The effect of maternal education on infant health: Evidence from an expansion of preschool facilities

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Abstract

This study estimates the effect of girlo more starting school earlier on health at birth of the next generation. The identification strategy uses a construction program of preschool facilities implemented in Uruguay by the mid 90's. I exploit variation across regions and over time in the number of facilities built. I find that health at birth as measured by extreme low birth weight and extreme prematurity improves for first-born children of mothers that were exposed to the schooling reform. Also, the likelihood that the birth occurs before the 28th week of gestation is completed, is significantly lower for the entire sample of females exposed to the reform. Maternal education increases preventive care during pregnancy, and reduces fertility.

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Contents

1 Introduction

Maternal education is an important determinant of children's health. Most of the evidence comes from studies that look at extensions of schooling at the end of the school trajectory. This paper studies the effect of an expansion of public preschool facilities in Uruguay on health at birth. I use an infrastructure program, implemented in the mid 1990‚Äôs by the Uruguayan government, that substantially increased the availability of preschool facilities. The program created approximately 36,000 places between the years 1995 and 2000 which represents an increase of 52% of enrollment in 1995 (ANEP, 2007). The identification strategy exploits variation induced by the differential timing and intensity of the construction across regions.

The importance of infant health is widely acknowledged. Infants that are born with low birth weight have worse outcomes both in the short-run and the long-run, in terms of a higher mortality within the first year of life and in terms of educational attainment and earnings in adulthood (Black et al., 2007).[1] In addition, the literature has found that the health income gradient in adulthood can be explained in part by poor health at infancy and that health at birth can contribute to the intergenerational transmission of poverty (Case et al., 2005).

Maternal education can affect infant health through several direct and indirect channels. Education can affect children's health directly because it increases the ability to acquire and process health information (Grossman, 1972), so more educated mothers are more efficient in the production and allocation of both their own health and the health of their offspring. Indirect effects of education on health may work through fertility decisions and assortative mating. Education entails higher earnings and therefore raises a woman's permanent income, influencing her birthing decisions towards fewer children of higher quality (see McCrary and Royer, 2011, and references therein). In the same line, better educated women match with better educated and higher income husbands (Behrman and Rosenzweig, 2002) and this reinforces the permanent income effect.

Only recently there have been some attempts to establish the causality of maternal schooling on infant health.[2] Most of the evidence comes from studies that look at schooling reforms that increased the school leaving age (Güneş, 2015; Chou et al., 2010; Currie and Moretti, 2003; Breierova and Duflo, 2004; Lindeboom et al., 2009; Doyle et al., 2005; **?**).[3] Within this set of studies, results are mixed. Some studies find positive impacts on health at birth as measured by very low birth weight (Güneş, 2015), low birth weight and infant mortality (Chou et al., 2010), birth weight and gestational age (Currie and Moretti, 2003), and child mortality (Breierova and Duflo, 2004). Other studies, however, find no effects of maternal education on infant health (Lindeboom et al., 2009, Doyle et al., 2005, Dinçer et al., 2014).

Increases in maternal education at the beginning of the school trajectory can potentially have large effects

[1] Black et al., 2007 also find that birth weight has an impact on height, the body mass index and the intelligence quotient at age 18.

[2] Numerous studies have documented a positive correlation between mother's schooling and child health (see a review in Grossman, 2006). This correlation should, however, not be interpreted causally. Selection bias arises, for example, if mothers of better ‚Äúquality‚Äù tend to have higher education. The correlation will in that case overestimate the true effect of schooling on birth outcomes.

[3] Güneş, 2015 uses a change in the compulsory schooling law in Turkey which extended compulsory schooling from five to eight years as an instrument for maternal education. Chou et al., 2010 look at an extension of compulsory education in Taiwan from 6 to 9 years and exploit differential rates in the expansion across regions. Schooling is instrumented using variations across cohorts in new junior high school openings. Currie and Moretti, 2003 instrument maternal education with college openings by county in the US at the time when the mother was aged 17. Breierova and Duflo, 2004 use a primary school construction program in Indonesia as exogenous variation in schooling to analyze the effect of parental education on child mortality. Lindeboom et al., 2009 exploit a compulsory schooling reform in 1947 in the UK which changed the age of school exit from 14 to 15 years old. Doyle et al., 2005 use another change in the age of school exit in Britain that occurred in the year 1957 and use grand-parental smoking behavior to instrument parental education and income. Dinçer et al., 2014 use a change in the compulsory schooling law in Turkey as an instrument for schooling.

