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Giving children a better start: Preschool attendance and school-age profiles ☆

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Abstract

We study the effect of pre-primary education on children's subsequent school outcomes by exploiting a unique feature of the Uruguayan household survey (ECH) that collects retrospective information on preschool attendance in the context of a rapid expansion in the supply of pre-primary places. Using a within household estimator, we find small gains from preschool attendance at early ages that get magnified as children grow up. By age 15, treated children have accumulated 0.8 extra years of education and are 27 percentage points more likely to be in school compared to their untreated siblings. Instrumental variables estimates that attempt to control for non random selection of siblings into preschool lead to similar results. Pre-primary education appears as a successful and cost-effective policy to prevent early grade failure and its long lasting consequences in low income countries.

JEL classifications: I2; J1
Keywords: Preschool; Pre-primary education; Primary school performance

1. Introduction

This paper estimates the effect of early exposure to pre-primary education on school stay-on rates and levels of completed education among individuals aged 7–15. We exploit a rather unique feature of the Uruguayan *Encuesta Continua de Hogares* (ECH) for the years 2001–2005 that collects retrospective information on the number of years of

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preschool attended. In order to control for unobserved household characteristics that are common to all children in the household and that might affect simultaneously exposure to pre-primary education and school progression we rely on a within household estimator that only exploits variability in the outcome and treatment variables across siblings. In order to account for the possibility of siblings' systematic differences in treatment and outcomes, we complement this strategy by instrumenting preschool attendance with average attendance rates by locality of residence and birth cohort. A major expansion in the provision of public pre-primary education in Uruguay over the last decade that led to an acceleration in preschool attendance among subsequent birth cohorts and that mainly affected more disadvantaged children generates sufficient variation in exposure to preschool education to warrant identification.

We find a significant positive effect of preschool attendance on completed years of primary and secondary education. This works through both a fall in grade retention rates since the early school years and a reduction in drop-out rates among teenagers. The gains from attending preschool increase as children grow older, so that exposure to pre-primary education leads to gradually diverging paths in school attainment between treated and untreated children. We show that pre-primary education, by reducing grade retention early in school life, reduces the probability of subsequent grade failure and the incentives for early drop-out. Preschool exposure appears as a successful policy to prevent early school failure and its long lasting consequences.

In poor countries, a large share of the population is excluded from the education system already at an early age and well before completion of the compulsory schooling cycle. Exclusion from the school system encompasses, in varying combinations, failure to enroll, late entry, intermittent and irregular attendance, high repetition rates and eventually early drop-out (UNESCO, 2005). In Uruguay, as in many other Latin American countries, the system is unable *de facto* to retain children in junior high school, despite this being in principle compulsory. Although graduation rates from primary school and enrollment in the first year of junior high are almost universal, as of 2001 about 25% of 25–29 years old declared not having completed junior high school.

In this context, early exposure to the school system appears as a possibly successful policy option. A large body of literature in neuroscience, psychology and cognition makes the case for early childhood interventions. Research has established that learning is easier in early childhood than later in life, and that nutrition and cognitive stimulation early in life are critical for long-term skill development (see, among others, Bransford, 1979; Shonkoff and Phillips, 2000; Shore, 1997 and Sternberg, 1985). Thus, learning starts well before the day children enter primary school. The process of cognitive development starts at home and it is expected that pre-primary education facilitates this process by planning and providing systematic activities for children. Indeed, there is a widespread belief among educators that the benefits of pre-primary education are carried over to primary school. In particular, teachers identify lack of academic skills as one of the most common obstacles children face when they enter school (see Rimm-Kaufman et al., 2000). Also, they perceive preschool education as facilitating the process of socialization and self-control necessary to make the most of classroom learning (see Currie, 2001).

In the economic literature, Carneiro and Heckman (2003) and Cunha et al. (2006) make a strong case for early investment in education. They suggest that the return to the investment in human capital declines exponentially during the life cycle, being the highest earlier in life. Not only the earlier the investment, the longer the time available for recovering it, but also some inputs are likely to have low returns when adopted later in life (e.g., it is hard to achieve any gains in IQ after a certain age) and potential complementarities arise among different types of investment, implying that higher levels of past inputs (and therefore of current human capital) yield higher returns to current investment in human capital.

While there is substantial empirical evidence that intensive early education interventions targeted specifically to disadvantaged children lead to significant benefits, both in the short and in the long run (see, among others, Lee et al., 1990; Barnett, 1993; Barnett, 1995; Currie and Thomas, 1995; Reynolds 1998; Karoly et al., 1998; Danziger and Waldfogel, 2000; Currie, 2001; Garces et al., 2002; Blau and Currie, 2004; Schweinhart et al., 2005; Belfield et al., 2006),[1] much less is known about the benefits of expanding pre-primary education for the population as a whole. Using variation across US states in the funding of kindergarten initiatives in the late 60s and early 70s Cascio (2004) finds that kindergarten reduces subsequent grade retention. Using data from the Early Childhood Longitudinal Study, Magnuson

[1] Although some of this work reports (e.g., Barnett, 1995; Currie and Thomas, 1995; Currie, 2001) that the gains in IQ and standardized achievement tests that are caused by these interventions may fade out over time, it also reports long lasting benefits on measures of scholastic achievement (e.g., grade retention, special education placement, school drop-out, high school graduation and college enrollment) as well as a reduction in crime and welfare usage.

et al. (2007) find that pre-primary education in the US is associated with higher reading and mathematics skills at primary school entry, but that these effects dissipate for most children by the end of first grade. Exploiting a natural experiment for Argentina, Berlinski et al. (2006) find a positive effect of pre-primary school attendance on third grade standardized Spanish and Mathematics test scores and on primary school pupils' behavioral outcomes such as attention, effort, class participation, and discipline.

A major challenge in identifying the causal effect of pre-primary school attendance on later school outcomes is non-random selection into early education. Positive selection, whereby parents whose children attend pre-primary school possess characteristics that promote better school performance, would result in a spurious positive correlation between preschool and later academic outcomes. Indeed, since children are not randomly selected into pre-primary education, selection based on parental heterogeneity is likely to be non-ignorable. In order to circumvent this problem, in this paper we control for unobserved determinants of school progression that are correlated with selection into pre-primary education by conditioning on household fixed effects in the regressions. This approach is similar to the one followed by Currie and Thomas (1995, 1999) and Garces et al. (2002) who examine the impact of Head Start on school performance using longitudinal data. To the extent that unobserved household characteristics affect all children in the same household similarly, this approach should control successfully for the potential bias in the OLS estimates due to household heterogeneity.

Nevertheless, parents may treat siblings differently, so that non-random selection within households is a potential threat to the consistency of the within households estimates. Parental preferential treatment of some children or changes in household resources along the family's life cycle might imply that some siblings in the same households are both more likely to attend preschool and to perform better in school or stay-on longer. To address this potential threat to the identification, we rely on a variety of approaches. First, we control for some of the potentially spurious correlation between treatment and outcomes by conditioning on a number of children's characteristics, such as order of birth, gender and mother's age at birth. Second, we present instrumental variable estimates that exploit average enrollment by cohort and locality as an instrument for treatment. Such source of variation is arguably uncorrelated with children's unobserved characteristics within each household, hence leading to consistent estimates of the treatment effects.

The rest of the paper is organized as follows. Section 2 provides background information on the Uruguayan school system and the educational reform of the 1990s that led to a rapid acceleration in preschool enrollment. Section 3 describes the data. Section 4 lays the empirical strategy and discusses the identification strategy. Section 5 presents the regression results. Section 6 presents a cost benefit analysis of this policy and Section 7 finally concludes.

2. Background

Uruguay is a relatively small middle-income country, boasting a long tradition of social inclusion and publicly provided education. Primary schooling was made compulsory in 1877. Universal primary schooling was achieved in the 1950s leading to high current adult literacy rates (97% among men and 98% among women). In terms of its education system, compulsory education comprises primary education (*Educación Primaria*, ages 6–11) and junior high school (*Ciclo Básico*, ages 12–14). Public provision of schooling also extends to pre-primary education (*Nivel Incial*, ages 3–5), supplied through both kindergartens (*Jardines de Infantes*) and increasingly so through primary schools (*Clases Jardineras en escuelas primarias con Educación Inicial*). Private fee-based education is also common, particularly in Montevideo, where it is estimated that around one third of children in primary education attend private institutions. In general, children in public pre-primary and primary educational institutions attend school four hours a day during a 180 day school term. Most of these institutions operate in two daily shifts (morning and afternoon).

Two of the most notable inefficiencies of the system are widespread grade retention and early drop-out (Manacorda, 2007). Both features are common to other Latin American countries (Urquiola and Calderon, 2004). Data from a specific education module administered in conjunction with the National Household Survey (*Encuesta Continua de Hogares*) of 2001 illustrate a long delay in the transition through the primary school system due to widespread grade retention. Despite normal entry into school (average age at entry is 5.82 versus a theoretical entry age of 6), and universal enrollment in primary school, by age 12 about 54% of children still have not completed primary education (sixth grade). Grade repetition affects 25% of primary school students and about 20% of those in secondary school. On average repeaters lose around 1.5 years in primary school and 1.2 years in secondary school. Data in ANEP (2005) show markedly more pronounced repetition rates among children from more disadvantaged backgrounds. Based on a socio-cultural indicator of schools, children in the bottom quintile of the distribution of that indicator are around three times more likely to repeat than children in the top quintile.

In an attempt to reverse the poor performance of the education system, in the mid-1990s, the Government of Uruguay took direct actions to achieve universal pre-primary education for 4 and 5 years old (ANEP, 2000). The motivation for this reform was twofold. First, this was meant to achieve an increase in the number of years of schooling without raising school leaving age. This appeared the most viable policy option given the inability of the system to retain a large proportion of teenagers. Second, this program hoped to ease children's insertion into and transition through the primary school system, by providing them with some basic foundations before the start of the primary cycle and socializing them (and their parents) to school from an early age. The hope was that this policy, by promoting a child's early socialization and alphabetization, would reduce the high incidence of repetition among primary school children, hence making the transition through the primary school cycle speedier and in turn reducing the incentive for early drop-out.

The lack of teaching infrastructures was a major constraint to a further expansion of the system and for this reason, in 1995 ANEP (*Administration Nacional de Educación Pública*), the government agency in charge of public education, started an ambitious building plan that aimed at expanding public preschool provision in public primary schools. By 1999, 414 new classrooms had been built (or made available via refurbishment), mainly through their addition to existing primary schools. It is estimated that another 370 classrooms were made available between 1999 and 2002. This policy was accompanied by an increase in the number of preschool teachers and a rationalization of existing spaces.

Based on government documents (ANEP, 2005), the reform was very successful at least as far as children incorporation into the system was concerned. In the face of a substantial stability in public pre-primary enrollment between 1992 and 1995 (with enrollment rising from 48,107 to 49,618 pupils), between 1995 and 2004 enrollment in public preschools grew from 49,618 to 87,237 pupils, a rise of 76% over 9 years. Moreover, the expansion attracted children from more disadvantaged backgrounds, while in 1991 attendance rates of 4 years old in households in the lowest quintile of the income distribution was in the order of 20%, by 2002 this figure was in the order of 60%.

