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Giving Children a Better Start: Preschool Attendance and School-Age Profiles

Samuel Berlinski*
University College London
and Institute for Fiscal Studies

Sebastian Galiani
Washington University
in St. Louis

Marco Manacorda
Queen Mary University of London,
CEP (LSE) and CEPR

Abstract: The authors 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 magnify 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 control for non random selection of siblings into pre-school lead to similar results. We speculate that early grade repetition harms subsequent school progression and that pre-primary education appears as a successful policy option to prevent early grade failure and its long lasting consequences.

JEL: I2, J1
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* Samuel Berlinski, Department of Economics, University College London, Gower Street, London WC1E 6BT, UK, s.berlinski@ucl.ac.uk. Sebastian Galiani, Department of Economics, Washington University in St Louis, Campus Box 1208, St Louis, MO 63130-4899, US, galiani@economics.wustl.edu. Marco Manacorda, CEP, London School of Economics, Houghton Street WC2A 2AE, London, UK, m.manacorda@lse.ac.uk. We thank the World Bank for financial support and Andres Peri, Emiliana Vegas, and Andrea Vigorito for helpful comments.

Introduction

This paper estimates the effect of 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 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 children from more disadvantaged backgrounds 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 both through a fall in retention rates since the early school years (from age 11 onwards) and a reduction in drop out rates among teenagers (from age 13 onwards). 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 speculate that early grade retention increases the incentives for early drop out and raises the probability of grade failure later in the school life and hence, pre-primary education 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 retention rates and eventually early drop out (UNESCO, 2005).

In Uruguay as in many other Latin American countries (UNESCO, 2005) 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, about 25% of 24-29 years old declare 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 (2002) 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 (see, among others, Lee et al. 1990, Barnett, 1993, Barnett, 1995, Currie and Thomas 1995, Reynolds 1998, Karoly et al., 1998, Danzinger and Waldfogel, 2000; Currie, 2001, Garces et al. 2002, Blau and Currie 2004, and Schweinhart 2005), much less is known about the benefits of expanding pre-primary education for the population as a whole. Cascio (2004) finds that the expansion of kindergarten financing in the late 60s and early 70s in the Southern and Western States of the US reduced subsequent grade repetition relative to Northern States. Using data from the Early Childhood Longitudinal Study, Magnuson et al. (2005) 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 by the end of first grade. They also find that pre-primary education in the US is associated with higher levels of behavioral problems, especially when pre-kindergartens are not located in public schools. Exploiting a natural experiment, Berlinski et al. (2006) find a positive effect of pre-primary school attendance on third grade standardized Spanish and Mathematics test scores in Argentina. They also find that pre-primary school attendance positively affects 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 across households. 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 most likely to be non-ignorable in identifying the effect of pre-primary education on subsequent school progression. 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), Currie and Thomas (1999) and Garces, et al. (2002) who examined 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 successfully control 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 pre-school and to perform better in school or stay-on longer. To tackle this second threat to the identification of the effects of interest we rely on a variety of approaches. First, we control for some of this 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 1 provides background information on the Uruguayan school system and the educational reform of the 1990s that led to a rapid acceleration in preschool enrollment rates. Section 2 describes the data. Section 3 lays the empirical strategy and discusses the identification strategy. Section 4 presents the regression results and Section 5 finally concludes.

1. Background

Uruguay is a relatively small middle-income country (GDP per capita was US$ 4,800 in 2005, IMF 2005). Although Uruguay is an early starter in the process of development, over the second half of last century the country has grown at a slow rate. While per capita GDP in 1870 was approximately equal to the contemporaneous per capita income in the USA, in 1920 this figure was about 50% and by the end of the last century this was around 30% (Maddison, 2004).

Uruguay boasts 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.[1] 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, 2006). 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 illustrates 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),

[1] Private schools do not receive subsidies from the government besides national and municipal tax exemptions.

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. In the age group 24-29 around 20% of individuals declare never having started junior high school. Among those who started this school cycle around 16% declare not having completed it. 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 schools 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.[2] The hope was that this policy 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 preschool provision in public primary schools. By 1999, 414 new classrooms had been built (or made available via refurbishment). 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%.

2. 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.

[2]The preschool curriculum was explicitly designed with the objectives of promoting both a child's socialization and alphabetization (ANEP, 2000).

