 29% of all children living in Argentina (ANSES 2012). Taking almost 0.8% of the GDP, this program represents one of the largest of its kind in Latin America.

[3] For a detailed description of the characteristics of the program: http://www.anses.gob.ar/asignaciln-universal/asignaciln-universal-hijo-144

The expected impact of the conditional cash transfers on educational outcomes may be traced in the literature. Some surveys expose the main conclusions in this regard. Cecchini (2014) states that while cash transfer programs have had a clear positive income in terms of 'access to school' indicators (improvements in enrollment and attendance rates) their capacity to induce changes in the final ends of educational systems (i.e., the effective learning of the children) remains rather unclear. Saavedra and García (2012) point to several factors that are associated with larger increases in these 'access to school' indicators of cash transfer programs: lower baselines, higher economical vulnerability among beneficiaries, conditionalities and less frequent payments seem to enlarge the impact of the programs.

In Latin America cash transfer programs have flourished abundantly and so has the literature aimed at evaluating them. Consistent with the mentioned surveys, the documented evidence in general points to improvements in educational outcomes. Even though the impacts differ across countries and are of course tied to the particular characteristics of the programs, in general results show that conditional cash transfer programs improve the so-called "intermediate objectives": better access to school, higher enrollment rates, higher attendance (Cecchini 2014, Bastagli 2008). More relevant effects are usually found where initial levels were lower and among the most vulnerable families. In the case of *Progresa* (Mexico), evidence shows a significant increase in enrollment rates especially among secondary school children (Schultz 2004) improvement in the probability of attending school (De Braw y Hoddinott 2008), a reduction of the gender gaps in secondary enrollment in rural areas (Parker 2003) as well as increases in indigenous children enrollment (Escobar y de la Rocha 2002, 2008) and reductions in drop-outs (SEDESOL 2008). For *Borsa Familia* (Brasil), attendance rates seem to increase between 2 and 4 percentage points on average across all levels (Silveira Neto 2010, De Braw 2012). *Familias en Acción* (Colombia) seems to have had more impact in attendance rates among children in secondary education (Attanasio *et al*. 2008). *Tekopora* (Paraguay) has contributed to increase in 2.5% the enrollment and between 5 and 8 percentage points the attendance rate among the beneficiary children (Veras Soares, Pérez Ribas e Hirata 2008).

As for the AUH in particular, the evidence of the program's success is scarce in quantitative terms, probably due to the fact that publicly available official

household survey data in Argentina does not include a variable capturing AUH beneficiary status, turning difficult to perform a rigorous evaluation. In spite of this, different studies tend to confirm that its impact on income distribution variables as well as poverty indexes has been positive. Gasparini and Cruces (2010), Rofman and Olivieri (2011), Agis *et al* (2010), Bertranou and Maurizio (2012) agree that poverty rates have fallen, although differing in the size of the impact. Some possible unplanned side-effects of the program have been evaluated as well, such as its impact on fertility decisions (Garganta *et al*. 2015), on formal market participation (Garganta *et al.* 2015) and female participation in the economy (Garganta *et al.* 2015).

As for the AUH impact on human capital variables, the evidence is scarce. In terms of school attendance, using a difference in difference approach Paz and Golovanevsky (2014) find that in terms of primary school there is a significative yet very small increase in attendance rates in favor of those not eligible for the program, while the effect is large and positive for those between 13 and 17 (around 7 percentage points). Taking advantage of the panel structure of the EPH they focus only on a two year window (2009-2010). The secondary level is analyzed as a whole, no evidence regarding lower and upper secondary differences is presented. Outcomes related to educational quality such as the educational gap have also been explored, but the effect is not clear and depends on the educational level under analysis (D'Elia and Navarro 2013).