on infant health. Preschool education is designed to prepare children for school and this can have an impact on the development of their cognitive and non-cognitive skills. Berlinski et al., 2009, for example, find that attending preschool in Argentina had positive impacts on test scores and on behavioral skills. Moreover, preschools can improve (future) parents' access to parenting or health information that can lead to better parental investments in general. In the economics literature, Cunha and Heckman, 2007 make a strong case that investments during the early years are cost-efficient given that these investments could benefit from autoproductivity and dynamic complementaries.

To the best of my knowledge, the only paper that focuses on the effect of mothers starting school earlier on infant health is McCrary and Royer, 2011. The authors use age-at-school-entry policies in California and Texas and exploit the fact that the year in which a person starts school is a discontinuous function of exact date of birth. The authors find that education has only small effects on infant health and does not affect prenatal behaviors such as smoking rates and prenatal care.

This study contributes to the literature by providing evidence of the effects of an expansion at an even earlier grade of education than analyzed by McCrary and Royer, 2011. I analyze whether a mother's participation in a preschool program impacts the health of her offspring. The main database used in the analysis compiles information from vital statistics natality micro-data for the years 2008-2015. This database has information on all registered births in Uruguay and provides information on pregnancy outcomes and parents' characteristics. I link this information with measures of availability of preschool places in the period 1992-2000, that I construct using school level data provided by the National Administration of Public Education.

I estimate reduced-form effects of the expansion of preschool facilities on infant health outcomes. As a first stage, I evaluate the impact of the reform on educational attainment of mothers and report that for every additional preschool place opened per child aged 4, completed education increased by 0.37 years. Second, I estimate the effect of the reform to which mothers were exposed on birth outcomes of their children. As measures for infant health, I use low birth weight, very low birth weight and extreme low birth weight and indicators for whether the child was born premature, very premature or extremely premature. The results for a sample of first time mothers show that the likelihood of extreme low birth weight and extreme prematurity decreases with the number of preschool places opened per child. In the full sample of mothers, the likelihood of extreme prematurity decreases with the number of preschool places opened per child.

After evaluating the effects on health at birth, I examine some of the pathways that could underlie the relationship observed. I find that exposed mothers have a larger probability of having more prenatal controls during pregnancy. Also, my results suggest that the expansion of preschool places had an impact on fertility. In particular, the probability of motherhood decreased for first time mothers. This effect mainly reflects a reduction in teenage pregnancies.

This remainder of this paper is organized as follows. Section 2 describes the schooling reform used in the identification. Sections 3 and 4 describe the data and the identification strategy, respectively. Results are presented in Section 5 and I conclude in Section 6.

2 The reform

Uruguay is a small middle-income country. In 1995, annual GDP per capita of USD 8,393. Around half of the country's 3.2 million inhabitants are concentrated in the capital city, Montevideo, and the rest of the population is separated across 18 other regional departments. One quarter of the total population are children aged 0-14. In terms of the education system, Uruguay has a long tradition of publicly provided education. Free schooling is provided to children aged 4 and 5 within primary school premises, or to children aged 3, 4 and 5 in separate kindergartens (ANEP-CODICEN, 2000). Children usually attend public preschool centers 4 hours per day (either in the morning or afternoon shift), 5 days a week, and 9 months a year.

By the mid 1990‚Äôs, the Uruguayan Government decided to implement a series of measures aimed at achieving universal preschool for children aged 4 and 5 (ANEP-CODICEN, 2000). This reform intended to alleviate two features of the education system in Uruguay: grade retention and early dropout. The hope was that the reform would increase the number of years of schooling and would facilitate children's insertion and transition through the primary school system.

One of the main constraints for the expansion of preschools, was the lack of infrastructure. In 1995, the National Administration of Public Education (ANEP) started a large construction program to expand preschool provision in public primary schools. Between 1995 and 1999, 414 classrooms were added either because they were newly built or because they were made available after being refurnished. New classrooms were added mainly in existing primary schools and were allocated across the different Uruguayan regional departments based on a specific allocation rule. Priority in the construction was given to: (i) places with strong demographic growth in corresponding age cohorts in the decade prior to the reform, (ii) deprived areas with low physical investment and (iii) bordering regions with Brazil where the cultural identity needed to be strengthened. This framework generated considerable variation in construction intensity and the supply of preschool facilities among the regional departments.