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 off 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.[3]

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 five 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 attendance is in the order of 97%, not far from universal although – as shown below – this masks substantial heterogeneity across age groups. On average, mothers have completed 10 years of schooling and mean mother's age at birth is 28.5.

In Table 2, we report the proportion of children attending primary school or above 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 and no information on birth date is available we concentrate on the months of January to April of the survey for the completed school grade statistics.[4] If entry into primary school were timely (at age 6) and transition from grade to grade were normal in Uruguay, children aged 7 during the interview months of January to April

[3] By age 16, 1% of individuals live outside their parental home (i.e. they classified as heads, spouses, non relatives or

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 the children are lagging behind at age 9. The first row of the table also illustrates rapidly growing drop out rates from age 12 onwards. While, until age 11, school attendance is almost universal (99%), at age 12 this is 98% and 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 percentage points 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.

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 percentage points. As time goes by, an increasing proportion of

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domestic employees). By age 20 this proportion rises to around 12%.

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 cohort, i.e. the cohort supposedly entering pre-primary school (at age 5) in 1997. Notice that this is exactly 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 acceleration in pre-school 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 get a grasp of 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 preprimary education respectively. The sum of these proportions, reported in column (4), gives an estimate of the number of years of pre-school completed.[5] We report the same statistics based on retrospective information from the 2001 to 2005 data in columns (5) to (8). If

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

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

3. 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

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).

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]

The inclusion of this large set of controls goes a long way towards eliminating potential confounders that might lead to inconsistent OLS estimates of the effect of preschool exposure on subsequent school outcomes. As it is normally the case, besides the expansion of the preschool system, the Government of Uruguay underwent some other educational interventions during the mid 1990s-mid 2000s. 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 biased. 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 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. Our model is:

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

where X_i is a vector of observed child's characteristics (including - but not limited to in some specifications- cohort dummies interacted with age dummies) and where X_j, $i \in j$, is a vector of household characteristics (including - but not limited to in some specifications - locality dummies

[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.

interacted with time dummies). We are interested in the vector of parameters β_{1a} which measure the effect of attending at least one year of preschool on school attainment at age a.

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, levels of 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 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, a second strategy we propose is to 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:

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

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, this is unable to account for any spurious correlation within households, i.e. across siblings. Parental preferential treatment of some of 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 pre-school enrolment by cohort in each of the 55 localities in the ECH as an instrument for a child's pre-school 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 of great policy interest, to analyze the effect of exposure at the intensive margin. This is to say, what is the value added in terms of school progression of going to preschool for one, two or three years. In the results 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.

4. Regression results

4.1 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 dummy for preschool attendance whose coefficient we allow to vary by age. 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, gender dummies, 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). Standard errors in these and all other regressions are clustered by locality.

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 percentage points, by age 15 this difference is in the order of 21 percentage points and statistically significant.

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%, 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

[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

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. Omission of parental characteristics leads potentially to downward biased estimates of the effect of interest while the variation among "compliers" leads to treatment effects that are larger than the ones to be found in the population at large. The second effect prevails so that the within household estimates happen to be slightly larger than the OLS ones.

The inclusion of children's characteristics such as order of birth, gender and sex (plus 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 20 percentage points, 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 that the estimates in column (1) are statistically different from those in column (2) to (4) and they show a roughly monotonically increase in stay-on rates among those who attended preschool that leads to a gain of between 20-28 percentage points in stay-on rates by age 15.

4.2 Preschool attendance and educational attainment

Although we have documented that preschool attendance is associated to a higher stay-on rate among teenagers, little is known about 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 happened to stay in school longer as a result of treatment were 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 overall educational attainment.

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 a basic set of control variables are included, shows that by age 8 children that attended preschool have already accumulated 0.17 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. 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 (4). Similarly the inclusion of additional controls reduces slightly further the estimated coefficients (see columns (7) and (8)). For example, when both household fixed effects and additional controls are included, we find that by age 15 treated individuals have around 0.79 additional years of education relative to untreated individuals.[8] [9]

To put this magnitude in context, given that we find an overall rise in preschool attended between the first (1986) and last (1998) cohort of around 13 percentage points, our model implies an overall increase in the average level of education of 15 years old of around 0.10 years (0.13 X 0.79) and a rise in school participation of around 3 percentage points (0.13 X 0.27). This is in the face of substantially stable stay-on rates and educational attainment of 15 years across subsequent cohorts.