The mechanisms through which the AUH may improve educational outcomes seem rather straightforward. The very existence of the transfer relaxes the family's budget constraints. Inasmuch as education may be regarded as a normal good its consumption could increase when household income does. The 20% conditionality sets an incentive to bias this increase in consumption towards investment in education. In this way, the AUH addresses several possible causes that may refrain children –and especially those in the upper-secondary age-range- from attending school: not being able to afford educational costs[4], having the need to work to increase household income, being in charge of siblings while the household adults are at work, etc. Furthermore, the conditionality may tilt the balance of those on the verge of dropping out. Clearly, the effects of this benefit may be heterogeneous

[4] Public education is free in Argentina. By educational costs we refer to transport and material costs.

across the population, as much of the mentioned literature points out. On the one hand, smaller impacts should be expected for those outcomes (enrollment, attendance, etc) for which initial levels are already high. On the other hand, larger effects may take place for those in the upper levels, for whom the opportunity cost of being at school is higher. Finally, female educational outcomes may be more difficult to improve to the extent that female education demand seems to be more inelastic (Sosa-Escudero & Marchionni 2000).The potential effects of the program as well as its differentiated impact, however, may only be assessed by way of empirical verification.

4. Data and Empirical Strategy

This paper intends to explore whether the AUH program in Argentina is responsible for the last decade increase in enrollment rates for those aged 15 through 17. With this aim at hand, the ideal procedure would be to compare the attendance rates of two groups of children aged 15-17, similar in every aspect but for their participation in the program. This strategy is unfeasible for two reasons: as mentioned earlier, the implementation of the AUH in Argentina was neither based on a randomly designed selection process nor accompanied by a publicly available comprehensive dataset that may allow for follow-ups of the beneficiary population. The absence of these features greatly determinates both the data and the empirical strategy that may be used for assessing the program's impact in any planned (and/or unintended) outcome.

In the present work we resort to the Permanent National Household Survey (*EPH*) carried out by the National Statistics Office (*INDEC*). This survey is carried out quarterly in Argentina and gathers data on demographic, income and employment issues. It covers urban areas, representing 62% of total country population. Unfortunately, even though it includes several questions regarding educational characteristics of the individuals, the survey does not indicate whether the child receives the AUH or not. We therefore are able, at most, to identify those children that are eligible for the program and thus we may not estimate other than an 'intention to treat' effect.

Our universe includes children in the upper-secondary age range (15-17). We define a 'treatment' and a 'control' group based on their eligibility, which in turn is defined by their parents' status on the labor market. A child is classified as belonging to the 'treatment' group whenever his/her parents are either inactive, unemployed, informal or self-employed workers, or registered in the formal sector but working in the domestic service. The control group includes all those children in the upper-secondary age-range for whom at least one of their parents is employed in the formal sector.

Both groups are circumscribed to the first four deciles, since the focus of the program is set on vulnerable families. Nevertheless, it is still possible that non-registered middle and high income earners apply for the program given that the condition of "earning less than the minimum wage" is easily evaded. Qualitative (Pautassi *et al.* 2013) and quantitative evidence (Garganta *et al*., 2015), however, indicate that even if some cases may exist, they are isolated and scarce. Furthermore, our results are robust to different cut-offs.

As for the attendance rates, an individual aged between 15 and 17 years old is considered to be attending school if he or she goes to secondary school. Unfortunately, the EPH does not include information on the specific year the child is attending. We will concentrate on data for the first semester over the 2004-2014 decade.

We follow a *difference-in-difference* methodology (DD), comparing the differences in attendance rates of the treatment and control group before and after the AUH was implemented. Given the time span of our data, 'before' will include years from 2004 through 2009 (the AUH was launched in November) and 'after' is restricted to the 2010-2014 period.

This methodology allows for avoiding some of the endogeneity problems that may bias our estimates given the fact that we are comparing different individuals across time. Our identification assumption is that in the absence of the AUH program, the attendance rates of both groups would have remained similar after 2009. Simultaneously, we are assuming that there was no event contemporaneous to the AUH implementation that may have affected differently both groups in terms of their school attendance.