3. Data and basic evidence

For the purpose of the empirical analysis we use micro data from the Uruguayan *Encuesta Continua de Hogares* (ECH). This is a representative household survey run throughout the year by the National Statistical Office (INE: *Instituto Nacional de Estadística*) that covers around 18,000 households each year in urban Uruguay. The survey collects data on the socio-demographic characteristics of the households and school attendance and highest grade completed for all individuals.

Starting from 2001 the ECH provides retrospective information on the number of years of pre-primary education completed. We can hence use data from 2001 to 2005 to relate current school attainment to past preschool attendance. One limitation of the data is that retrospective data on either past repetition or on school entry age are not available. The data also do not distinguish between the type of pre-primary school attended, whether public or private.

We restrict our analysis to a sample of individuals aged 7–15 that live in two parent families where all children are children of the head of the household. We restrict the sample to children of the household head due to the key role that within siblings differences play in the identification of the parameter of interest. We restrict to children aged 7 or older because some children aged 6 are still of preschool age during some survey months. We exclude children aged 16 or older for two reasons. First, by age 15 children should have completed their compulsory schooling cycle, so this appears a natural cutoff point. In addition, after this age, some of them (notably girls) have already moved out of their parental home and this is possibly correlated with preschool exposure.[2]

In Table 1 we define the variables used in the paper and present a set of descriptive statistics. We have a sample of 23,042 children over 5 years, 90% of them attended at least one year of preschool with an average of 1.75 years of preschool. Average age is 11 while the average years of education completed after preschool is 4.56. Therefore, on average, children have completed around half a year of education less than one would expect if they had all enrolled at age 6, progressed regularly and stayed on until age 15 (in which case one will expect 5 years of completed education). School enrollment is in the order of 97%, not far from universal, although – as shown below – quickly declining with age. On average, mothers have completed 10 years of schooling and mean mother's age at birth is 28.5.

[2] By age 16, 1% of individuals live outside their parental home (i.e. they classified as heads, spouses, non relatives or domestic employees). By age 20 this proportion rises to around 12%.

Table 1
Definition and description of variables

Variable	Description of variables	Mean	S.D.	Min.	Max.
Preschool Education	Years of preschool education completed as retrospectively reported by parents.	1.75	0.90	0	3
Attended 1, 2 or 3 years of preschool	=1 for children that attended 1, 2 or 3 years of preschool and 0 otherwise.	0.91	0.29	0	1
Years of Preschool=2	=1 for children that attended 2 of preschool and 0 otherwise.	0.40	0.49	0	1
Years of Preschool=3	=1 for children that attended 3 years of preschool and 0 otherwise.	0.22	0.42	0	1
Years of Schooling	Years of primary and secondary schooling completed.	4.59	2.58	0	12
School Attendance	=1 for children currently attending primary or above school and 0 otherwise.	0.97	0.17	0	1
Public School	=1 for children that attend public schools and 0 otherwise.	0.83	0.38	0	1
Age	Child age. In the regressions we use 8 age dummies.	11.04	2.56	7	15
Cohort	Birth cohort.			1986	1998
Female	=1 if the child is female and 0 otherwise.	0.49	0.50	0	1
Birth Order	Birth order among all cohabitating children. In the regressions we use 6 dummies.	1.45	0.73	1	7
Mother's Age at Birth	Age of the mother at birth. In the regression analysis we use 9 dummies.	28.52	6.19	12	51
Schooling of the Mother	Years of completed education of the mother.	9.80	3.95	0	23
Year	Year of Interview. In the regressions we use 4 year dummies.			2001	2005
Month	Month of Interview. In the regressions we use 11 month dummies.			1	12
Locality	Locality where the child lives. In the regressions we use 54 dummies.			1	55
Observations			23,042		

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.

In Table 2, we report the proportion of children attending school and the distribution of completed school grades at each age. Children can enroll in the first grade of primary education if they become 6 before the 10th of May of the school year (March–December) they intend to start. Because the ECH is collected continuously throughout the year and no information on the exact birth date is available, we concentrate on the months of January to April of the survey for the completed school grade statistics.[3] If entry into primary school were timely (at age 6) and transition from grade to grade were normal, children aged 7 during the interview months of January to April should have completed 1 year of education. However, 13% of them have not completed any education at this point. This problem aggravates as children become older. For example, 26% of children aged 9 are lagging behind. The first row of the table also illustrates rapidly growing drop-out rates from age 12 onwards. While, until age 11, school enrollment is almost universal (99%), by age 15 this is in the order of 90%.

In Table 3 we document the rapid rise in preschool attendance across subsequent birth cohorts. Here we report the coefficients of a regression of a dummy for preschool attendance on birth cohort dummies. In the first column we include no additional controls while in the second column we condition on household fixed effects. In practice, the latter investigates the growth in preschool attendance of siblings born in different years. Standard errors are heteroskedasticty consistent. The OLS estimates in column (1) show a pronounced trend in preschool attendance across cohorts. Preschool attendance grows by 12 p.p. between those born in 1986 (the omitted group) and those born in 1998. Results are qualitatively similar if one examines the fixed effect estimates in column (2). If anything, point estimates are slightly larger in magnitude.[4]

Although these results show a secular rise in preschool attendance in the population at large, they also mask substantial heterogeneity across different households. As already mentioned, the reform was apparently extremely successful in incorporating children from more disadvantaged backgrounds. Columns (3) and (4) check for this by reporting the same regressions as in columns (1) and (2) where now the cohort dummies are interacted with a dummy for mother's low education. We define a low-education mother as one with at most compulsory education (9 years of education). Around 50% of children in the sample have mothers with at most compulsory education so by this criterion we split the sample into two approximately equally sized groups. Column (3) shows that children of low-educated mothers start from lower enrollment. For the 1986 cohort this difference is in the order of 12 p.p. As time goes by, an

[3] In the regression exercises that follow we address this problem by conditioning on month of the survey.

[4] Consistent with the finding that participation grows across subsequent cohorts even within households, we find that participation among children whose elder sibling did not attended preschool is 43%. Among children whose younger sibling did not attend preschool, instead only 21% attended preschool. Effectively, preschool attendance grows at higher parities.

Table 2
School progression: school attendance (in percentages) and years of schooling completed by age

	Age								
	7	8	9	10	11	12	13	14	15
School Attendance	98.56	98.73	98.23	98.76	98.59	98.14	96.15	94.57	90.91
0 years of schooling	13.04	2.99	0.77	0.12	0.24	0.46	0.75	0.12	0.11
1 years of schooling	72.85	14.81	4.87	1.40	0.00	0.11	0.43	0.12	0.11
2 years of Schooling	14.11	65.83	18.72	5.93	3.10	1.38	0.85	0.98	0.34
3 years of Schooling	0.00	16.37	61.15	18.72	5.24	1.72	0.64	0.25	0.00
4 years of Schooling	0.00	0.00	14.49	60.23	16.55	4.94	2.67	0.62	0.23
5 years of Schooling	0.00	0.00	0.00	13.60	63.45	20.67	6.93	2.46	0.91
6 years of Schooling	0.00	0.00	0.00	0.00	11.43	56.37	18.76	12.18	7.43
7 years of Schooling	0.00	0.00	0.00	0.00	0.00	14.35	55.12	18.70	9.71
8 years of Schooling	0.00	0.00	0.00	0.00	0.00	0.00	13.86	50.43	20.00
9 years of Schooling	0.00	0.00	0.00	0.00	0.00	0.00	0.00	14.15	51.09
10 years of Schooling	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	9.71
11 years of Schooling	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.23
13 years of Schooling	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.11

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Note: The information on years of schooling is based only on data from the months of January to April.

increasing proportion of children are incorporated into the preschool system. This is true for both groups of children. However the data reveal a significant catching up among children of low-educated mothers starting with the 1992 birth cohort, i.e. the cohort supposedly entering pre-primary school (at age 5) in 1997. Notice that this is precisely the first cohort who should have benefited from the infrastructure expansion. The same pattern is found when we condition on household fixed effects, although differences between the two groups are generally smaller.

It is important to point out that the data in Table 3 are based on retrospective information on preschool attendance. One might be concerned that the increase in preschool attendance across cohorts in Table 3 is a statistical artifact of the data, stemming from older cohorts being more likely to underreport preschool attendance due to (systematic) recall error. To check for this, in Table 4 we compare the retrospective preschool data (from 2001–2005) with contemporaneous statistics computed from preschool attendance reported by parents of children age 3, 4 and 5 in several waves of the ECH before 2001. The statistics presented in this table are for all children and for the cohorts covered by the paper (1986–1998). In columns (1) to (3), we report the mean level of preschool attendance by cohort for children age 3, 4 and 5 respectively based on 1989–2000 data. Assuming that the children who enter preschool do not leave it before enrolling in primary school and that every child enrolls in primary school at age 6, these statistics provide unbiased estimates of the number of preschool years attended by each cohort. In practice, the share of each cohort enrolled at age 3 will be an unbiased estimate of the proportion of children having attended at least 3 years of preschool. Similarly, the proportions at age 4 and 5 provide estimates of the share attending at least 2 years and 1 year of pre-primary education respectively. The sum of these proportions, reported in column (4), gives an estimate of the number of years of preschool completed.[5] We report the same statistics based on retrospective information from the 2001 to 2005 data in columns (5) to (8). If anything, retrospective data tend to underestimate the average years of preschool (by around 0.15 years) and to slightly overestimate the probability of ever having attended preschool (by 0.05). Trends across cohorts though are remarkably similar in the two data sets, showing the same increase over time.

In sum, consistent with the evidence from administrative data, the ECH data confirm a strong delay in school progression among urban children and teenagers and a substantial school drop-out before completion of compulsory schooling. We find evidence of a rise in preschool attendance across cohorts, and we show that this rise is not a statistical artifact due to recall error. The timing of this increase is also remarkably consistent with the implementation of the preschool reform. We finally find that, in the face of a generalized upward trend in preschool attendance, a faster

[5] Let $P(A=j)$ be the probability of attending preschool at age j and $P(Y=k)$ the probability of having attended k years of preschool. Under the assumptions in the text (no drop-out and primary starting age equal 6) average preschool years by cohort is then $E(PS)=3*P(Y=3)+2*P(Y=2)+P(Y=1)=3*P(A=3)+2*[P(A=4)-P(A=3)]+[P(A=5)-P(A=4)]=P(A=3)+P(A=4)+P(A=5)$.