The reform was successful in increasing preschool participation. Enrollment and attendance rates for children aged 4 and 5 grew substantially between the years 1995 and 2000 (ANEP, 2005, ANEP, 2007). The number of children enrolled in public preschools increased from 49,618 to 84,984, a rise of 71%, while enrollment in private preschools remained more stable (the number of pupils grew from 19846 to 20806). Consequently, the percentage of students enrolled in public preschools increased from 71% to 80% between the years in consideration. The attendance rates of children aged 4 and 5 grew from 65% to 82% between 1995 and 2000 and the increase was more pronounced in the group of 4 year olds. Moreover, the expansion was progressive as it attracted students from more disadvantaged backgrounds. By 1991, attendance rates to preschool of children aged 4 was around 20% for the lowest quintile, while in 2002 this number was in the order of 60%.

3 Data

This paper combines pregnancy and delivery data with school-level data. This section describes the two datasets used and then describes the treatment and outcome variables.

Pregnancy and delivery data comes from the vital statistics natality micro-data for the period 2008-2015. This database provides information on all registered live births in Uruguay. Registered births are around 98% of all pregnancies in the country and the dataset covers on average almost 48,000 births per year. Starting from 2008, the vital statistics provide the following information: (i) parents' characteristics such as year and department of birth, years of education and marital status, (ii) number of previous pregnancies of the mother, (iii) prenatal care utilization, and (iv) birth outcomes including birth weight and gestational week in which the birth occurred.

Apart from natality data, I use school-level data from the Monitor Educativo de Ense√±anza Primaria, an administrative registry produced by the Department of Research and Statistics of ANEP. This source provides information on preschool and primary education in Uruguay since 1992 for all public schools. The database contains information on each school,Äôs: (i) location, (ii) social context, (iii) enrollment by level, number of groups and group size, and (iv) students,Äô educational outcomes (insufficient attendance, repetition and dropouts). The administrative registry has a 100% coverage in all years. I consider information for the period 1992-2000, which includes cohorts exposed and not exposed to the reform, to construct a measure of availability of preschool places.

The two data sources are merged and form a pooled cross-section of mothers born between 1988 and 1996 that gave birth in the years 2008-2015. I restrict the sample to mothers older than 14 years of age given to a low number of births for younger women (less than 1% of the sample). In total, the sample includes 95,450 births of mothers aged 15 to 27 with complete information for all variables considered in the analysis.[4]

I provide results for the full sample of mothers and for the subsample of first-time mothers. More than half of the observations of the sample are first time-mothers (55%). As discussed by McCrary and Royer, 2011, the group of first-time mothers is more comparable to other samples of women that have been analyzed in the literature. Moreover, this subsample includes only women that are giving birth for the first time and, hence, constitutes a more homogeneous group than the full sample. In the full sample the same woman can be observed multiple times.

3.1 Treatment variable

The school-level data enables the construction of a measure for availability of public preschool places for children aged 4 and 5.[5] Availability of preschool places per child is a measure for treatment intensity and it varies by department and cohort of birth of the mother. It is constructed by multiplying the total number of groups in the mother's department of birth, in the year in which she was 4 or 5 years old by an average of 25 students per group and dividing this number by the population of the corresponding age (4 or 5) in that department and year.[6] [7] Even though exposure varies according to department where the mother lived at the age of 4 or 5, department of birth is preferable to assign treatment intensity because it is not subject to endogenous migration.

Tables 1 and 2 show the availability of preschool places per child by cohort and department. On average, available preschool places per child was 0.4 for 4-year-olds and 0.66 for 5 year-olds in the period 1992-2000. The growth of preschool places available for 4-year-olds and 5-year-olds was different across departments. For example, between the years 1992 and 2000, Maldonado increased availability of preschool places per child by 381% (from 0.11 to 0.53) in the 4 year-old-group and 68% in the 5-year-old-group, while Rocha increased its availability of preschool places per child by 55% (from 0.34 to 0.53) for the 4-year-old group and by only 1% for the 5-year-old-group. The expansion was more pronounced for the youngest cohort (see last column of Tables 1 and 2). Given the latter fact, I will focus on the effect of the expansion of preschool places for children aged 4.