We propose a standard linear specification of the DD model:

$$A_i = \propto + \beta_1 T_i + \beta_2 After + \gamma(T_i.After) + \theta\, X_i + u_i \quad (1)$$

Where A is a binary variable indicating whether the individual attends secondary school; T identifies whether the child belongs to the treatment or control group; *After* tags periods after the AUH implementation and X includes a set of child and household level controls. We also control for time (year and quarter dummies) regional fixed effects and regional trends.

By defining two periods (i.e., 'before' (0) and 'after' (1)) we may show that the mean difference in attendance rates between those in the treatment and those in the control group is given by:

$$DD = (A_1^T - A_0^T) - \left(A_1^C - A_0^C\right) \quad (2)$$

And thus:

$$DD = \gamma + (\mu_1^T - \mu_0^T) - \left(\mu_1^C - \mu_0^C\right) \quad (3)$$

By assuming that the unobserved characteristics of the treatment and control groups do not have a differential impact on their schooling attendance, we may claim that the γ parameter represents the causal effect of the program. Indeed, this assumption is crucial in a program such as the AUH that was not randomly assigned. Only by way of it we may sustain that the expected value of the latter terms is zero and thus obtain a consistent estimator of the treatment effect represented by γ.

5. Results

Table 2 shows that attendance rates for upper secondary increased for both the treatment and the control group after the AUH implementation. The effect, however, was considerably larger for the former: 5.1 percentage points increase for the treatment group as compared to 1.9 for the control group. This preliminary evidence suggests that the program may have had the effect of rising upper secondary attendance rates by 3.2 percentage points.

Table 2 - Differences in Upper Secondary Attendance Rates

	Treatment (i)	Control (ii)	(i)-(ii)
Before AUH	75.1	87.0	-11.9
After AUH	80.2	89.0	-8.7
Difference (After-Before)	***5.1***	***1.9***	***3.2***

Source: our estimates from EPH

It is true, however, that the very nature of the program and its non-random assignment implies that both groups differ both in their characteristics and, consequently, in their attendance rates prior to the program. Table 3 shows that in fact the treatment group differs from the control group in two significant aspects: it is poorer on average and includes a larger proportion of single-parent and female headed households. As for human capital variables, on average in both groups household heads finished primary and continued for one or two years into secondary school, even though it is true that household heads belonging to the treatment group show on average one less year of education. Households seem to have the same size on average and the difference in the number of children per household is negligible.

Table 3 - Descriptive Statistics

	Before				After			
	Treatment Group	Control Group	Difference	P-Value	Treatment Group	Control Group	Difference	P-Value
Male	51.5	51.0	0.0	0.6	51.0	51.5	0.0	0.9
Single-Parent HH	36.7	17.2	19.5	0.0	37.3	16.8	20.5	0.0
Female (HH head)	38.9	20.2	18.7	0.0	44.4	25.2	19.2	0.0
Age (HH head)	46.9	45.9	1.0	0.0	46.0	46.8	0.8	0.0
Years of Education (HH head)	7.9	9.0	-1.1	0.0	8.2	9.7	-1.5	0.0
Household Size	6.0	6.0	0.0	0.4	5.8	5.7	0.1	0.1
Number of Childre under 18	3.3	3.2	0.1	0.0	3.2	3.1	0.2	0.0
Per Capita Income	184.4	290.7	-106.3	0.0	696.0	997.4	-301.4	0.0
Observations	***12,466***	***6,363***			***10,002***	***6,171***		

Source: our estimates from EPH.

These differences are reflected in the different levels of the attendance rates for each group prior to the implementation of the program. Nevertheless, albeit attendance rates *levels* differed before the AUH started, we claim that the *time patterns* were similar, as may be appreciated in Figure 4 and confirmed by the results of a pre-program common trends test: we find no enough evidence to reject the null hypothesis that the pre-treatment trends were equal[5]. Thus, even though upper-secondary attendance rates were higher for the control group than for the treatment groups before the program, both groups were experiencing similar increasing trends. This in turn provides evidence in favor of our identification assumption (i.e. trends for both groups would have remained similar after 2009 if the AUH had not been launched), even though it may not be tested.