Table 3
The relationship between attendance to pre-primary education and birth cohort

	Dependent variable: attended 1, 2 or 3 years of preschool			
	(1)	(2)	(3)	(4)
Cohort = 1987	0.009	−0.000	0.001	0.004
	[0.018]	[0.023]	[0.020]	[0.023]
Cohort = 1988	0.004	0.002	−0.009	0.027
	[0.017]	[0.021]	[0.019]	[0.019]
Cohort = 1989	0.002	−0.006	−0.016	0.002
	[0.016]	[0.021]	[0.019]	[0.020]
Cohort = 1990	0.020	0.017	0.005	0.026
	[0.016]	[0.020]	[0.018]	[0.018]
Cohort = 1991	0.036	0.028	0.009	0.023
	[0.016]**	[0.020]	[0.018]	[0.020]
Cohort = 1992	0.045	0.053	0.011	0.031
	[0.016]***	[0.020]**	[0.018]	[0.019]
Cohort = 1993	0.066	0.063	0.032	0.039
	[0.015]***	[0.021]***	[0.017]*	[0.021]*
Cohort = 1994	0.077	0.079	0.034	0.037
	[0.015]***	[0.021]***	[0.017]*	[0.019]*
Cohort = 1995	0.091	0.086	0.039	0.047
	[0.015]***	[0.021]***	[0.017]**	[0.020]**
Cohort = 1996	0.083	0.108	0.035	0.048
	[0.016]***	[0.023]***	[0.018]**	[0.022]**
Cohort = 1997	0.099	0.123	0.049	0.063
	[0.016]***	[0.024]***	[0.018]***	[0.023]***
Cohort = 1998	0.117	0.127	0.064	0.074
	[0.016]***	[0.031]***	[0.018]***	[0.031]**
Low mother's education			−0.122	
			[0.027]***	
Low mother's education × cohort = 1987			0.008	−0.009
			[0.034]	[0.043]
Low mother's education × cohort = 1988			0.018	−0.044
			[0.032]	[0.039]
Low mother's education × cohort = 1989			0.023	−0.018
			[0.031]	[0.039]
Low mother's education × cohort = 1990			0.019	−0.020
			[0.030]	[0.037]
Low mother's education × cohort = 1991			0.043	0.006
			[0.030]	[0.038]
Low mother's education × cohort = 1992			0.056	0.035
			[0.030]*	[0.038]
Low mother's education × cohort = 1993			0.056	0.038
			[0.029]*	[0.039]
Low mother's education × cohort = 1994			0.072	0.071
			[0.029]**	[0.039]*
Low mother's education × cohort = 1995			0.090	0.063
			[0.029]***	[0.040]
Low mother's education × cohort = 1996			0.081	0.099
			[0.030]***	[0.043]**
Low mother's education × cohort = 1997			0.084	0.101
			[0.030]***	[0.045]**
Low mother's education × cohort = 1998			0.089	0.093
			[0.032]***	[0.059]
Observations	23,042	23,042	23,042	23,042
	Specification includes:			
Household dummies	No	Yes	No	Yes

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Notes: OLS regression. Omitted category: Birth Cohort of 1986. Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4
Years of pre-primary education by cohort using contemporaneous and retrospective data

Birth cohort	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Contemporaneous data				Retrospective data			
	Attended				Attended			
	>=3 years	>=2 years	>=1 year	Average years of preschool	>=3 years	>=2 years	>=1 year	Average years of preschool
1986	0.326	0.509	0.765	1.600	0.154	0.484	0.822	1.461
1987	0.354	0.531	0.757	1.642	0.168	0.493	0.830	1.491
1988	0.345	0.500	0.778	1.624	0.173	0.524	0.837	1.534
1989	0.330	0.478	0.775	1.583	0.181	0.519	0.835	1.535
1990	0.322	0.503	0.805	1.629	0.187	0.531	0.859	1.577
1991	0.358	0.522	0.817	1.697	0.191	0.546	0.876	1.613
1992	0.361	0.554	0.871	1.786	0.205	0.578	0.891	1.675
1993	0.326	0.604	0.871	1.801	0.202	0.623	0.912	1.737
1994	0.293	0.672	0.905	1.870	0.204	0.663	0.926	1.792
1995	0.317	0.687	0.905	1.909	0.216	0.669	0.943	1.827
1996	0.328	0.709	0.919	1.957	0.215	0.673	0.937	1.825
1997	0.316	0.719	0.900	1.935	0.227	0.692	0.949	1.868
1998	0.414	0.731	0.932	2.078	0.220	0.706	0.973	1.899

Source: Own calculations based on Encuesta Continua de Hogares 1989–2005.
Notes: Columns (1) to (4) use ECH data from 1989 to 2000 and Columns (5) to (8) use ECH data from 2001 to 2005.

rise took place among children from more disadvantaged backgrounds (proxied by those whose mother has at most compulsory education).

4. Specification and identification

In this section we present our empirical strategy to estimate the impact of preschool exposure on later school outcomes. Our objective is to devise a strategy that controls for potential spurious correlation between the treatment and the outcome variables.

In the next section we start by regressing school outcomes of child i of age a in household j at time t (Y_{iajt}) on a dummy variable (PS_i) for whether child i attended at least one year of preschool, unrestricted age and cohort dummies and interactions of the two. The model essentially identifies the effect of preschool education by comparing the school trajectories of children and teenagers who attended preschool to those who did not attend. We then also include in this model a full set of unrestricted locality dummies interacted with time dummies.[6] In particular, the first model we estimate is:

$$Y_{ijat} = \beta_0 + \beta_{1a} PS_i + X_i' \beta_2 + X_j' \beta_4 + \varepsilon_{ijat} \tag{1}$$

where X_i is a vector of observed child's characteristics (including at least cohort dummies interacted with age dummies) and where X_j, $i \in j$, is a vector of household characteristics (including — at least locality dummies interacted with time dummies). We are interested in the parameters β_{1a} which measure the effect of having attended at least one year of preschool on school attainment at age a.

The inclusion of these large set of controls goes a long way towards eliminating potential confounding effects that might lead to inconsistent OLS estimates. Besides the expansion of the preschool system, the Government of Uruguay underwent some other educational interventions during the mid 1990s–mid 2000s (ANEP, 2000). To the extent that other features of the Uruguayan school system changed in such a way to affect the same children who were exposed to an increase in the supply of pre-primary places and that these other interventions affected the speed of transition through the compulsory school system and/or the incentives to stay-on, one might be concerned that the OLS estimates of model (1) would be inconsistent. By conditioning on cohort-age dummies effectively we only exploit for identification the differential age profiles of individuals from the same birth cohort with different exposure to treatment (PS_i). If the other policy ingredients affected everybody in the

[6] Overall we have 55 localities. These localities correspond to the 18 neighborhoods (*Centros Comunales Zonales*) of Montevideo plus 37 localities from the urban areas of the other 18 provinces. We have 60 time dummies, defined based on the interaction of the interview month with the interview year.

same cohort similarly – independently of whether they attended pre-primary education or not – the inclusion of these controls should purge the OLS estimates of this source of potential bias. Similarly, by conditioning on locality-time dummies, we effectively compare individuals in the same cohort and of the same age living in the same area, and we abstract from time specific shocks to both the local demand and supply of schooling that might be correlated with preschool exposure over time.

In practice, though, even conditional on the large set of individual and household observed characteristics, a simple comparison of children with different exposure to preschool will not necessarily lead to consistent estimates of the effect of interest. As hinted at in the introduction, parental education, household permanent income and wealth, family background and tastes, parents' labor force status — just to quote a few- are all likely to affect both the probability of attending preschool and later progression in school. For example, more educated parents might have a preference for or the ability to afford preschool education for their children while at the same time promoting their academic achievement. If such family factors affect positively both variables, simple OLS estimates of school progression on preschool exposure are likely to lead to upward biased estimates of the effect of interest.

In order to circumvent this problem, as a second strategy we compare the differential school progression of siblings who experienced different exposure to preschool. As a variant of model (1) hence, we present estimates of the effect of preschool where we subsume unobserved household characteristics that are common to all children in a household by including household fixed effects (d_j) in the model and estimate the following equation:

$$Y_{ijat} = \beta_0 + \beta_{1a} PS_i + X_i' \beta_2 + d_j + \varepsilon_{ijat} \tag{2}$$

Model (2) identifies the effect of preschool exposure at each age by comparing siblings with different preschool histories. One can use differences in outcomes between a couple of siblings of different ages who either both attended or did not attend preschool to identify the age-cohort effects. One can then identify the effect of preschool exposure at different ages (the β_{1a}'s) by attributing any residual differences in outcomes between an otherwise identical pair of siblings with different preschool histories to preschool exposure.

If conditional on age, time, locality and cohort effects, any spurious correlation between preschool exposure and latent school outcomes can be attributed to family characteristics that are common across siblings, then model (2) leads to consistent estimates of the treatment effects of interest.

Clearly, while the within household estimator controls for the spurious correlation between exposure and outcomes between children in different households, it does not account for any spurious correlation within households, i.e. across siblings. Preferential treatment of some children or variations in household resources over the household life-cycle might lead to estimates of the treatment effects that are inconsistent. For example, if parents have systematic preferences for one of their children, and hence they tend to invest more in her/his human capital, this might lead to both higher preschool enrollment and better school outcomes for this child compared to her/his siblings. Thus, we check the robustness of our within household estimates by also presenting instrumental variable estimates that use the average preschool enrolment by cohort in each of the 55 localities in the ECH as an instrument for a child's preschool attendance.

Finally, in model (1) and (2), we have defined exposure to treatment as participating in a preschool program for at least one year. Clearly, it is possible, and certainly of policy interest, to analyze the effect of exposure at the intensive margin. This is to say, we measure the value added in terms of school progression of going to preschool for one, two or three years. In the next section, we present estimates that allow for the effect of preschool attendance to vary with the intensity of exposure using similar strategies to those described above.

5. Regression results

5.1. Preschool attendance and stay-on rates

In this section we present our empirical results.[7] We start by analyzing stay-on rates of individuals aged 7–15. Following model (1), in Table 5 we regress a dummy equal one if the individual is currently enrolled in school on a

[7] Two studies before us analyze the effect of preschool attendance on subsequent school progression among Uruguayan children. ANEP (2001) analyzes a panel of 268 children who attended pre-primary education since the ages of 4 or 5 and follows them up to first grade. ANEP (2005) uses administrative data from the *Evaluación Nacional de Aprendizaje en el primer nivel de la escolaridad* plus survey data from the education module of 2001 ECH. Both studies find a significant positive effect of preschool attendance on promotion rates and school progression. Differently from us, these studies only analyze the short-term effects of preschool and ignore the potential endogeneity of treatment.

Table 5
The Impact of Preschool Attendance on School Attendance and Years of Schooling Completed

	Dependent variable:							
	School `attendance				Years of schooling			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended 1, 2 or 3 years of preschool × Age = 7	0.035 [0.021]*	0.040 [0.019]**	0.033 [0.020]	0.043 [0.020]**	−0.065 [0.078]	−0.384 [0.155]**	−0.210 [0.066]***	−0.341 [0.151]**
Attended 1, 2 or 3 years of preschool × Age = 8	0.046 [0.025]*	0.048 [0.024]*	0.043 [0.023]*	0.053 [0.024]**	0.134 [0.086]	−0.187 [0.180]	−0.034 [0.086]	−0.143 [0.171]
Attended 1, 2 or 3 years of preschool × Age = 9	0.053 [0.027]*	0.056 [0.031]*	0.050 [0.026]*	0.056 [0.031]*	0.272 [0.131]**	0.002 [0.246]	0.131 [0.116]	0.011 [0.243]
Attended 1, 2 or 3 years of preschool × Age = 10	−0.003 [0.009]	−0.009 [0.021]	−0.007 [0.008]	−0.008 [0.022]	0.464 [0.099]***	0.211 [0.132]	0.293 [0.096]***	0.209 [0.135]
Attended 1, 2 or 3 years of preschool × Age = 11	0.035 [0.018]*	0.033 [0.030]	0.031 [0.018]*	0.033 [0.030]	0.620 [0.089]***	0.262 [0.119]**	0.462 [0.088]***	0.250 [0.121]**
Attended 1, 2 or 3 years of preschool × Age = 12	0.049 [0.012]***	0.053 [0.023]**	0.042 [0.011]***	0.049 [0.023]**	0.556 [0.124]***	0.397 [0.124]***	0.409 [0.121]***	0.359 [0.127]***
Attended 1, 2 or 3 years of preschool × Age = 13	0.107 [0.020]***	0.120 [0.023]***	0.102 [0.020]***	0.115 [0.023]***	0.952 [0.130]***	0.643 [0.196]***	0.835 [0.131]***	0.610 [0.187]***
Attended 1, 2 or 3 years of preschool × Age = 14	0.114 [0.027]***	0.144 [0.028]***	0.109 [0.026]***	0.138 [0.027]***	0.917 [0.141]***	0.852 [0.109]***	0.810 [0.133]***	0.811 [0.115]***
Attended 1, 2 or 3 years of preschool × Age = 15	0.214 [0.040]***	0.279 [0.057]***	0.206 [0.039]***	0.274 [0.056]***	1.029 [0.127]***	0.818 [0.153]***	0.881 [0.117]***	0.788 [0.157]***
Observations	23,042	23,042	23,042	23,042	23,042	23,042	23,042	23,042
	Specification includes:							
Age X Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year X Month X Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female, Birth Order	No	No	Yes	Yes	No	No	Yes	Yes
Mother's age at birth, Mother's Years of Education	No	No	Yes	Yes	No	No	Yes	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes	No	Yes