[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 (education and number of children) give essentially the same results.

[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). If one takes the household fixed effect regressions, these imply that by age 12 children who did not attend preschool have accumulated just below a third of a year of delay. 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

4.3 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 whether there are 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 for different years of preschool (1, 2 and 3). 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 Figures 1 and 2, we report separate graphs for the effect of attending at least 1 year, at least 2 years and 3 years. So, 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 side column 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 side column 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 Figure 1, we look at the effect of additional years of preschool on school attendance. The biggest effect of the treatment on school attendance rates is due to attendance at the extensive margin (having attended versus not having attended). There is a small additional effect from 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 Figure 2 we look at how the intensity of treatment affects years of schooling completed. Similarly to Figure 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, statistically significant effects at the intensive margin are found. This suggests a monotonic relationship between years of preschool and completed schooling. However, once we condition on household fixed effects, this 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 effects of preschool at on school attainment at the intensive margin.

4.4 Heterogeneous effects

We now investigate whether and to what extent there are differential effects of pre-school exposure for different groups of individuals. In table 6 we present separate results of preschool exposure on stay-on rates for children of low- 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 grades 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 Montevideo. For example, column (1) illustrates that children of mothers with low education who were exposed to treatment are 27 percentage points more likely to be in school by age 15 compared to their siblings who did not receive treatment. This effect is only 8 percentage points for children of highly educated mothers, and not statistically significant. Similarly we find that at age 15 the effect of pre-school exposure on stay-on rates is in the order of 34 percentage points 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 data also illustrate significant differences between boys and girls. It appears that boys benefit more from pre-school exposure than girls. The estimated marginal impact of pre-school exposure on stay-on rates by age 15 is 36 percentage points for boys and 24 percentage points for girls.

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 where 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).

4.5 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 untestable 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 both more likely to 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.

Interestingly, 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 would expect pre-school exposure to show up significantly in the public school attendance regression, which is clearly not the case.

4.6 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 the household. As already discussed, one additional source of potential threat to 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. To compute these averages we use retrospective information on preschool exposure for all children born in the same cohort and living in the same locality independent of the year (2001-2005) in which they are observed.[10][11] Identification of the IV estimates is warranted by the interaction of cohort and locality, which is excluded from the main equation. By exploiting the area specific variation in pre-school enrollment across cohorts we effectively control for children's unobserved traits that might be correlated with the outcome and treatment variable.[12]

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 pre-school at a given age (e.g. age 7 in column 1, age 8 in column 2, etc.) on the average pre-school 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. However, we still find large differences at ages 14 and

[10] Recall that there are 55 localities and 13 cohorts in the sample, with an average of 32 children by cohort and locality.
[11] We also tried to compute these means excluding the child of interest. Results are unchanged.
[12] 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 pre-school construction at such a detailed geographical level.

15 between treated and untreated children. 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, we restricted the sample to siblings with at most 4 years of difference in age. The idea is that the closer the age difference between siblings, the most likely it 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).

5. Discussion and 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 comes from household characteristics that are common to all siblings, this strategy leads to consistent estimates of the effect of interest. Because we are concerned 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.

In order to control for the potential confounding effects of other government interventions, 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.

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 11 treated individuals show an advantage in terms of completed education in the order of 0.34 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 percentage points 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. Instrumental variables estimates lead to qualitatively similar conclusions although admittedly the point estimates are rather imprecise.

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 the results in this paper to the population at large.

Although we have no way to identify in our data the precise mechanism through which small initial differences tend to be exacerbated as children grow older, one explanation is that the initial penalty suffered by children who did not attend preschool gets compounded by the state dependency in grade repetition. Early grade failure may lower expectations and induce disenfranchisement among children, their households or teachers. If the (assumed) remedial effect of grade failure is small or not existent, early grade failure may worsen children's later school progression inducing further grade failure and explaining the diverging paths found in this paper.

Compulsory preprimary 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 potentially very high. Public provided pre-school education hence appears as a very successful 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.