[4] In Table A1 in Appendix I show the corresponding age for each pair of birth-cohort and year of observation in the sample.
[5] I consider data for schools and kindergartens.
[6] I exclude data from rural schools given that groups within these schools are typically very different in terms of size to those in other schools.
[7] Population data comes from the Uruguayan Population Projections by year and age provided by the Uruguayan National Institute of Statistics.

Table 1: Availability of preschool places per child by cohort and department for 4-year-olds

Department	Year									Increase
	1992	1993	1994	1995	1996	1997	1998	1999	2000	1992-2000
Montevideo	0.23	0.25	0.25	0.20	0.29	0.35	0.41	0.43	0.43	89%
Artigas	0.15	0.18	0.17	0.13	0.17	0.31	0.34	0.48	0.45	202%
Canelones	0.21	0.20	0.22	0.18	0.23	0.36	0.50	0.46	0.48	125%
Cerro Largo	0.15	0.20	0.15	0.22	0.28	0.26	0.36	0.36	0.40	168%
Colonia	0.32	0.29	0.31	0.31	0.38	0.54	0.48	0.49	0.53	64%
Durazno	0.30	0.25	0.23	0.28	0.38	0.37	0.58	0.50	0.59	93%
Flores	0.39	0.32	0.39	0.39	0.52	0.76	0.63	0.62	0.50	30%
Florida	0.37	0.34	0.39	0.34	0.39	0.46	0.68	0.64	0.65	77%
Lavalleja	0.34	0.45	0.42	0.45	0.48	0.69	0.63	0.70	0.59	75%
Maldonado	0.11	0.11	0.16	0.21	0.19	0.28	0.59	0.51	0.53	381%
Paysandu	0.12	0.14	0.11	0.12	0.14	0.33	0.49	0.44	0.39	219%
Rio Negro	0.30	0.32	0.32	0.32	0.40	0.40	0.44	0.49	0.61	105%
Rivera	0.20	0.21	0.20	0.27	0.39	0.44	0.39	0.53	0.60	203%
Rocha	0.34	0.25	0.32	0.37	0.37	0.49	0.50	0.44	0.53	55%
Salto	0.16	0.20	0.20	0.15	0.13	0.33	0.40	0.43	0.44	179%
San Jose	0.20	0.22	0.24	0.26	0.29	0.57	0.63	0.58	0.63	213%
Soriano	0.29	0.27	0.29	0.29	0.27	0.47	0.37	0.45	0.52	84%
Tacuarembo	0.42	0.40	0.38	0.31	0.43	0.57	0.59	0.65	0.68	63%
Treinta y Tres	0.24	0.21	0.32	0.38	0.35	0.48	0.47	0.44	0.60	155%

Note: Availability of preschool places per child of age 4 is calculated as the number of groups opened for 4-year olds by department and year multiplied by an average of 25 students per group and divided by the number of children aged 4 in each region in the corresponding year (obtained from the Uruguayan National Institute of Statistics).

Table 2: Availability of preschool places per child by year and department for 5-year-olds