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Notes: OLS regression. All regressions control for cohort dummies interacted with age dummies and locality dummies interacted with time dummies. Additional controls include: the child's birth order, a gender dummy, mother's age at birth and dummies for mother's completed years of education. Standard errors clustered by locality in brackets (55 clusters). * Significant at 10%; ** significant at 5%; *** significant at 1%.

dummy for preschool attendance whose coefficient we allow to vary by age. As said, in this and all the other regressions we include age dummies interacted with cohort dummies, and locality-year–month of interview dummies. In column (2) we present the same specification with household fixed effects. In column (3) we present a specification like the one in column (1) where we additionally control for child's birth order, a gender dummy, mother's age at birth and dummies for mother's completed years of education. In column (4), we present within household estimates of the specification in column (3).[8] Standard errors in these and all other regressions are clustered by locality (there are 55 clusters).

Column (1) shows a significant positive effect of preschool on school enrollment that grows monotonically with age. While at age 7 the difference in enrollment between treated and untreated individuals is in the order of 3 p.p., by age 15 this difference is in the order of 21 p.p. and statistically significant.

[8] Regressions (not reported) that additionally attempt to control for differences across children by interacting children's observed characteristics (gender, order of birth and mother's age at birth) with mother's and household characteristics (mother's education and number of children) give essentially the same results. In addition, one can interact household characteristics such as maternal education with age and cohort and obtain similar results.

As said, it might be the case that years of preschool education completed are correlated with household traits that also determine drop-out rates. The evidence in column (2) where household fixed effects are included, suggests that – if anything – the omission of household characteristics leads to estimates that are slightly downward biased. For example, we estimate the effect of treatment at age 15 to be 28 p.p., around 30% higher than the OLS estimates.

One interpretation for this finding is that household unobserved characteristics affecting latent school attainment are negatively correlated with exposure to preschool. However, the evidence in Table 3 – based on household observable characteristics – suggests that this is unlikely to be the case, since children of low-educated mothers show a significant lower level of preschool enrollment. One alternative explanation is that children in households warranting identification in the fixed effect estimator, i.e. those displaying sibling's variability in preschool attendance, also display relatively higher returns to preschool. Recall that these are relatively more disadvantaged households.

The inclusion of observable characteristics such as order of birth, gender and mother's education reduces slightly the magnitude of the OLS estimates (cfr. Column (3) and (1)). For example, at age 15, differences between treated and untreated children are in the order of 21 p.p., only slightly lower than those estimated in column (1). Again the inclusion of controls in the household fixed effect model (column (4)) makes little difference to the magnitude of estimated coefficients. Generally it is hard to reject the null that the estimates in column (1) are statistically different from those in column (2) to (4). The estimates show a roughly monotonically increase in stay-on rates among those who attended preschool that leads to a gain of between 21–28 p.p. in stay-on rates by age 15.

Notice the large jump in stay-on rates between the ages of 14 and 15 (an increase of around 14 p.p.). Age 15 is the minimum legal working age in Uruguay. One interpretation is that non-preschoolers, with much worse school histories, tend disproportionately to take advantage of the opportunity to enter the labor market when this materializes. Consistent with this, regressions (available upon requests) show that at age 15, when all controls an household fixed effects are included, untreated individuals are around 7 p.p. more likely to work and 6 p.p. more likely to be looking for work than treated individuals, and these differences are statistically significant. At age 14 these differences are small (respectively 6 and −1 p.p.) and statistically insignificant. (No labor market information is available for children younger than 14).

5.2. Preschool attendance and educational attainment

Although we have documented that preschool attendance is associated to a higher stay-on rate among teenagers, this says little on the effect of the treatment on actual educational attainment. In principle, a higher stay-on rate does not necessarily imply more years of completed education if this is associated to a higher failure rate. In particular, if those children who happen to stay in school longer as a result of treatment are also those with lower latent educational attainment (e.g. those at higher risk of failing a grade), one might find little difference between treated and untreated individuals in terms of completed school grades.

In columns (5) to (8) of Table 5 we present the same models reported in columns (1) to (4) where the dependent variable is now maximum grade completed. In all the specifications we include both children who have already dropped out from school and those who are still in school, for whom the variable 'maximum grade completed' is right censored at age 15. Column (5), where only the basic set of control variables are included, shows that by age 8 children that attended preschool have already accumulated 0.13 more years of education compared to those who did not attend preschool. Again differences grow roughly monotonically with age, so that, by age 15, treated individuals have 1.03 extra years of education compared to non-treated individuals.[9] There is some evidence that these effects confound the impact of household variables that also affect children attainment. The inclusion of household fixed effects leads to slightly lower estimates of this effect (column (6)). Preschool appears to delay entry into primary school. At age 7, treated children have accumulated around a third of a grade less compared to their untreated siblings. By age 15, the attainment gap is in the order of 0.82 years. Similarly, the inclusion of observable controls reduces slightly the estimated coefficients (cfr. columns (5) and (7)). When both household fixed effects and additional controls are included (column

[9] Notice that among young children (ages 7-12), for whom preschool affects only marginally and generally insignificantly stay-on rates, our estimates provide essentially a measure of the effect of treatment on age-grade distortion (overage). This is simply the opposite of the effects reported in columns (5) to (8). From age 13 onwards our estimates mix the delay among those still in school plus the effect of drop-out, both of which tend to depress completed education among untreated individuals.

(8)), we find that by age 15 treated individuals have around 0.79 additional years of education relative to untreated individuals.[10]

5.3. Effects at the intensive margin

So far we have constrained the effect of preschool to be the same independently of the years of preschool attended. To investigate the presence of additional returns to extra years of preschool, we have re-estimated the regressions in Table 5, where we now allow the effect of treatment to vary by years of preschool (1, 2 and 3) attended. Rather than reporting a table with 27 different effects (i.e., 9 age groups times 3 possible years of preschool), we present these results in graphs. In Figs. 1 and 2 we report separate graphs for the effect of attending at least 1 year, 2 years and 3 years respectively on stay-on rates and educational attainment. The first row of each graph gives the effect at the extensive margin, the second row gives the additional effect of attending 2 or more years compared to 1 year and the third row gives the additional effect of attending 3 years compared to attending 2. In the left-hand panel we present estimates derived from a model where we condition on gender, age-cohort dummies, and locality-time dummies (as in columns (3) and (7) of Table 5). In the right hand panel we additionally include household fixed effects (as in columns (4) and (8) of Table 5). In both cases we report 95% confidence intervals around the point estimates.

In Fig. 1 we look at the effect of additional years of preschool on school attendance. The biggest effect of the treatment on school attendance is due to attendance at the extensive margin (having attended versus not having attended). There is a small additional effect associated to having attended a second year of preschool that shows up after age 12. There is a little evidence of gains from a third year of preschool. Results are essentially robust to the inclusion of household fixed effects, although the point estimates become less precise and the confidence intervals become wider.

In Fig. 2 we look at how the intensity of treatment affects years of schooling completed. Similarly to Fig. 1, the largest effect is at the extensive margin with an impact that increases with age. When we do not condition on household fixed effects, we find no statistically significant effects at the intensive margin. This suggests a monotonic relationship between years of preschool and completed schooling. However, once we condition on household fixed effects, these additional effects disappear indicating that they are a consequence of a spurious correlation between household traits and years of preschool attended. We conclude that there is little evidence of preschool effects on school attainment at the intensive margin.

One concern with this conclusion is that the types of schools attended by 3, 4 and 5 years old might be different. Since public pre-primary education in Uruguay mainly covers 5 (and sometimes 4) years old, one might worry that the estimates in Figs. 1 and 2 confound the true effect of different years of preschool exposure with differences in quality between private and public preschools.

Contemporaneous data allow us to distinguish the type of school attended by children, and shed some light on the role of this potential confounding factor. Using ECH data from the 1990s, we have computed the probability of attending public preschool for all individuals aged 3 to 5. Regressions (available upon request) show that, conditional on attendance, public preschool attendance rises significantly with age, implying that children migrate from private to public preschools as they grow older or that the increase in enrolment over age is largely due to children previously out of preschool gradually entering the public system. Interestingly, though, when two or more siblings are compared, differences in the type of institution attended become negligible in size and statistically insignificant. This seems reasonable, as there must be costs associated to sending children from the same household to different preschools.

The household fixed effect estimates hence pick up genuine variations in years of preschool, while the OLS estimates additionally reflect, among other things, differences in the type of educational establishments attended. The within household estimates point unequivocally to insignificant returns to additional years of preschool.

5.4. Heterogeneous effects

We now investigate whether and to what extent there are differential effects of preschool exposure for different groups of individuals. In Table 6, we present separate results of preschool exposure on stay-on rates for children of low-

[10] We have also estimated a model where we fully interact age and cohort dummies with a dummy for preschool. However, we find little evidence of models (1) and (2) being outperformed by this more flexible model. As a further robustness check, we have run our regressions only on children aged 11-15 or children born before 1995. These regressions lead to similar conclusions, although standard errors are generally larger.