Figure 4. 15-17 Secondary Attendance Rates: Treatment and Control Groups

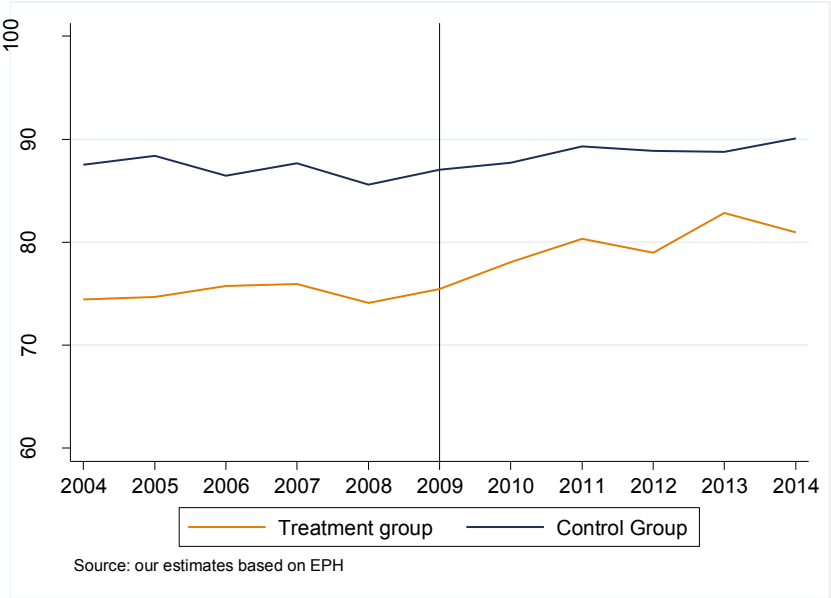

Table 4 shows the results of the linear model of attending school for those aged 15 through 17 outlined in the previous section. In that model the coefficient of the interaction variable between the *Treatment* dummy (equals 1 if the child belongs to the treatment group and 0 if it belongs to the control group) and the *After* dummy

[5] 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 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.

(equals 1 for observations corresponding to years 2010 through 2014 and 0 for the previous period) is positive and statistically significant in all the specifications. Models 1, 2 and 3 in the table progressively control for child and household head characteristics (gender, household head´s gender, age, squared age, educational level), other household characteristics (total number of members, single-parent household, per capita income) and region (6 regions) time fixed effects (years and quarter) and regional trends. The treatment effect remains almost the same across specifications and it implies that the mean attendance rate for the treatment group is around 4 percentage points higher than it would have been in the absence of the program.

Table 4 – AUH Impact on School Attendance – 15 to 17 age range

	(1)	(2)	(3)
Treatment*After	***0.0345*** (0.00813)***	***0.0385*** (0.00918)***	***0.0387*** (0.00912)***
Treatment	*-0.0809*** (0.00600)*	*-0.0807*** (0.00636)*	*-0.0793*** (0.00641)*
After	*0.00987 (0.00619)*	*-0.000290 (0.00690)*	*0.0354 (0.0427)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, 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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional time trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Our estimations indicate that the effect of the AUH program on the treatment group is statistically significant. That is, in the absence of the program that group would have had lower attendance rates. In particular, the program seems to have increased secondary attendance rates among those eligible aged 15-17 by almost 4 percentage points.

6. Robustness Analysis

In this section we carry out several robustness checks aimed at strengthening the results found in the previous section, either by contributing evidence in favor of our identification assumption or by showing that results are not sensitive to changes in operational definitions. In particular, we run the same lineal model for different placebo dates, for alternative samples and for a different definition of the pre-intervention period. The placebo experiments show that it is only when considering 2009 as the intervention date we may show consistent differences among the treatment and control groups. Furthermore, the evidence suggests that the effect does not depend upon the sample used nor on the definition of the pre-intervention period.