Department	Year									Increase
	1992	1993	1994	1995	1996	1997	1998	1999	2000	1992-2000
Montevideo	0.50	0.52	0.53	0.51	0.52	0.46	0.51	0.50	0.51	2%
Artigas	0.49	0.50	0.47	0.50	0.49	0.63	0.68	0.62	0.50	3%
Canelones	0.47	0.49	0.53	0.54	0.56	0.56	0.56	0.62	0.60	27%
Cerro Largo	0.67	0.69	0.62	0.67	0.64	0.51	0.73	0.52	0.59	-12%
Colonia	0.81	0.78	0.81	0.78	0.73	0.82	0.79	0.79	0.84	4%
Durazno	0.70	0.62	0.70	0.70	0.88	0.76	0.74	0.55	0.62	-12%
Flores	0.85	0.85	0.91	0.85	0.85	0.90	0.82	0.75	0.75	-12%
Florida	0.89	0.79	0.84	0.89	0.84	0.86	0.79	0.78	0.69	-23%
Lavalleja	0.88	0.88	0.94	0.94	1.06	0.73	0.55	0.73	0.65	-27%
Maldonado	0.38	0.34	0.43	0.48	0.50	0.64	0.47	0.56	0.64	68%
Paysandu	0.43	0.46	0.41	0.47	0.46	0.46	0.58	0.60	0.65	49%
Rio Negro	0.65	0.65	0.73	0.76	0.76	0.64	0.50	0.75	0.69	7%
Rivera	0.59	0.63	0.66	0.66	0.74	0.70	0.79	0.59	0.76	29%
Rocha	0.65	0.70	0.79	0.70	0.77	0.64	0.71	0.67	0.66	1%
Salto	0.44	0.51	0.48	0.48	0.42	0.46	0.45	0.49	0.51	15%
San Jose	0.67	0.57	0.67	0.68	0.76	0.70	0.79	0.57	0.63	-5%
Soriano	0.72	0.72	0.69	0.65	0.80	0.78	0.65	0.64	0.48	-33%
Tacuarembo	0.69	0.76	0.69	0.69	0.80	0.72	0.72	0.74	0.83	-23%
Treinta y Tres	0.70	0.64	0.64	0.79	0.86	0.70	0.68	0.55	0.54	77%

Note: Availability of preschool places per child of age 5 is calculated as the number of groups opened for 5-year olds by department and year multiplied by an average of 25 students per group and divided by the number of children aged 5 in each region in the corresponding year (obtained from the Uruguayan National Institute of Statistics).

3.2 Outcome variables

In Table 3 I describe the definitions of the outcome variables used in the analysis and their sources. The main outcomes of interest are birth outcomes. To measure health at birth, I focus on low birth weight, a measure that is generally considered as an indicator for intrauterine growth retardation during pregnancy, and on being born premature. In particular, I use indicators for low birth weight, very low birth weight and extreme low birth weight and premature, very premature and extreme prematurity. I employ the thresholds according to the definitions listed in the international Statistical Classification of Diseases and Related Health Problems (ICD-10) codes of the World Health Organization. Children born with extreme low birth weight or extreme prematurity have a higher risk of facing health difficulties later on, so it is worthwhile exploring whether results are sensitive to these margins. I also consider outcomes that could shed light on potential channels for changes in birth outcomes. These variables relate to maternal health behavior, maternal and paternal characteristics and fertility decisions.

Table 3: Description of variables

Variable	Definition
Birth weight	Weight in grams.
Birth weight indicators	Binary variables. Equals 1 if birth weight is below threshold, 0 otherwise. The thresholds considered in the analysis are: 2500g, 1500g, and 1000g. These thresholds are referred to as: low birth weight, very low birth weight and extreme low birth weight, respectively.
Gestational weeks	Weeks of gestation.
Prematurity indicators	Binary variables. Equals 1 if birth occurred before the threshold week of gestation, 0 otherwise. The thresholds considered in the analysis are: 37 weeks, 32 weeks, 28 weeks. These thresholds are referred to as: premature, very premature and extreme prematurity, respectively.
Mother's years of education	Completed years of education of the mother.
Primary school completion	Binary variable. 1 if mother has completed primary school and 0 otherwise.
Mother's age at birth	Age in years.
Mother and father of child live together	Binary variable. 1 if father and mother of the child live together, 0 otherwise.
Previous gestations	Binary variable. Equals 1 if woman had previous gestations, 0 if it is her first gestation.
Prenatal control during first trimester of pregnancy	Binary variable. Equals 1 if woman had a prenatal checkup during her first trimester of pregnancy, 0 otherwise.
Mother had more than 7 prenatal controls during pregnancy	Binary variable. 1 if woman had more than 7 prenatal controls during pregnancy, 0 otherwise.
Fertility by age	Number of first-borns per women of each age, cohort and department until 2015 divided by the population of the corresponding cohort. This measure is aggregated at the department-cohort-age level.
Total fertility	Number of first-borns per women of each cohort and department until 2015 divided by the population of the corresponding cohort. This measure is aggregated at the department-cohort level.

Note: Sources for construction of variables are the Uruguayan natality vital statistics and population projections by year and age provided by the Uruguayan National Institute of Statistics.