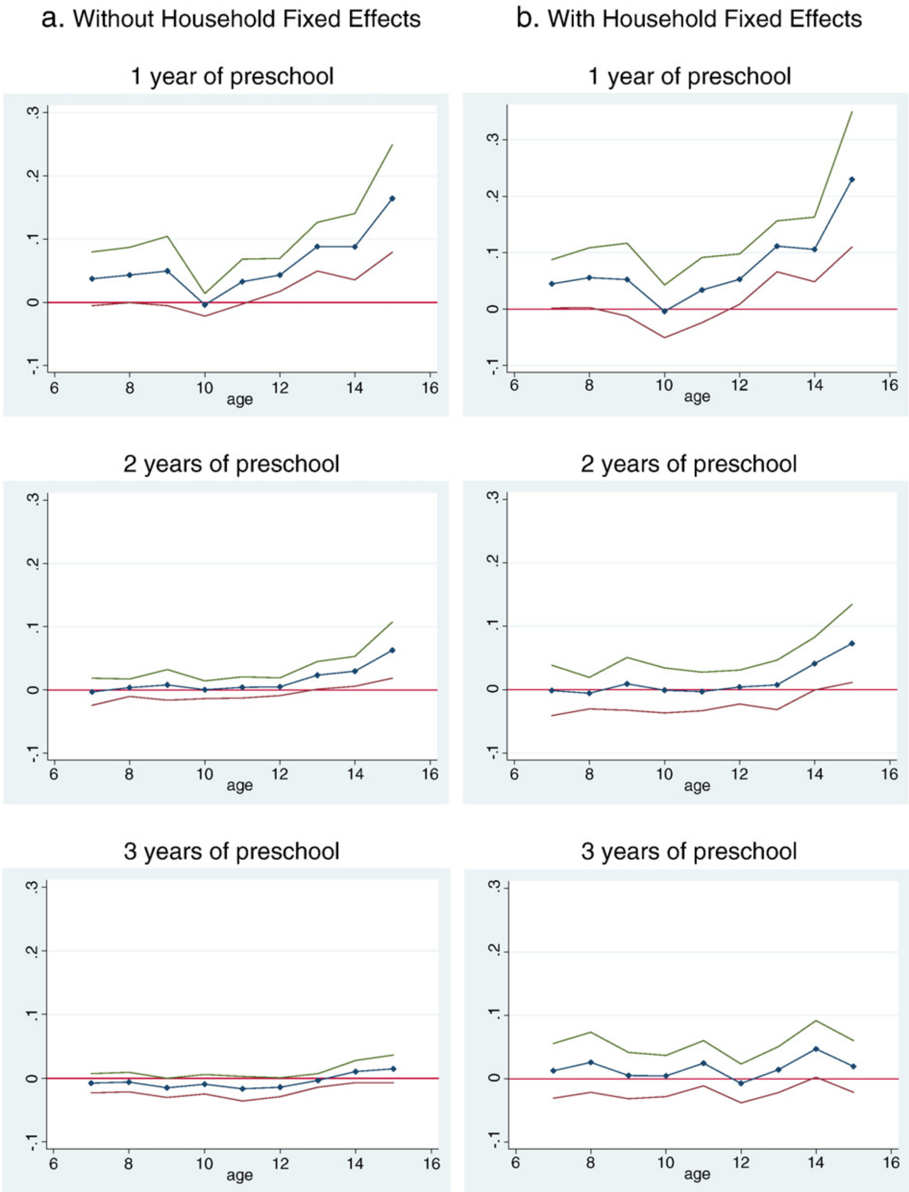

Fig. 1. The effect of additional years of preschool on school attendance by age. Source: Own calculations based on *Encuesta Continua de Hogares* 2001–2005. Notes: The graph reports the estimated effect of each additional year of preschool on years of schooling completed. In columns a. and b., respectively, we condition on the same variables as in columns (3) and (4) of Table 5. 95% confidence intervals around the estimated effects are also reported.

and high-education mothers (columns (1) and (2)), children in Montevideo compared to the rest of the country (columns (3) and (4)) and boys and girls ((columns (5) and (6)). Columns (7) to (12) report results for the same groups of children, where now the dependent variable is maximum grade completed. For brevity, we only present specifications with the entire set of additional controls and household fixed effects (as in columns (4) and (8) of Table 5) and we revert to the basic specification where we only examine the effects at the extensive margin. Interestingly, we find that preschool exposure has a much bigger impact on children whose mother is less educated, and among those living outside the relatively more affluent area of Montevideo. For example, column (1) illustrates that children of mothers with low education who were exposed to treatment are 27 p.p. more likely to be in school by age 15 compared to their siblings who did not receive treatment. This effect is only 8 p.p. for children of highly educated mothers, and

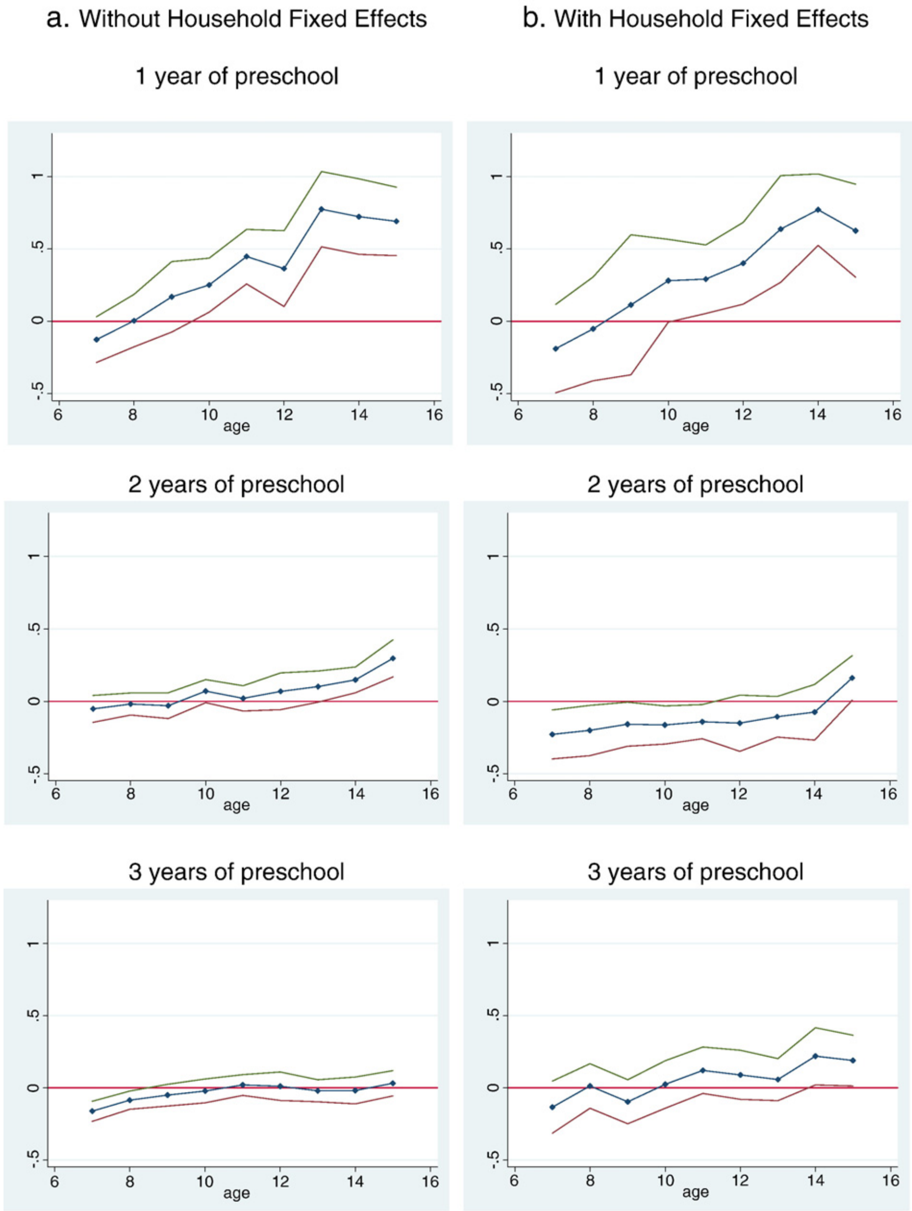

Fig. 2. The effect of additional years of preschool on years of schooling completed by age. Source: Own calculations based on *Encuesta Continua de Hogares* 2001–2005. Notes: The graph reports the estimated effect of each additional year of preschool on years of schooling completed. In columns a. and b., respectively, we condition on the same variables as in columns (3) and (4) of Table 5. 95% confidence intervals around the estimated effects are also reported.

not statistically significant. Similarly we find that at age 15 the effect of preschool exposure on stay-on rates is in the order of 34 p.p. in the rest of the country and only two thirds of this in Montevideo. This same pattern is found when one uses maximum number of years completed as a dependent variable (columns (7) to (10)).

The point estimates of preschool exposure on stay on rates are generally larger for boys than for girls, at least for those aged 11 or above. For example, at age 15 the estimated coefficients are 36 p.p. for boys and 24 p.p. for girls. However, gender differences are never statistically significant at conventional levels. This is true both if one performs separate t-tests by age and if one performs a joint F-test across different ages (11 to 15 or 14 to 15).

Table 6
The impact of preschool attendance on school attendance and years of schooling — hetherogeneous effects

	Dependent variable:											
	School attendance						Years of schooling					
	Mother's education		Area		Gender		Mother's education		Area		Gender	
	Low	High	Montevideo	Rest of country	Boys	Girls	Low	High	Montevideo	Rest of country	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Attended 1, 2 or 3 years of preschool x Age=7	0.040	0.047	0.047	0.036	0.132	0.093	−0.327	−0.256	−0.244	−0.451	−0.409	−0.181
	[0.023]*	[0.033]	[0.036]	[0.024]	[0.039]***	[0.031]***	[0.195]*	[0.243]	[0.214]	[0.217]**	[0.263]	[0.267]
Attended 1, 2 or 3 years of preschool x Age=8	0.049	0.078	0.075	0.031	0.083	0.090	−0.258	0.483	−0.242	−0.164	−0.336	0.215
	[0.027]*	[0.057]	[0.033]**	[0.036]	[0.036]**	[0.067]	[0.184]	[0.306]	[0.289]	[0.211]	[0.229]	[0.278]
Attended 1, 2 or 3 years of preschool x Age=9	0.069	0.039	0.051	0.057	0.065	0.088	0.044	0.092	0.175	−0.168	−0.081	0.261
	[0.038]*	[0.051]	[0.042]	[0.046]	[0.064]	[0.056]	[0.229]	[0.450]	[0.292]	[0.337]	[0.432]	[0.306]
Attended 1, 2 or 3 years of preschool x Age=10	−0.002	−0.003	−0.023	0.007	−0.003	0.007	0.171	0.361	−0.037	0.366	0.063	0.567
	[0.029]	[0.041]	[0.030]	[0.029]	[0.037]	[0.034]	[0.160]	[0.252]	[0.174]	[0.164]**	[0.244]	[0.279]**
Attended 1, 2 or 3 years of preschool x Age=11	0.033	0.074	0.031	0.030	0.100	−0.008	0.099	0.722	0.222	0.225	0.454	0.294
	[0.032]	[0.072]	[0.039]	[0.047]	[0.052]*	[0.052]	[0.133]	[0.291]**	[0.170]	[0.167]	[0.303]	[0.248]
Attended 1, 2 or 3 years of preschool x Age=12	0.058	0.048	0.075	0.024	0.107	0.047	0.307	0.523	0.201	0.480	0.814	0.317
	[0.030]*	[0.026]*	[0.038]*	[0.025]	[0.042]**	[0.036]	[0.162]*	[0.271]*	[0.152]	[0.179]**	[0.254]***	[0.133]**
Attended 1, 2 or 3 years of preschool x Age = 13	0.126	0.053	0.077	0.148	0.174	0.047	0.572	0.492	0.408	0.754	0.567	0.728
	[0.028]***	[0.029]*	[0.028]**	[0.036]***	[0.062]***	[0.046]	[0.217]**	[0.260]*	[0.204]*	[0.307]**	[0.307]*	[0.252]***
Attended 1, 2 or 3 years of preschool x Age=14	0.134	0.058	0.148	0.129	0.211	0.098	0.750	0.625	0.615	0.948	1.069	0.891
	[0.035]***	[0.037]	[0.038]***	[0.040]***	[0.059]***	[0.062]	[0.170]***	[0.169]***	[0.118]***	[0.171]***	[0.208]***	[0.231]***
Attended 1, 2 or 3 years of preschool x Age = 15	0.269	0.084	0.203	0.342	0.359	0.241	0.741	0.254	0.593	0.923	0.888	0.876
	[0.066]***	[0.076]	[0.083]**	[0.064]***	[0.087]***	[0.087]***	[0.216]***	[0.217]	[0.141]***	[0.266]***	[0.260]***	[0.263]***
Observations	12,069	10,973	11,043	11,999	11,840	11,202	12,069	10,973	11,043	11,999	11,840	11,202

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Notes: OLS regression. Table presents the same specifications as in columns (4) and (8) of Table 5. All controls and household fixed effects included. For a full list of controls see notes to Table 5. Standard errors clustered by locality in brackets (55 clusters). * significant at 10%; ** significant at 5%; *** significant at 1%.