Placebo Experiments

First of all, we run a series of placebo experiments that intend to contribute evidence in favor of our identification assumption, that is, that the attendance rate of the treatment group would have evolved in the same pattern that the control group in the absence of the program. With that aim, we run the same linear model but pretending that the program took place in any year previous to 2009 (when the AUH was really implemented). Table 5 shows the results for five alternative 'placebo' dates: 2004, 2005, 2006, 2007 and 2008. In all cases the coefficient accompanying the interaction term is not statistically significant. This implies that only in late 2009 some event shifted the attendance rates for the treatment group, clearly not before.

Table 5 – AUH Impact on School Attendance – 15 to 17 age range

Placebo Regressions

	(1) 2004	(2) 2005	(3) 2006	(4) 2007	(5) 2008
Treatment*After	***0.0196***	***0.0256***	***0.0182***	***0.0169***	***0.0163***
	(0.0183)	***(0.0165)***	***(0.0142)***	***(0.0126)***	***(0.0153)***
Treatment	*-0.0881****	*-0.0896****	*-0.0818****	*-0.0776****	*-0.0742****
	(0.0168)	*(0.0146)*	*(0.0105)*	*(0.00813)*	*(0.00734)*
After	*-0.0542***	*0.0421*	*0.0474*	*0.0485*	*0.0755**
	(0.0232)	*(0.0400)*	*(0.0401)*	*(0.0393)*	*(0.0428)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, 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); child´s and/or household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional time trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

As stated before, this does not prove that it was the AUH that generated an improvement in secondary attendance rates for the 15-17 age-range, but rather that an event occurring in late 2009 did so. Other contemporaneous events may have taken place in that date and be responsible for these results. It is worth noting, however, that the AUH was a nation-wide massive program directly linked to educational outcomes through the conditionalities imposed not paralleled to any other program developed in the last decade.

Alternative Samples

One important consideration to be made: the AUH is awarded only if neither parent of the child has a formal job. Thus, to determine eligibility we need to know both

parents' employment conditions. This is not possible when one or none of the child parents live within the household[6]. We have thus defined three alternative nested samples that account for this three possible situations: *(i)* a sample that only contains those children for whom both parents are present; *(ii)* one that includes children for whom both or at least one parent are present; *(iii)* one that also incorporates those children living in households where neither parent is present. When considering our universe (children aged 15-17 belonging to the first four deciles) sample *(i)* represents 64.4% while sample *(ii)* adds up to 94.1% of our universe (sample *(iii)*, by construction, holds the total universe).

All of the analysis presented was carried out based on sample *(ii)*. This choice is grounded on conceptual reasons. On the one hand, it extends sample *(i)* by including many single-parent, 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, we are excluding those children for whom we have no information on neither of their parents working conditions. The 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 program's eligibility conditions but the parent not living within the household does not. Nevertheless, we estimate that at the most 9% of our sample could be wrongly classified in the treatment group[7].

Given that all the analysis has been conducted for sample *(ii),* in this section we include robustness checks using the alternative samples. The program effect in secondary attendance rates for those aged 15 to 17 is not altered when using these alternative samples (neither in magnitude nor in terms of statistical significance) as shown in table 6.

[6] The latter generally includes households where grandparents take care of children.

[7] This estimate derives from making quite pessimistic assumptions. On the one hand, we assume that all missing parents live and are recognized as such. On the other hand, we consider that missing parents have the same formality rate than present parents (around 36%).