Table 4 shows sample statistics for maternal and paternal characteristics, birth outcomes and maternal health behavior. In terms of maternal and paternal characteristics, on the full sample of mothers, women on average have completed 8.6 years of education and completion of primary school is almost universal (98%). First-time mothers have a slightly higher average of completed years of education (9 years) and the likelihood of completing primary school is also very large (98%). Regarding parental education, average education of the parents of the child is lower in the full sample (8.4 years) than in the sample of first-time mothers (8.7 years). The full sample corresponds to women with an average age of 21 while the subsample of firs- time mothers corresponds to women that are on average one year younger. Slightly more than half of the mothers in both samples report living with the father of the child and the average number of previous pregnancies in the full sample is one.

Regarding birth outcomes, the average weight of newborns is 3253 grams in the full sample and 3222 grams in the sample of first time mothers. The incidence of low birth weight for the thresholds of 2500, 1500 and 1000 grams is higher in the sample of first-time mothers than in the full sample, being 8%, 1.2% and 0.5%, and 7.3%, 1.1% and 0.4%, respectively. The likelihood that the baby is born before the 37th, 32nd and 28th week of gestation is also higher in the first-time mothers sample than in the full sample being 8.8%, 1.3% and 0.5%, and 8.5%, 1.2% and 0.4%, respectively. The average number of gestational weeks is approximately 38.6 weeks in both samples. In terms of prenatal care, 62% of all mothers have a prenatal control in the first trimester of pregnancy and this number is slightly higher for first-time mothers (67%). On average, 70% of the full sample of mothers and 76% of first-time mothers visit the doctor more than 7 times during pregnancy.

Table 4: Descriptive statistics

	Full sample		First-time mothers	
	Mean	s.d.	Mean	s.d.
Maternal and paternal characteristics				
Mother's education in years	8.572	2.378	8.965	2.420
Mother completed primary school	0.976	0.152	0.984	0.124
Avg. years of education between mother and father	8.397	2.168	8.744	2.192
Mother's age at birth	20.643	2.694	19.827	2.605
Mother and father of child live together	0.513	0.500	0.547	0.498
Number of previous pregnancies	0.671	0.954	0	0
Birth outcomes				
Weight (in grams)	3252.747	548.822	3221.557	552.208
Low birth weight (<2500g)	0.073	0.260	0.080	0.271
Very low birth weight (<1500g)	0.011	0.103	0.012	0.111
Extreme low birth weight (<1000g)	0.004	0.063	0.005	0.068
Premature (<37 weeks of gestation)	0.085	0.279	0.088	0.283
Very Premature (<32 weeks of gestation)	0.012	0.109	0.013	0.115
Extreme Premature (<28 weeks of gestation)	0.004	0.062	0.005	0.069
Gestational weeks	38.566	1.886	38.571	1.954
Maternal health behavior				
Prenatal care in first trimester of pregnancy	0.617	0.486	0.665	0.472
More than 7 prenatal controls during pregnancy	0.698	0.459	0.758	0.428

Note: S.d. corresponds to standard deviation. The number of observations in the full sample is 95,450 and the number of observations in the subsample of first-time mothers is 52,831. Mother completed primary school=1, mother and father of child live together=1, prenatal care in first trimester=1 if mother had at least one prenatal control during the first trimester of pregnancy, more than 7 prenatal controls=1.

4 Empirical strategy

The aim of this paper is to estimate the effect of the expansion of preschool places on the health at birth of the children of the exposed mothers. The main analysis focuses on the expansion for 4-year-olds given that, as was previously noted, this was the age group that was more intensively treated. Following Duflo (2001) and Berlinski and Galiani (2007) the empirical strategy relies on a generalized difference-in-differences strategy that combines differences across regions in the number of facilities built with differences in exposure across cohorts induced by the timing of the program. In my estimations, I control for department and cohort fixed effects: department fixed effects control for constant characteristics at the department level that are fixed over time and cohort fixed effects control for unobserved differences across cohorts. I also include year of observation fixed effects to control for common time effects across mothers giving birth such as changes in economic conditions or policies.