Overall we find evidence of substantial heterogeneity of treatment. Not surprisingly we find larger gains for more disadvantaged children. Since, as shown, more disadvantaged children were the ones who largely benefited from the reform of preschool, this suggests that our estimates of the effect of treatment among the treated are most likely an upper bound for the average effect of treatment (i.e. in the population at large).

5.5. Public versus private schooling

One potential threat to the validity of our estimates is migration of students from the private to the public school system associated with increased preschool attendance. Because typically the expansion of pre-primary places came through the addition of preschool classrooms to existing public primary school, one possible explanation for our findings is that such expansion created incentives for children to remain in the public school system. If progression rates systematically differ between private and public schools and, in particular, if promotion rates are higher in public schools, this might explain the results found above.

To check for this we examine whether attendance to a public school is associated with exposure to preschool education. This exercise serves the additional purpose of checking for the validity of the identification assumption underlying the consistency of the within estimator, namely that household fixed effects wash out any spurious correlation between preschool exposure and latent school outcomes. Although this identification assumption is ultimately non-testable with our data, the existence of some correlation between public school attendance and preschool exposure across siblings would raise some concerns.

In Table 7, we regress a public school attendance dummy on age dummies interacted with a dummy for pre-primary education. Here we restrict to only those still in school. We reproduce the same structure as that of Table 5. Column (1), where basic controls are included, reveals a clear negative correlation between public school attendance and previous exposure to pre-primary school. It is plausible that this correlation is largely explained by the circumstance that better-off children are more likely to both attend a private school and to have attended preschool. This is confirmed in columns (2) and (3) where we include controls for children and household characteristics. Results are still negative but not significant (except in one case). Once we include household fixed effects and controls in columns (4), the effects tend to become smaller and again not significant. In sum, the results give little support to then notion that preschool exposure affects the decision to attend a public versus a private institution later in the school life.

Possibly, this evidence also suggests that our treatment variable is unlikely to be correlated with other potential reforms of the public school system. If such reforms were correlated with preschool exposure and, at the same time, they affected the incentives for children to enroll in the public system, one might expect preschool exposure to show up significantly in the public school attendance regression, which is clearly not the case. Obviously, as any other falsification exercise, this is only a one-sided test, so acceptance of the null is not automatically proof of preschool attendance being orthogonal to the unobserved component of siblings' subsequent school outcomes.

5.6. Instrumental variable estimates

As a last empirical strategy, in this section we present instrumental variable estimates aimed at controlling for selective treatment of children within households. As already discussed, one additional source of potentially invalidation of the estimates in Table 5 is that parents might accord differential treatment to some of their children based on their preferences (e.g. favoritism towards some of them), differential returns to human capital investment across siblings or just differences in household resources over the household life cycle (coupled with credit constraints). Most likely these factors will tend to lead to within household estimates that are upward biased. Controls for children's order of birth, gender and mother's age at birth go some way towards controlling for this potential differential treatment but they cannot obviously account for differential treatment based on characteristics that are unobserved to the econometrician. This problem is likely to be particularly pronounced when household fixed effects are included, since in this case one only exploits the variation in exposure and outcomes across siblings.

As a way to control for this additional source of bias, we present IV estimates where children's school attendance is instrumented by the average school attendance in the child's cohort in his locality of residence. We expect these local trends to be largely driven by changes in preschool supply rather than demand-side shocks. To compute these averages we use retrospective information on preschool exposure for all children born in the same cohort and living in the same

Table 7
The Impact of Preschool Attendance on Public School Attendance

	Dependent variable:			
	School Attendance			
	(1)	(2)	(3)	(4)
Attended 1, 2 or 3 years of preschool x Age=7	0.005	0.039	0.046	0.036
	[0.029]	[0.027]	[0.028]	[0.028]
Attended 1, 2 or 3 years of preschool x Age=8	−0.054	0.007	−0.022	0.008
	[0.034]	[0.017]	[0.034]	[0.017]
Attended 1, 2 or 3 years of preschool x Age=9	−0.024	0.031	0.004	0.030
	[0.031]	[0.014]**	[0.027]	[0.015]**
Attended 1, 2 or 3 years of preschool x Age=10	−0.047	0.009	−0.005	0.008
	[0.023]**	[0.015]	[0.023]	[0.015]
Attended 1, 2 or 3 years of preschool x Age=11	−0.092	0.007	−0.055	0.006
	[0.020]***	[0.021]	[0.020]***	[0.021]
Attended 1, 2 or 3 years of preschool x Age=12	−0.042	0.002	−0.011	0.003
	[0.019]**	[0.013]	[0.017]	[0.013]
Attended 1, 2 or 3 years of preschool x Age=13	−0.033	0.006	−0.006	0.006
	[0.021]	[0.013]	[0.017]	[0.013]
Attended 1, 2 or 3 years of preschool x Age=14	−0.056	0.002	−0.025	0.002
	[0.017]***	[0.015]	[0.016]	[0.015]
Attended 1, 2 or 3 years of preschool x Age=15	−0.029	0.002	0.003	0.003
	[0.023]	[0.015]	[0.022]	[0.015]
Observations	22,998	22,998	22,998	22,998
	Specification includes:			
Age X Cohort	Yes	Yes	Yes	Yes
Year X Month X Locality	Yes	Yes	Yes	Yes
Female, Birth Order	No	No	Yes	Yes
Mother's age at birth, Mother's Years of Education	No	No	Yes	Yes
Household fixed effects	No	Yes	No	Yes

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Notes: OLS regression. For list of controls see notes to Table 5. Standard errors clustered by locality in brackets (55 clusters). * significant at 10%; ** significant at 5%; *** significant at 1%.

locality independent of the year (2001–2005) in which they are observed.[11,12] Identification is warranted by the interaction of cohort and locality, which is excluded from the main equation. By exploiting the area specific variation in preschool enrollment across cohorts we effectively control for children's unobserved traits that might be correlated with the outcome and treatment variable.[13]

We revert to the basic specification with homogenous effects across groups and again we concentrate only on the effect at the extensive margin. We do so since the IV estimator is inevitably leading to a loss in precision and we are unable to estimate precisely a large number of cross effects.

We report the first stage estimates in Table A1 in the appendix. For brevity we only report results with the entire set of additional controls and household fixed effects (additional results are very similar and available upon request). Each column refers to the probability of having attended preschool at a given age (e.g. age 7 in column (1), age 8 in column (2), etc.) on the average preschool attendance by cohort and locality interacted with age. The first stage estimates illustrate that locality-cohort enrollment is a very good predictor of the individual probability of attending.

In Table 8, which has the same structure of Table 5, we present the instrumental variable estimates. The effect of the treatment on stay-on rates, columns (1) to (4), does not show such a clear pattern as the one reported in Table 5.

[11] Recall that there are 55 localities and 13 cohorts in the sample, with an average of 32 children by cohort and locality.
[12] We also tried to compute these means excluding the child of interest. Results are unchanged.
[13] Potentially a better instrument would only exploit differences in the local supply of preschool places across cohorts. Unfortunately we do not have detailed information on preschool construction at such a detailed geographical level.

Table 8
The impact of preschool attendance on school attendance and years of schooling completed — instrumental variable estimates

	Dependent variable:							
	School attendance				Years of schooling			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended 1, 2 or 3 years of preschool x Age=7	0.039	0.066	0.041	0.076	−0.217	−0.382	−0.357	−0.273
	[0.057]	[0.071]	[0.056]	[0.070]	[0.295]	[0.465]	[0.274]	[0.409]
Attended 1, 2 or 3 years of preschool x Age=8	0.182	0.181	0.176	0.199	−0.138	−0.082	−0.303	−0.090
	[0.070]**	[0.117]	[0.067]**	[0.110]*	[0.440]	[0.575]	[0.456]	[0.570]
Attended 1, 2 or 3 years of preschool x Age=9	0.078	0.119	0.083	0.115	0.356	0.451	0.140	0.379
	[0.078]	[0.119]	[0.078]	[0.116]	[0.414]	[0.745]	[0.428]	[0.735]
Attended 1, 2 or 3 years of preschool x Age=10	0.047	−0.018	0.047	−0.018	0.304	0.162	0.274	0.054
	[0.061]	[0.103]	[0.061]	[0.104]	[0.402]	[0.514]	[0.378]	[0.517]
Attended 1, 2 or 3 years of preschool x Age=11	0.090	0.037	0.094	0.029	0.713	0.447	0.603	0.350
	[0.049]*	[0.105]	[0.049]*	[0.106]	[0.342]**	[0.460]	[0.326]*	[0.456]
Attended 1, 2 or 3 years of preschool x Age=12	0.058	0.019	0.048	0.007	0.796	0.506	0.743	0.446
	[0.057]	[0.112]	[0.057]	[0.113]	[0.507]	[0.483]	[0.474]	[0.492]
Attended 1, 2 or 3 years of preschool x Age=13	0.023	−0.001	0.018	−0.019	0.719	0.806	0.649	0.691
	[0.056]	[0.078]	[0.057]	[0.080]	[0.378]*	[0.484]	[0.373]*	[0.457]
Attended 1, 2 or 3 years of preschool x Age=14	0.116	0.071	0.116	0.058	0.820	0.955	0.868	0.871
	[0.072]	[0.085]	[0.071]	[0.085]	[0.295]***	[0.381]**	[0.283]***	[0.389]**
Attended 1, 2 or 3 years of preschool x Age=15	0.307	0.410	0.298	0.398	1.242	1.049	1.120	0.921
	[0.080]***	[0.114]***	[0.081]***	[0.112]***	[0.444]***	[0.552]*	[0.448]**	[0.553]
Observations	23,042	23,042	23,042	23,042	23,042	23,042	23,042	23,042
	Specification includes:							
Age X Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year X Month X Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female, Birth Order	No	No	Yes	Yes	No	No	Yes	Yes
Mother's age at birth, Mother's Years of Education	No	No	Yes	Yes	No	No	Yes	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes	No	Yes

Source: Own calculations based on Encuesta Continua de Hogares 2001–2005.
Notes: OLS regression. For list of controls see notes to Table 5. Standard errors clustered by locality in brackets (55 clusters). * Significant at 10%; ** significant at 5%; *** significant at 1%.See also notes to Table 5.

However, we still find large differences between treated and untreated children at ages 14 and 15. We find a monotonic effect of the treatment on completed years of schooling that is very similar to the OLS estimates (Columns (5) to (8)). Although the IV estimates tend to be rather imprecise, these exercises suggest that parental differential treatment of their offspring does not appear to be biasing our results.