Table 6 – AUH Impact on School Attendance – 15 to 17 age range

Alternative Samples

	(1) Sample (i)	(2) Sample (ii)	(3) Sample (iii)
Treatment*After	***0.0350******	***0.0387******	***0.0361******
	(0.0103)	***(0.00912)***	***(0.00882)***
Treatment	*-0.0784****	*-0.0793****	*-0.0814****
	(0.00767)	*(0.00641)*	*(0.00608)*
After	*0.0579*	*0.0552*	*-0.0236*
	(0.0342)	*(0.0456)*	*(0.0239)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, equals 1 if the child attends upper 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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Alternative definition of Pre-Intervention Period

It could also be the case that the National Education Law of 2006 may have had some effect on upper-secondary attendance rates and thus it would be more accurate to establish the pre-intervention period not from 2004 to 2009 but from 2007 to 2009. Table 7 shows that even restricting the pre-intervention period we find very similar results. Column (1) shows results for our linear model based on the 2004-2009 pre-intervention period while column (2) shows results taking 2007-2009 as the pre-intervention period. Coefficients are quite similar in terms of magnitude and statistical significance.

Table 7 – AUH Impact on School Attendance – 15 to 17 age range

Alternative Pre-Intervention Periods

	(1) Pre-intervention period: 2004-2009	(2) Pre-intervention period: 2007-2009
Treatment*After	***0.0387*** (0.00912)***	***0.0331*** (0.0100)***
Treatment	*-0.0793*** (0.00641)*	*-0.0746*** (0.00831)*
After	*0.0604 (0.0390)*	*-0.0171 (0.0248)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, 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); child´s and/or household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

7. Heterogeneity of Results

As stated before, we expect to find disparities behind the average effect found on attendance. Following the literature, heterogeneities will most probably be linked to lower baseline levels of the indicator evaluated. In particular, in this section we explore differences across age ranges, gender and household head's characteristics.

Alternative Age Ranges

Consistent with international evidence, the effect of the AUH diminishes for younger children among whom enrollment rates were already very high before the implementation of the program. In particular, for those in the age of attending

lower secondary school (12 to 14 years old) the effect is statistically significant but its magnitude is very small (the AUH program seems to have increased attendance rates by less than 1 percentage point among the treatment group). For those in primary school age-range (6 to 11), the effect is even smaller (0.4 percentage points).

Table 8 – AUH Impact on School Attendance

Alternative Age Ranges

	(1) 6-11	**(2)** 12-14	**(3)** 15-17
Treatment*After	***0.00422*** (0.00152)***	***0.00834** (0.00314)***	***0.0387*** (0.00912)***
Treatment	*-0.00400*** (0.00110)*	*-0.0159*** (0.00261)*	*-0.0793*** (0.00641)*
After	*-0.0231** (0.0119)*	*-0.00595 (0.0173)*	*-0.0159 (0.0264)*
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	69,332	34,904	35,002

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, equals 1 if the child attends the corresponding level (primary if 6 to 11 years old, secondary otherwise); *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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Ages

It is interesting to explore if and how effects differ within the 15-17 age range. Both the literature and the preceding results suggest that the effect should decrease with age. Surprisingly, our estimates show a rather unclear pattern that is not consistent with this evidence. Table 9 shows that the greatest effect seems to be operating among older children.

Table 9 – AUH Impact on School Attendance

Alternative Ages

	(1) 15 years old	(2) 16 years old	(3) 17 years old
Treatment*After	***0.0434**** ***(0.0132)***	***0.0278*** ***(0.0166)***	***0.0522**** ***(0.0160)***
Treatment	*-0.0753**** *(0.00933)*	*-0.0568**** *(0.0103)*	*-0.107**** *(0.00944)*
After	*-0.0358* *(0.0352)*	*-0.0516* *(0.0487)*	*0.0635* *(0.0499)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, equals 1 if the child attends secondary school; *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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Gender

In terms of gender, it seems all the increase in attendance rates was driven by improvements in boy's attendance: their rate augmented in about 5 percentage points while the effect on girls is not statistically significant. This is consistent both with the fact that initial rates were lower for that group (around 70% as compared to 80% of girls among the treatment group) and with evidence shown in the literature that points out that girls educational demand is rather inelastic (Sosa-Escudero and Marchionni 2000)