As a first stage, I consider the impact of the expansion of preschool facilities per child on educational attainment. If the construction of preschool facilities led to an increase in the number of completed years of education, then the difference in educational attainment between a mother that was exposed to the preschool expansion and one that was not would be positively related to the number of facilities built. More formally, I estimate the following equation using Ordinary Least Squares (OLS):

$$S_{icd} = \alpha_1 + \delta_{1d} + \gamma_{1c} + \beta_1 Stock_{cd} + \epsilon_{icd} \tag{1}$$

where S_{icd} is a measure of educational attainment (completed years of education -without including preschool- or an indicator variable of completion of primary school) of woman i, who was born in cohort c, and in department d; δ_{1d} are department fixed effects; γ_{1c} are cohort fixed effects; and $Stock_{cd}$ is a measure for the availability of preschool places per child.

I next evaluate the impact of the preschool expansion on health at birth outcomes, maternal and paternal characteristics and prenatal care by estimating equations of the following form:

$$Y_{icdt} = \alpha_2 + \delta_{2d} + \gamma_{2c} + \rho_{2t} + \beta_2 Stock_{cd} + \varepsilon_{icdt} \tag{2}$$

where Y_{icdt} is the outcome of interest for the birth of child i, whose mother was born in cohort c and department d, and is observed in year t; δ_{2d} are department fixed effects; γ_{2c} are cohort fixed effects and ρ_{2t} are year of observation fixed effects that are common to all departments in period t. β_2 captures the average effect of an extra place available per child on the outcome variable of interest. I adjust the standard errors for clustering at the department and cohort level.

Changes in fertility can naturally lead to differences in birth outcomes. Therefore, to study whether fertility is one of the channels through which health at birth is affected, I estimate the effect of the expansion of preschool places on fertility behaviors including the probability of motherhood and the timing of motherhood. I construct a proxy for the probability of motherhood as follows: I aggregate the number of births from first-time mothers for each cohort and divided this number by the size of the corresponding cohort in 2015.[8] In the same way, I also constructed a measure that is disaggregated by age.

The identification strategy relies on the assumption that the outcome variable would not have been systematically different in departments where the program constructed more preschool places and departments where the program constructed fewer preschool places. In other words, the main identification assumption

[8] I use data from the Uruguayan Population Projections for 2015 by year and age provided by the Uruguayan National Institute of Statistics.

is that in the absence of an increase in availability of preschool places, changes in health at birth would not have been systematically different between departments with high and low construction rates. To verify the robustness of my estimates, I will conduct several checks.

The treatment intensity of implementation of the policy could be correlated with specific trends in birth rates between departments. Indeed, treatment intensity was based on the demographic growth preceding the implementation of the construction program. Therefore, as a first robustness check, I include differential trends by department in my estimations. Equation 2 transforms into:

$$Y_{icdt} = \alpha_3 + \delta_{3d} + \gamma_{3c} + \rho_{3t} + \beta_3 Stock_{cd} + \phi_{cd} + e_{icdt} \tag{3}$$

where ϕ_{cd} corresponds to the interaction between the cohort of birth of the mother and department dummies.

The common trend assumption could also be violated if changes in health at birth would have happened faster in the absence of the program in regions where the starting enrollment rates in preschool were higher and if the allocation of preschool places was correlated to the starting enrollment rates. To address this concern, following Duflo (2001), I control for possible omitted time-varying department-level factors that may be correlated with pre-program enrollment rates by adding to the main model the interaction of available preschool places per child by department in 1995 with fixed effects by cohort. Equation 2 is adjusted in the following way:

$$Y_{icdt} = \alpha_4 + \delta_{4d} + \gamma_{4c} + \rho_{4t} + \beta_4 Stock_{cd} + \beta_5 \sum_{c=1}^{9} (Stock_{1995d} * d_c) + \mu_{icdt} \tag{4}$$

where $(Stock_{1995d} * d_c)$ represents the interaction of the stock of available preschool places in 1995 in each department by cohort dummies.

Finally, I perform additional placebo regressions to verify the robustness of my estimates. I estimate Equation 2 using pre-treatment cohorts. If trends between departments with different treatment intensity are the same in the pre-treatment period, the expectation is that there should be no impact of the expansion of preschool places on health at birth on those years.