Of course, the instrumental variable strategy is of no help in disentangling the effects of pre-primary education on the outcomes of interest from the effect of other interventions which may be correlated with average locality-cohort variability in preschool attendance. As a further robustness check, we restrict the sample to siblings with at most 4 years of difference in age. The idea is that the smaller the age difference between siblings, the most likely is that they have been exposed to similar experiences in primary and secondary school. The results of analogous models to those estimated in Table 5 for this sub-sample are reassuringly similar to the results for the whole sample. (Results are available upon request from the authors).

6. Discussion

Regression results in Tables 5–8 show unequivocally an increasing gap between treated and untreated siblings as they age. In this section we investigate further the dynamics of these diverging paths, and we show that some delay early in school life has permanent consequences that reverberate throughout adulthood.

The ECH data do not allow us to identify the precise mechanism through which small initial differences tend to be exacerbated as children grow older since – as said – they do not contain information on the time when repetition occurred and paths of state dependence in grade failure. To investigate this issue, we use data from the 2001 ECH education module, that collects retrospective information on education histories for a sub-sample of around 3,500 individuals aged 12–29.[14] Using information on repetition at each grade and school starting age, for each individual in the sample we estimate years of completed education at each age between 6 and 11.[15] Similarly to our main data, this auxiliary data set illustrates some increasing delay in progression among untreated children. By age 11 preschoolers have accumulated around 0.24 additional years of education relative to non-preschoolers. This is exclusively due to higher repetition among the latter group rather than differential starting ages or intermittent attendance rates (see below). We have computed how much of this gain can be explained by lower repetition rates in the early years of schooling. Our calculations (available upon request) show that differences in repetition in first grade between treated and untreated individuals explain almost 50% of the observed educational gap by age 11. Differences in repetition in the first three grades explain more than 80% of the observed gap. This simple exercise suggests that the increase in schooling attainment caused by preschool attendance is likely to be generated by a reduction in the probability of repetition early in school life that gets compounded by the state dependence in grade failure as time goes by. This might be due to grade failure early in life lowering expectations, inducing stigmatization or disenfranchisement among children, their families or teachers, in turn leading to further grade failure and incentives for early drop-out. [16]

One second caveat to our conclusions is that stay-on rates and school progression at age 15 may not necessarily be good predictors of later academic success. If school drop-out is only temporary or children make up for lost school years later in life, our outcomes variable might be of little interest. The 2001 ECH education module contains a question on whether a child ever temporarily dropped out of school (and if yes, at what age(s)). Although, these data show a higher drop-out rate among untreated individuals relative to treated individuals,[17] temporary drop-out is similar across groups, ruling out that this is responsible for the worse school performance of untreated children.[18]

We finally investigate the correlation between primary school completion, a straightforward measure of school progression among pre-teenagers and teenagers, and a number of later outcomes.[19] In Table A2 in the appendix we use the 2001–2005 ECH data to run regressions similar to the ones in Table 5, where the dependent variable is now the probability of completing primary school (columns (1) to (4)) or the probability of being a primary school dropout (i.e. not in school and incomplete primary, columns (5) to (8)). It should be no surprise that even along these dimensions, treated individuals do better than untreated individuals and this is increasingly true as they age. While no differences can obviously be detected at age 7, by age 15, preschoolers are 24 p.p. more likely to have completed primary school and 8 p.p. less likely to have dropped out without completing primary school.

In row 1 of Table 9 we use the 2001 ECH education module to correlate later educational and labor market outcomes among individuals aged 25–29 to the age the individual ended primary school (row 1) or a dummy for whether the

[14] The education module reports information on school starting age, school completion age and whether the individual ever dropped temporarily out of school and – if he did – at what age. Unfortunately, the data do not provide information in the age at which temporary drop-outs returned to school. To be as conservative as possible, we assume that temporary drop-outs only stay out of school for one year only.

[15] We do not go past age 11, since identifying repetition past primary school is complicated with these data. After primary school individuals can follow different tracks. Indeed, a non negligible proportion of individuals appear to have followed at least two tracks but the data do not allow us to recover their precise time sequence.

[16] If a child's preschool attendance is associated to higher mother's labor supply, and if mother's earnings benefit more children currently of preschool age, then our estimates also encompass this additional channel. Unfortunately, our data do not provide information on mother's employment/unemployment/inactivity or hours of work other than at the time of observation and therefore it is not possible to describe the relationship between maternal labor supply and preschool attendance at the time the child was of preschool age. Evidence for both the USA (Gelbach, 2002) and South America (Attanasio and Vera-Hernandez, 2007; Berlinski and Galiani, 2007; respectively for Colombia and Argentina) though points towards small effects of preschool attendance on maternal labor supply. In addition, it is unlikely that only children of preschool age benefit from increased mother's labor supply. So, although we cannot rule out this additional channel, this seems unlikely to fully explain our results.

[17] At age 15, these are in the order of 16% for untreated individuals and less than 9% for treated individuals.

[18] At age 15, this is in the order of 3% for both groups.

[19] The education module shows that only 1% of individuals older than 15 are still in primary school (whether they have completed it or not).

Table 9
Early school delay and subsequent school outcomes. Individuals aged 25–29

	Dependent variable:				
	In school	Years of education	Work	Labor income	Observations
	(1)	(2)	(3)	(4)	
Age completed primary school	−0.076	−1.737	−0.059	−1,012.98	803
	[0.012]***	[0.125]***	[0.019]***	[160.388]***	
Completed primary school	−0.198	−6.997	−0.24	−2,455.05	831
	[0.018]***	[0.281]***	[0.086]***	[333.869]***	

Source: Own calculations based on Encuesta Continua de Hogares 2001 (Education Module).
Notes: OLS regression. Each cell refers to a separate regression. Entries report the coefficient from a regression of the dependent variable (in school, years of education, in work, labor income) on age completed primary school among those who eventually completed (row 1) and on a dummy for primary school completion (row 2). Additional controls include age and sex dummies. Data refer to individuals aged 25–29. Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

individual is a primary school drop-out (row 2). All regressions include additive age and sex dummies. Results are unaffected by controls for school starting age. Column (1) refers to the probability of currently attending an educational institution. A one year delay in primary school completion is associated to an 8 p.p. lower probability of attending an educational institution past age 24. Among those who dropped out of primary school, current attendance is 20 p.p. lower. The following column refers to years of education. Since, as seen, those individuals that finish primary school earlier are more likely to currently attend an educational institution, if anything, these differences tend to underestimate the educational disadvantage of early repeaters. Even so, one additional year of delay in primary school completion is associated to 1.73 less years of education past age 25. Among primary school drop-outs, average years of education are 7 years lower. Columns (3) and (4) report the same regressions for the probability of working (at least one hour last week) and total earnings (including zeros) from labor. One additional year of delay in completing primary school is associated to a lower employment probability of around 5.9 p.p. and lower monthly earnings in the order of 1,013 Uruguayan Pesos (around US$43, at today's exchange rate, approximately a 25% fall in earnings). Again, we find an even larger penalty among early drop outs. In sum, even if one has to be cautious in attaching any causal interpretation to the results in Table 9, it appears that early academic delay is associated to an array of negative outcomes later in life. Early underperformers, tend to end up with less education, and to do worse in the labor market relative to individuals who happened to progress timely in primary school. Preschool has the potential of leading to lifetime gains.

7. Comparing benefits and costs

In the previous section we estimated that by age 15 children who attended preschool have accumulated 0.79 additional years of education (Table 5, column (4)) and are 27 p.p. more likely to be in school (Tale 5, column (8)) relative to their non-treated siblings. We can now use these estimates to compare the costs of offering one year of pre-primary education to the returns from this intervention. We assume that our estimates extend to all treated children and that the general equilibrium effects of this intervention are ignorable.

For simplicity, consider an intervention that consists in providing one year of public pre-primary education to a group of 50 students aged 5. This is equivalent to the construction of one classroom, since each classroom can accommodate 25 students in each shift. We measure the benefits and costs of the project into Uruguayan Pesos as of March 1997 (the exchange rate of March 1997 was 9.02 UY$ per US$).

The direct cost of this intervention encompasses the cost of constructing a new classroom, teachers' wages and other miscellaneous costs. The cost of building a pre-primary classroom is UY$ 315,700 (ANEP, 2000). A given cohort has to bear only a portion of this cost because the classroom will be utilizable by subsequent cohorts of students. We assign to the project the annual payment needed to cancel a loan of this amount in 25 years assuming an interest rate of 10% per year. Therefore, construction costs attributable to the project are calculated as 12 monthly payments of UY

$ 2774.03. This assumption might be conservative since it implies that we fully depreciate the investment in 25 years. The spot price of the land over which the classroom is built is assumed to be UY$ 45,100. We also assign to the cost of the project the interest over the value of the land using an annual rate of 10%, which amounts to 12 monthly payments of UY$ 359.71.

We estimate the average monthly wage of schoolteachers using microdata from the ECH for the period 1992–1999. The monthly wage of a teacher that we assume attends both shifts is UY$ 4460.22. Additionally, we estimate miscellaneous monthly costs in UY$ 2230.11 (i.e. 0.5 of the monthly cost of a school teacher).

There are also other costs associated to acquiring higher education. Since, as said, preschool induces children to stay longer in school, these students will consume resources while individuals of the same cohort in the labor market might be contributing to the production of goods and services. We measure this cost by assuming that each year of school costs the same as one year of preschool. Since by age 15 treated children are 27 p.p. more likely to be in school, this means that 10 years after the intervention, there is an additional cost that needs to be imputed to the project equal to 0.27 times the total cost of the first year of the program. We do this same computation for children aged 6 to 14. In each case, costs are appropriately discounted. Second, we estimate the opportunity cost of attending school. We estimate that 27% of the children that are now in school would have been in the labor market in the absence of the intervention. A certain portion of them would have been actually employed. Therefore, we estimate the opportunity cost of the forgone labor income as the probability of being employed (0.7) times the proportion of children in school as a result of the intervention (i.e. 0.27 at age 15) times the mean income for children of each age (i.e. UY$ 1209 at age 15) times 50 children. We assume that only children 14 or older participate in the labor market. Again, these costs are appropriately discounted.

We now turn to the benefits of this policy. The higher educational achievement induced by preschool should translate into higher productivity and wages later in life. In the absence of reliable estimates on the non-pecuniary returns to education, we ignore other long run benefits associated to education in general, and early interventions in particular, such as a lower crime rate, higher tax revenues and lower welfare payments (Belfield et al., 2006; Schweinhart et al., 1993). We also ignore the effect of education on the probability of being employed, although this might be non negligible. If anything we err on the side of underestimating the benefits of this policy.

By age 15 each of these 50 children will have accumulated 0.79 more years of education. We assume that this difference will be maintained beyond age 15. This is likely to be a conservative estimate, since we have argued above that the effect of treatment appears to extend well beyond age 15. Estimates of the economic returns to one additional year of education for Uruguay are always above 10% (Sanroman, 2006; and references therein). These compare to "consensus" estimates of the economic returns to one additional year of education in other countries that range from 6.8 to 12% (Duflo, 2001; Schultz, 1999; Parker, unpublished). To be on the safe side, we adopt an annual rate of return to education of 10%. Therefore, a treated individual will earn 7.9% more per year than an untreated one. We further assume that all individuals still in school by age 15 enter the labor market at age 16 and we calculate the benefits stemming from the intervention using average age-earnings profiles estimated from the ECH between 1992 and 1999.