Table 10 – AUH Impact on School Attendance

Gender

	(1) Boys	(2) Girls
Treatment*After	***0.0576****	***0.0179***
	(0.0111)	***(0.0124)***
Treatment	*-0.105****	*-0.0523****
	(0.00821)	*(0.00756)*
After	*0.0190*	*0.0495*
	(0.0391)	*(0.0436)*
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

Source: our estimates based on EPH.
Note: OLS estimations. Dependent binary variable: *Attendance*, equals 1 if the child attends secondary school; *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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

Household Type

The AUH shows larger effects on the 15-17 attendance rate in single-parent households and where the head shows low levels of education (less than secondary). This is consistent with the fact that children belonging to those households presented lower attendance rates by 2009.

Table 11 – AUH Impact on School Attendance

Household Type

	(1) **Two-Parent**	(2) **Single-Parent**	(3) **Low Education**	(4) **High Education**
Treatment*After	***0.0370*** (0.0111)***	***0.0414*** (0.0148)***	***0.0341*** (0.0104)***	***0.0251** (0.0142)***
Treatment	*-0.0768*** (0.00790)*	*-0.0729*** (0.00967)*	*-0.0875*** (0.00740)*	*-0.0470*** (0.00973)*
After	*0.0589 (0.0456)*	*0.0413 (0.0885)*	*0.108** (0.0523)*	*0.0267** (0.0147)*
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	17,822	17,180	25,505	9,497

Source: our estimates based on EPH.
Note: OLS estimations. "Low Education" includes household which head has less than secondary school education, "High Education" refers to households where head completed secondary education. Dependent binary variable: *Attendance*, equals 1 if the child attends secondary school; *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 household head´s characteristics (gender, household head age, household head squared age, household head educational level), other household characteristics (household size, per capita income, single headed household), region fixed effects (6 regions), time fixed effects (years and quarter) and regional trends. Clustered robust standard errors in parenthesis; * p<0.10, ** p<0.05, *** p<0.01.

8. Concluding Remarks and Further Research

Attending school at certain age ranges involves a large opportunity cost. It is common knowledge that attendance rates tend to fall as educational levels proceed, especially among less developed countries. Upper-secondary education thus generally shows attendance rates far from being complete, and the issue is more

severe among the economically vulnerable. Laws aimed at making higher levels compulsory do not seem to produce relevant effects unless they include enforcement mechanisms, either embedded in their own design or as accompanying policy.

The *Asignación Universal por Hijo* in Argentina may be regarded in those terms: a large massive cash transfer program destined to those living in vulnerable conditions that includes a conditionality scheme aimed at encouraging families to invest in education at all compulsory levels. It is in this sense that it may work as an enforcement mechanism of the compulsory education laws, especially to the National Education Law of 2006 that turned upper secondary education mandatory.

Using a difference-in difference strategy based on the National Permanent Household Survey data we are able to identify a 3.9 percentage point increase in the secondary attendance rates for those aged 15 through 17 among potential beneficiaries. This increase in not homogenous: the effect seems to be larger for boys and for children living in single-parent households where household head's education is lower. The result is robust to several specifications and a large set of checks.

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. It is possible that the effect is given by the monthly benefit itself, or driven 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 conditional cash transfer programs. Secondly, it would also be relevant to disentangle whether this effect is matched by a similar result in the employment realm. It could be expected that an increase in attendance rates may contribute to reducing employment among those aged 15-17. It could also be the case, however, that children aged 15-17 are not working, but taking care of siblings, and that the *AUH* in that case may be altering instead female participation rates which in turn may increase schooling among those in upper secondary without having any impact in those children's employment. Although household decision processes are certainly difficult to assess, exploring this hypothesis would shed light on the mechanisms that are at work and thus further refine the *AUH's* design.

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