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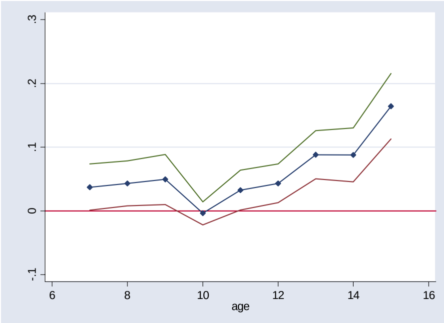
Figure 1

The Effect of Additional Years of Preschool on School Attendance by Age

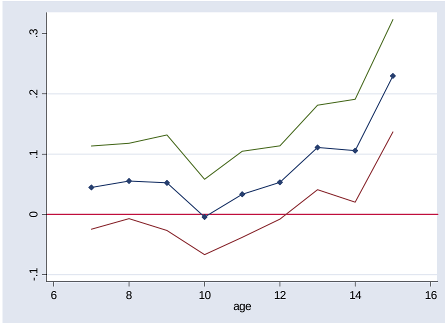
a. Without Household Fixed Effects

1 year of preschool

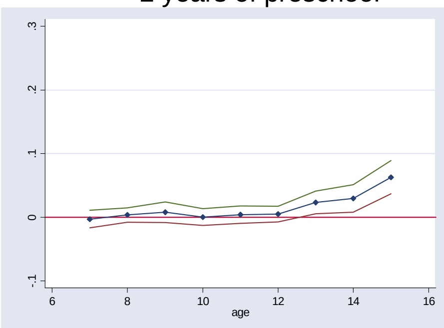


b. With Household Fixed Effects

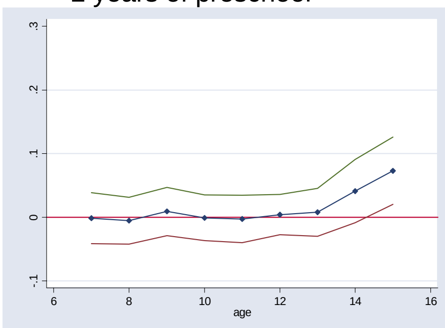
1 year of preschool



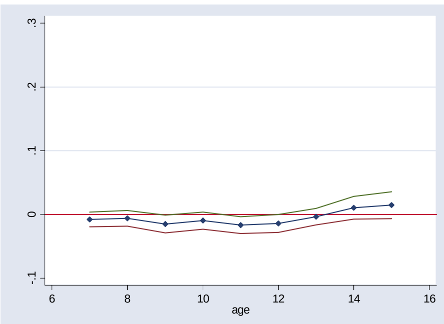
2 years of preschool



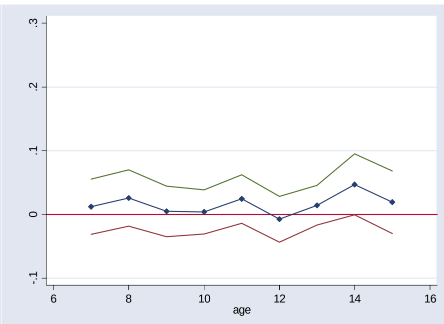
2 years of preschool



3 years of preschool



3 years of preschool



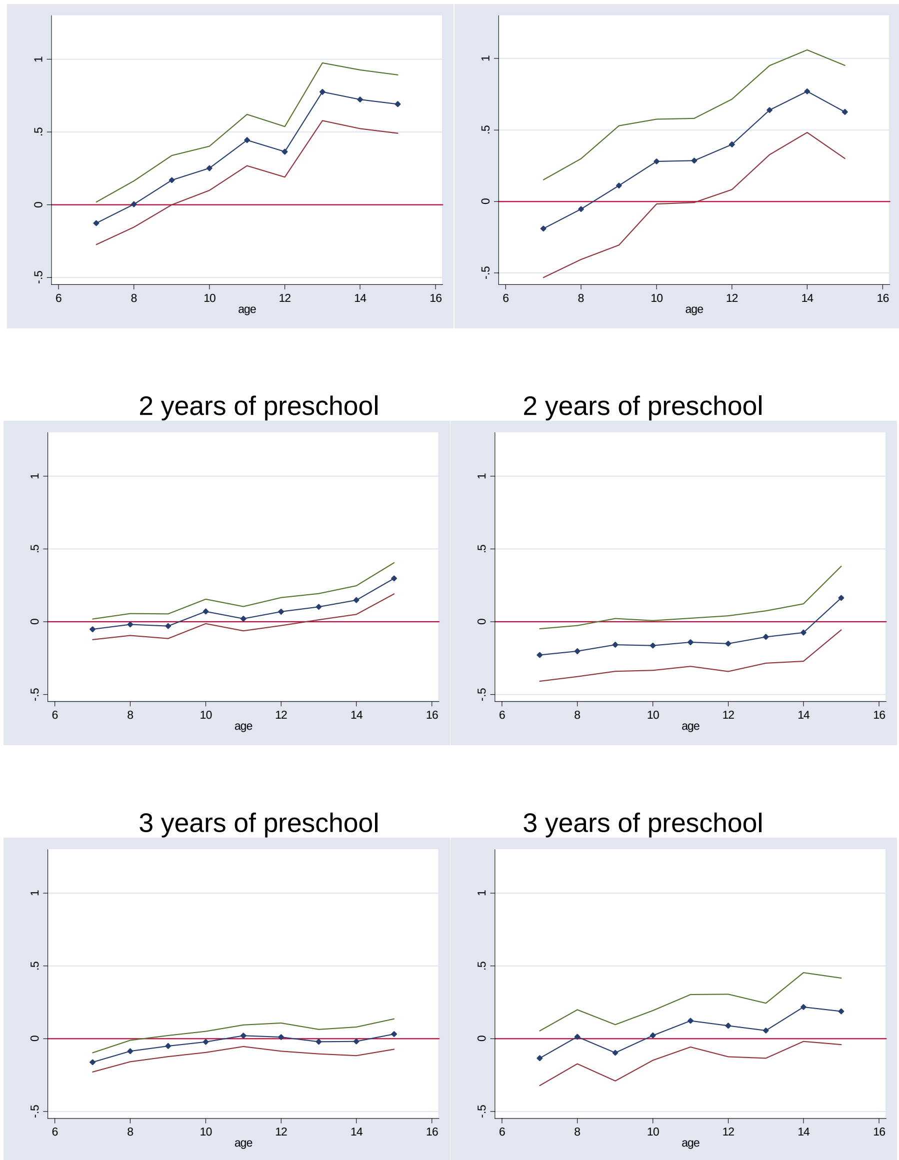
Source: Own calculations based on *Encuesta Continua de Hogares* 2001-2005.
Notes: The graph reports the estimated effect of each additional year of preschool on years of schooling completed. In columns a. and b., respectively, we condition on the same variables as in columns (3) and (4) of Table 5.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

Figure 2

The Effect of Additional Years of Preschool on Years of Schooling Completed by Age

a. Without Household Fixed Effects | b. With Household Fixed Effects

1 year of preschool | 1 year of preschool



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 (7) and (8) of Table 5. 95% confidence intervals around the estimated effects are also reported.

Table 1. Definition and Description of Variables

Variable	Description of Variables	Mean	Std. Dev.	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 9 age dummies.	11.04	2.56	7	15
Cohort	Birth cohort. In years	1991.87	2.94	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	19