Based on these figures, we estimated an annual internal rate of return of the intervention of 16.1%, and a Benefit–Cost ratio for this project that ranges from 19.1 for a discount rate of 3% to 3.2 for a discount rate of 10%. These results are commensurate to those found for other early child interventions.[20]

We have conducted some sensitivity analysis on these Benefit–Cost ratios. Before, we assumed that one teacher works two shifts. However, it might be that even if they work for 8 hours per day, due to collective agreements or other rules, they only work one shift. In this case, the internal rate of return drops slightly to 14% and the benefit–cost ratio varies between 13.1 and 2.2 depending on the discount rate. Second, we assume that the return to one extra year of education is 8% instead of 10%. The internal rate of return drops to 14.7% and the benefit–cost ratio varies between 15.2 and 2.5, again depending on the discount rate.

[20] Benefit-Cost ratios for the Perry preschool program range from 6.87 to 16.14 for annual discount rates of 7% and 3%, respectively (Schweinhart et al., 2005). The Chicago Child−Parent Center Program exhibits ratios ranging from 4.3 to 7.14 for discount rates of 7 and 3% (Reynolds et al., 2002.). Finally, the ratios for the Abecedarian project are estimated to be between 1.45 and 3.78 (Masse and Barnett, 2003).

In sum, our data suggest that this policy intervention is highly cost-effective. Under the most conservative scenarios, we find an estimated rate of return to the expansion of preschool as high as 14% and Benefits–Cost ratios greater than 2.2.

8. Conclusions

This paper uses micro data from the Uruguayan *Encuesta Continua de Hogares* (ECH) to study the short and medium term effects of preschool attendance on school progression among children aged 7–15. We use a rather unique feature of the data that collects retrospective information on the number of years of preschool attended to estimate the impact of this variable on school stay-on rates and the number of school years completed. A major government intervention aimed at universalizing pre-primary education warrants sufficient variation in the data to identify precisely the effects of interest.

A major challenge in identifying the causal effect of preschool exposure on subsequent school progression stems from the difficulty of distinguishing between unobserved heterogeneity – whereby better-off or more able children are both more likely to attend preschool and to perform better in school – and state dependence, that is the effect of interest. In order to control for such source of heterogeneity we compare school progression of siblings with different exposure to preschool. To the extent that most of the heterogeneity in preschool exposure and school attainment is ascribable to household characteristics that are common to all siblings, this strategy leads to consistent estimates of the effect of interest.

In order to control for the potential confounding effects of concurrent government interventions in the area of education, we condition in the model for the interaction of cohort-age effects plus unrestricted time dummies interacted with locality dummies. Identification is warranted by the differential cohort trends between children residing in different localities, once local area shocks that are common to all children in the same locality independent of their cohort of birth are taken into account. To the extent that concurrent government reforms affected children in different cohorts and localities similarly (or in fashion that is uncorrelated with the expansion of preschools), our estimates should be consistent.

Finally, to address the potential concern that differential treatment of siblings within households might translate into an additional source of spurious correlation between treatment and outcomes, we present alongside instrumental variable estimates that use average preschool enrollment by locality and birth cohort as an instrument for each child's exposure.

Our results show a significant positive effect of preschool attendance on the number of years of schooling completed since very early ages. Already by age 12 treated individuals show an advantage in terms of completed education in the order of 0.50 years. As time goes on, the difference in attainment between children who attended preschool and those who did not increases, and the two groups follow eventually starkly diverging paths. By age 15, treated individuals have accumulated around 0.79 more years of education compared to their non treated siblings. We also find evidence that untreated individuals are more likely to drop out of school compared to treated individuals. By age 15 children who attended preschool are 27 p.p. more likely to be in school. Because our observations are right censored given that most children are still in school by age 15, these are presumably conservative estimates of the effect of preschool on subsequent stay-on rates. This is confirmed by complementary evidence from the 2001 ECH education module. Instrumental variables estimates lead to qualitatively similar conclusions although admittedly the point estimates are less precise.

We find substantial heterogeneity in the effect of treatment. In particular, it is children whose mother has lower than average education that appear to largely benefit from exposure to preschool. This is also the group that largely benefited from the expansion of the pre-primary school system in terms of increased preschool attendance. One should hence be cautious in extending these point estimates to the population at large.

We use auxiliary data to identify the mechanism through which small initial differences tend to be exacerbated as children grow older. The data show that the initial penalty suffered by children who did not attend preschool gets compounded by the state dependency in grade repetition. Preschool has the potential to revert this trend. Compulsory education increases the length of the school cycle at an age where the opportunity costs of attending school are arguably low and the potential returns from it apparently very high. Public provided preschool education hence appears as a very effective policy option in countries where the system is unable to retain a large number of children and teenagers into the system, as it is the case in many developing countries. A cost–benefit analysis shows that this policy is also cost effective. Even under the most conservative scenarios, we estimate a rate of return to investing in preschools as high as 14% and Cost–Benefit ratio of at least 2.2.

Appendix

Table A1
Individual pre-school attendance and average pre-school attendance by cohort and locality – first stage estimates separate estimates by age – household fixed effects and controls included

	Dependent variable: individual attendance								
	X Age=7	X Age=8	X Age=9	X Age=10	X Age=11	X Age=12	X Age=13	X Age=14	X Age=15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Average attendance by cohort and locality									
x Age=7	1.342	−0.128	−0.102	−0.094	−0.039	−0.096	−0.115	−0.046	−0.158
	[0.156]***	[0.045]***	[0.057]*	[0.048]*	[0.028]	[0.080]	[0.078]	[0.038]	[0.076]**
x Age=8	−0.043	1.125	−0.007	−0.169	−0.177	−0.047	−0.085	−0.087	0.090
	[0.038]	[0.143]***	[0.032]	[0.069]**	[0.080]**	[0.034]	[0.071]	[0.069]	[0.069]
x Age=9	−0.023	−0.044	1.080	−0.083	−0.199	−0.129	0.024	−0.069	−0.074
	[0.043]	[0.025]*	[0.170]***	[0.033]**	[0.090]**	[0.075]*	[0.057]	[0.050]	[0.048]
X Age=11	−0.018	−0.087	−0.039	1.236	−0.130	−0.203	−0.111	−0.080	−0.091
	[0.036]	[0.033]*	[0.034]	[0.138]***	[0.046]***	[0.097]**	[0.051]**	[0.065]	[0.044]**
X Age=12	−0.013	−0.032	−0.072	−0.077	0.969	−0.037	−0.076	−0.058	−0.072
	[0.024]	[0.033]	[0.034]**	[0.035]**	[0.156]***	[0.036]	[0.074]	[0.059]	[0.041]*
X Age=13	−0.020	−0.057	−0.076	−0.064	−0.013	0.923	−0.020	−0.067	−0.015
	[0.024]	[0.027]**	[0.039]*	[0.044]	[0.023]	[0.106]***	[0.037]	[0.038]*	[0.039]
X Age=14	−0.023	−0.054	0.018	−0.094	−0.070	−0.068	1.123	−0.019	−0.043
	[0.019]	[0.027]**	[0.015]	[0.037]**	[0.040]*	[0.029]**	[0.106]***	[0.025]	[0.039]
X Age=15	−0.028	−0.063	−0.042	−0.058	−0.031	−0.147	−0.053	1.266	−0.052
	[0.024]	[0.024]**	[0.035]	[0.031]*	[0.036]	[0.066]**	[0.035]	[0.142]***	[0.024]**
X Age=16	−0.050	−0.021	−0.011	−0.083	−0.070	−0.114	−0.018	−0.032	1.197
	[0.026]*	[0.022]	[0.024]	[0.035]**	[0.030]**	[0.052]**	[0.049]	[0.028]	[0.127]***
Observations	23,042	23,042	23,042	23,042	23,042	23,042	23,042	23,042	23,042
	Specification includes:								
Age X Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year X Month X Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female, Birth Order	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother's age at birth, Mother's Years of Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Own calculations based on *Encuesta Continua de Hogares* 2001−2005.
Notes: OLS regression. For the definition of variables see Table 1. Standard errors clustered by locality in brackets (55 clusters). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2
The impact of preschool attendance on primary school completion

	Dependent variable:							
	Completed Primary				Primary School Drop−out			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended 1, 2 or 3 years of preschool x Age=7	−0.006	−0.028	−0.015	−0.009	−0.036	−0.030	−0.036	−0.033
	[0.010]	[0.033]	[0.012]	[0.029]	[0.020]*	[0.013]**	[0.019]*	[0.012]***
Attended 1, 2 or 3 years of preschool x Age=8	−0.005	−0.046	−0.020	−0.031	−0.048	−0.035	−0.047	−0.036
	[0.010]	[0.034]	[0.011]*	[0.034]	[0.024]*	[0.015]**	[0.022]**	[0.015]**
Attended 1, 2 or 3 years of preschool x Age=9	0.001	−0.037	−0.012	−0.032	−0.053	−0.037	−0.052	−0.038
	[0.007]	[0.028]	[0.009]	[0.026]	[0.027]*	[0.032]	[0.027]*	[0.031]
Attended 1, 2 or 3 years of preschool x Age=10	−0.009	−0.018	−0.025	−0.018	0.003	0.007	0.003	0.006
	[0.008]	[0.021]	[0.009]***	[0.020]	[0.006]	[0.015]	[0.006]	[0.016]

Table A2 (*continued*)

	Dependent variable:							
	Completed Primary				Primary School Drop−out			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended 1, 2 or 3 years of preschool x Age=11	−0.016	−0.076	−0.037	−0.079	−0.042	−0.028	−0.042	−0.028
	[0.009]*	[0.024]***	[0.010]***	[0.025]***	[0.017]**	[0.022]	[0.017]**	[0.022]
Attended 1, 2 or 3 years of preschool x Age=12	−0.011	−0.031	−0.033	−0.051	−0.029	−0.014	−0.026	−0.013
	[0.020]	[0.030]	[0.021]	[0.031]	[0.014]**	[0.017]	[0.014]*	[0.017]
Attended 1, 2 or 3 years of preschool x Age=13	0.207	0.220	0.186	0.199	−0.074	−0.072	−0.074	−0.071
	[0.029]***	[0.047]***	[0.027]***	[0.045]***	[0.016]***	[0.019]***	[0.016]***	[0.020]***
Attended 1, 2 or 3 years of preschool x Age=14	0.239	0.297	0.220	0.272	−0.056	−0.052	−0.054	−0.050
	[0.033]***	[0.038]***	[0.031]***	[0.037]***	[0.015]***	[0.021]**	[0.015]***	[0.021]**
Attended 1, 2 or 3 years of preschool x Age=15	0.252	0.261	0.227	0.242	−0.062	−0.084	−0.059	−0.083
	[0.041]***	[0.058]***	[0.039]***	[0.058]***	[0.015]***	[0.019]***	[0.014]***	[0.018]***
Observations	23,042	23,042	23,042	23,042	23,042	23,042	23,042	23,042
	Specification includes:							
Age X Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year X Month X Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female, Birth Order	No	No	Yes	Yes	No	No	Yes	Yes
Mother's age at birth, Mother's Years of Education	No	No	Yes	Yes	No	No	Yes	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes	No	Yes

Source: Own calculations based on *Encuesta Continua de Hogares* 2001–2005.
Notes: OLS regression. For list of controls see notes to Table 5. Standard errors clustered by locality in brackets (55 clusters). * significant at 10%; ** significant at 5%; *** significant at 1%.

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