Observations			23,042		

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

Table 2. School Progression: School Attendance and Years of Schooling Completed by Age (in percentages).

	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.

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 x Cohort = 1987			0.008	-0.009
			[0.034]	[0.043]
Low Mother's Education x Cohort = 1988			0.018	-0.044
			[0.032]	[0.039]
Low Mother's Education x Cohort = 1989			0.023	-0.018
			[0.031]	[0.039]
Low Mother's Education x Cohort = 1990			0.019	-0.020
			[0.030]	[0.037]
Low Mother's Education x Cohort = 1991			0.043	0.006
			[0.030]	[0.038]
Low Mother's Education x Cohort = 1992			0.056	0.035
			[0.030]*	[0.038]
Low Mother's Education x Cohort = 1993			0.056	0.038
			[0.029]*	[0.039]
Low Mother's Education x Cohort = 1994			0.072	0.071
			[0.029]**	[0.039]*
Low Mother's Education x Cohort = 1995			0.090	0.063
			[0.029]***	[0.040]
Low Mother's Education x Cohort = 1996			0.081	0.099
			[0.030]***	[0.043]**
Low Mother's Education x Cohort = 1997			0.084	0.101
			[0.030]***	[0.045]**
Low Mother's Education x 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. For the definition of control variables see Table 1.
Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. Years of Pre-Primary Education by Cohort using Current and Retrospective Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Current data				Retrospective data			
	Attended				Attended			
Birth Cohort	>=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 current attendance data for the 1986 to 1998 birth cohort and Columns (5) to (8) use retrospective data.

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 x Age = 7	0.035	0.040	0.033	0.043	-0.065	-0.384	-0.210	-0.340
	[0.022]	[0.032]	[0.021]	[0.032]	[0.076]	[0.240]	[0.062]***	[0.236]
Attended 1, 2 or 3 years of preschool x Age = 8	0.046	0.048	0.043	0.053	0.134	-0.187	-0.034	-0.142
	[0.027]*	[0.035]	[0.025]*	[0.035]	[0.083]	[0.243]	[0.084]	[0.230]
Attended 1, 2 or 3 years of preschool x Age = 9	0.053	0.056	0.050	0.056	0.272	0.002	0.130	0.006
	[0.029]*	[0.055]	[0.028]*	[0.055]	[0.154]*	[0.416]	[0.142]	[0.417]
Attended 1, 2 or 3 years of preschool x Age =10	-0.003	-0.009	-0.007	-0.008	0.464	0.211	0.293	0.206
	[0.010]	[0.034]	[0.010]	[0.035]	[0.092]***	[0.191]	[0.093]***	[0.196]
Attended 1, 2 or 3 years of preschool x Age = 11	0.035	0.033	0.031	0.033	0.620	0.262	0.460	0.244
	[0.019]*	[0.047]	[0.019]	[0.047]	[0.090]***	[0.167]	[0.088]***	[0.167]
Attended 1, 2 or 3 years of preschool x Age = 12	0.049	0.053	0.042	0.049	0.556	0.397	0.409	0.360
	[0.014]***	[0.038]	[0.013]***	[0.039]	[0.134]***	[0.162]**	[0.132]***	[0.169]**
Attended 1, 2 or 3 years of preschool x Age = 13	0.107	0.120	0.102	0.115	0.952	0.643	0.835	0.608
	[0.018]***	[0.030]***	[0.018]***	[0.031]***	[0.151]***	[0.335]*	[0.158]***	[0.310]**
Attended 1, 2 or 3 years of preschool x Age = 14	0.114	0.144	0.109	0.138	0.917	0.852	0.810	0.811
	[0.031]***	[0.049]***	[0.031]***	[0.047]***	[0.147]***	[0.143]***	[0.138]***	[0.154]***
Attended 1, 2 or 3 years of preschool x Age = 15	0.214	0.279	0.206	0.274	1.029	0.818	0.881	0.789
	[0.047]***	[0.100]***	[0.046]***	[0.099]***	[0.156]***	[0.260]***	[0.143]***	[0.271]***
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 the definition of variables see Table 1. Standard errors clustered by locality in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

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	0.048	0.037	0.133	0.094	-0.325	-0.256	-0.249	-0.449	-0.409	-0.171
	[0.035]	[0.064]	[0.063]	[0.038]	[0.083]	[0.068]	[0.297]	[0.441]	[0.377]	[0.315]	[0.515]	[0.645]
Attended 1, 2 or 3 years of preschool x Age = 8	0.050	0.078	0.076	0.031	0.083	0.089	-0.254	0.480	-0.247	-0.161	-0.334	0.216
	[0.037]	[0.103]	[0.058]	[0.042]	[0.069]	[0.120]	[0.287]	[0.574]	[0.506]	[0.140]	[0.385]	[0.561]
Attended 1, 2 or 3 years of preschool x Age = 9	0.070	0.039	0.051	0.057	0.064	0.089	0.043	0.078	0.157	-0.167	-0.091	0.263
	[0.061]	[0.099]	[0.073]	[0.084]	[0.149]	[0.127]	[0.349]	[0.817]	[0.508]	[0.585]	[0.927]	[0.708]
Attended 1, 2 or 3 years of preschool x Age =10	-0.002	-0.003	-0.023	0.007	-0.002	0.006	0.169	0.356	-0.046	0.367	0.062	0.565
	[0.045]	[0.076]	[0.052]	[0.046]	[0.082]	[0.078]	[0.285]	[0.466]	[0.304]	[0.171]**	[0.431]	[0.645]
Attended 1, 2 or 3 years of preschool x Age = 11	0.033	0.074	0.032	0.030	0.100	-0.007	0.093	0.721	0.207	0.226	0.453	0.284
	[0.054]	[0.128]	[0.068]	[0.072]	[0.109]	[0.125]	[0.213]	[0.499]	[0.299]	[0.202]	[0.542]	[0.604]
Attended 1, 2 or 3 years of preschool x Age = 12	0.058	0.047	0.075	0.024	0.107	0.047	0.309	0.519	0.194	0.485	0.811	0.322
	[0.047]	[0.049]	[0.067]	[0.036]	[0.097]	[0.078]	[0.231]	[0.593]	[0.264]	[0.199]**	[0.386]**	[0.315]
Attended 1, 2 or 3 years of preschool x Age = 13	0.126	0.053	0.078	0.148	0.174	0.048	0.571	0.491	0.399	0.756	0.562	0.729
	[0.031]***	[0.062]	[0.049]	[0.033]***	[0.161]	[0.095]	[0.333]*	[0.545]	[0.361]	[0.509]	[0.646]	[0.606]
Attended 1, 2 or 3 years of preschool x Age = 14	0.134	0.058	0.148	0.129	0.211	0.099	0.753	0.622	0.609	0.952	1.064	0.894
	[0.067]**	[0.072]	[0.066]**	[0.075]*	[0.128]*	[0.152]	[0.241]***	[0.272]**	[0.205]***	[0.168]***	[0.402]***	[0.563]
Attended 1, 2 or 3 years of preschool x Age = 15	0.269	0.084	0.203	0.342	0.359	0.241	0.742	0.255	0.588	0.927	0.884	0.875
	[0.105]**	[0.133]	[0.145]	[0.107]***	[0.179]**	[0.200]	[0.347]**	[0.335]	[0.248]**	[0.408]**	[0.439]**	[0.612]
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. For the definition of variables see Table 1. Standard errors clustered by locality in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

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
	[0.031]	[0.047]	[0.031]	[0.048]
Attended 1, 2 or 3 years of preschool x Age = 8	-0.054	0.007	-0.022	0.008
	[0.036]	[0.029]	[0.037]	[0.030]
Attended 1, 2 or 3 years of preschool x Age = 9	-0.024	0.031	0.004	0.031
	[0.033]	[0.025]	[0.030]	[0.026]
Attended 1, 2 or 3 years of preschool x Age =10	-0.047	0.009	-0.005	0.008
	[0.026]*	[0.026]	[0.024]	[0.026]
Attended 1, 2 or 3 years of preschool x Age = 11	-0.092	0.007	-0.055	0.006
	[0.022]***	[0.038]	[0.021]***	[0.037]
Attended 1, 2 or 3 years of preschool x Age = 12	-0.042	0.002	-0.011	0.003
	[0.019]**	[0.024]	[0.017]	[0.024]
Attended 1, 2 or 3 years of preschool x Age = 13	-0.033	0.006	-0.006	0.006
	[0.022]	[0.024]	[0.019]	[0.025]
Attended 1, 2 or 3 years of preschool x Age = 14	-0.056	0.002	-0.025	0.002
	[0.019]***	[0.026]	[0.017]	[0.025]
Attended 1, 2 or 3 years of preschool x Age = 15	-0.029	0.002	0.003	0.003
	[0.024]	[0.026]	[0.023]	[0.026]
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 the definition of variables see Table 1. Standard errors clustered by locality in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8.The Impact of Preschool Attendance on School Attendance and Years of Schooling Completed - Instrumenal 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	-0.359	-0.278
	[0.052]	[0.114]	[0.051]	[0.115]	[0.294]	[0.519]	[0.305]	[0.494]
Attended 1, 2 or 3 years of preschool x Age = 8	0.182	0.181	0.176	0.199	-0.138	-0.082	-0.302	-0.089
	[0.071]**	[0.200]	[0.069]**	[0.189]	[0.417]	[0.819]	[0.421]	[0.814]
Attended 1, 2 or 3 years of preschool x Age = 9	0.078	0.119	0.083	0.114	0.356	0.451	0.137	0.368
	[0.084]	[0.206]	[0.083]	[0.196]	[0.432]	[1.228]	[0.457]	[1.234]
Attended 1, 2 or 3 years of preschool x Age =10	0.047	-0.018	0.047	-0.018	0.304	0.162	0.273	0.048
	[0.061]	[0.167]	[0.060]	[0.167]	[0.374]	[0.786]	[0.385]	[0.786]
Attended 1, 2 or 3 years of preschool x Age = 11	0.090	0.037	0.094	0.029	0.713	0.447	0.601	0.346
	[0.072]	[0.212]	[0.071]	[0.214]	[0.363]**	[0.649]	[0.288]**	[0.646]
Attended 1, 2 or 3 years of preschool x Age = 12	0.058	0.019	0.048	0.007	0.796	0.506	0.744	0.449
	[0.055]	[0.182]	[0.055]	[0.186]	[0.445]*	[0.718]	[0.410]*	[0.715]
Attended 1, 2 or 3 years of preschool x Age = 13	0.023	-0.001	0.017	-0.019	0.719	0.806	0.648	0.688
	[0.069]	[0.117]	[0.072]	[0.119]	[0.391]*	[0.891]	[0.394]*	[0.808]
Attended 1, 2 or 3 years of preschool x Age = 14	0.116	0.071	0.116	0.058	0.820	0.955	0.869	0.872
	[0.077]	[0.133]	[0.075]	[0.131]	[0.361]**	[0.572]*	[0.355]**	[0.567]
Attended 1, 2 or 3 years of preschool x Age = 15	0.307	0.410	0.298	0.397	1.242	1.049	1.120	0.917
	[0.084]***	[0.171]**	[0.084]***	[0.170]**	[0.508]**	[0.976]	[0.533]**	[0.955]
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 the definition of variables see Table 1. Standard errors clustered by locality in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A1.Individual Pre-school Attendance and Average Pre-school Attendance by Cohort and Locality - First Stage Esitmates
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
	[0.214]***	[0.085]	[0.083]	[0.089]	[0.041]	[0.147]	[0.073]	[0.073]	[0.108]
x Age = 8	-0.043	1.125	-0.007	-0.169	-0.177	-0.047	-0.085	-0.087	0.090
	[0.066]	[0.232]***	[0.051]	[0.119]	[0.132]	[0.055]	[0.135]	[0.121]	[0.169]
x Age = 9	-0.023	-0.044	1.079	-0.083	-0.199	-0.128	0.024	-0.069	-0.074
	[0.071]	[0.045]	[0.285]***	[0.053]	[0.108]*	[0.139]	[0.101]	[0.068]	[0.078]
X Age = 11	-0.018	-0.087	-0.040	1.236	-0.130	-0.203	-0.111	-0.080	-0.091
	[0.064]	[0.051]*	[0.053]	[0.242]***	[0.080]	[0.155]	[0.084]	[0.116]	[0.075]
X Age = 12	-0.013	-0.032	-0.072	-0.077	0.969	-0.036	-0.077	-0.058	-0.072
	[0.037]	[0.052]	[0.062]	[0.063]	[0.253]***	[0.066]	[0.129]	[0.095]	[0.081]
X Age = 13	-0.020	-0.057	-0.076	-0.064	-0.013	0.923	-0.020	-0.067	-0.015
	[0.032]	[0.041]	[0.067]	[0.076]	[0.035]	[0.188]***	[0.072]	[0.060]	[0.078]
X Age = 14	-0.023	-0.054	0.018	-0.094	-0.071	-0.067	1.122	-0.019	-0.043
	[0.030]	[0.042]	[0.026]	[0.063]	[0.066]	[0.047]	[0.183]***	[0.042]	[0.073]
X Age = 15	-0.028	-0.063	-0.042	-0.058	-0.031	-0.147	-0.053	1.266	-0.052
	[0.036]	[0.033]*	[0.063]	[0.048]	[0.068]	[0.149]	[0.066]	[0.220]***	[0.046]
X Age = 16	-0.050	-0.021	-0.011	-0.083	-0.071	-0.114	-0.018	-0.032	1.197
	[0.040]	[0.040]	[0.038]	[0.059]	[0.046]	[0.082]	[0.080]	[0.037]	[0.193]***
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. * significant at 10%; ** significant at 5%; *** significant at 1%.