5 Results

In this section I present the results of the analysis. First, I consider the effects of the expansion of preschool places on maternal education. Secondly, I provide evidence on the effects on health at birth, maternal and paternal characteristics and prenatal care. Thirdly, I evaluate the impact on fertility and analyze the sensitivity of my estimates to changes in birthing decisions. Fourthly, I analyze heterogeneous impacts on health at birth. Finally, I analyze the robustness of my estimates.

5.1 Effects of the expansion of preschool places on education

Table 5 presents estimates of the effect of the reform on education (Equation 1) for the full sample and for the sample of first-time mothers. I consider two outcome variables: completed years of education and likelihood of completing primary school. Results show a positive and significant effect on years of education in the full sample. For every extra preschool place per child available for children aged 4, completed years of education increased by 0.37 years.[9] The last two columns of Table 5 show the impact of the expansion

[9] Berlinski et al., 2008 exploit the same expansion of preschool places in Uruguay using a within-household estimator. The authors find that by the age of 15 treated children accumulated 0.8 extra years of education in comparison to untreated

of preschool places on the likelihood of completing primary school. For this outcome variable, the effect is significant in the subsample of first-time mothers, and the interpretation of the coefficient is that for every preschool place available per child aged 4, the probability of completing primary school increased by 3.7 percentage points.

Table 5: Effects of the expansion of preschool places per child on completed education

	Years of education		Completed primary school	
	Full sample	First-time mothers	Full sample	First-time mothers
Available preschool places	0.366**	0.193	0.023	0.037***
	(0.162)	(0.166)	(0.014)	(0.013)

Note: * p<.1, ** p<.05, *** p<.01. Table shows results of estimating Equation 1 using OLS. The dependent variable in the first two columns is completed years of education and in the last two columns is an indicator that takes value 1 if the mother of the child completed primary school. The independent variable is the availability of preschool places per child, and the estimations include cohort fixed effects and department fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level. Number of observations for the full sample is 95,450 and number of observations for subsample of first-time mothers is 52,831.

5.2 Effects of the expansion of preschool places on birth outcomes, maternal and paternal characteristics and prenatal care

Table 6 shows the results of estimating Equation 2 for birth outcomes (Panel A) and maternal and paternal characteristics and prenatal care (Panel B). The expansion of preschool places per child implied an improvement in health at birth as measured by extreme prematurity and extreme low birth weight. The estimated effect suggests that for every preschool placed opened per child, the likelihood of extreme low birth weight decreased by 0.7 percentage points in the full sample and by 1.1 percentage points in the sample of first time mothers. Similarly, the likelihood of extreme prematurity decreased by 0.8 percentage points in the full sample and by 1.4 percentage points in the sample of first time mothers. In the case of first-time mothers, the expansion of preschool places per child also had an effect on the probability of low birth weight and the suggested effect is that the probability of giving birth to a child that weighs less than 2500 grams decreased by 4.6 percentage points for every preschool place opened per child. The fact that I find results for extreme thresholds of low birth weight and prematurity suggests that those are the most important margins in which (early) education impacts health at birth.

Panel B of Table 6 shows the results of estimating the effect of the preschool expansion on parental characteristics as well as on prenatal care. First, in the full sample, there is a higher probability that the mother lives with the father at the moment of giving birth. The likelihood that the mother and father live together increases by 9 percentage points for every preschool place per child that was made available. Moreover, average parental education increases. For every preschool placed opened per child, average years of education of the mother and the father of the child increases by 0.36 years. Also, there is an impact on health behaviors as the likelihood that the mother has more than 7 prenatal controls increases by 7.9 percentage points for every preschool place opened per child. In the subsample of first-time mothers, the results indicate that the expansion of preschool had an effect on the average years of education of mother and father of 0.259 years and increased the age at motherhood by 0.13 years for every preschool place that was opened per child. In addition, the likelihood that the mother has more than 7 prenatal controls increases by 13 percentage points per preschool place per child opened.

siblings. The effect works through a fall in grade retention rates in the school trajectory and a reduction in dropout rates during adolescence.

Table 6: Effects of the expansion of preschool places per child on birth outcomes, maternal and paternal characteristics and prenatal care

Dependent variable	Full sample		First-time mothers	
	β_2	s.e.	β_2	s.e.
Panel A: Birth outcomes				
Birth weight (in grams)	20.121	(45.239)	4.489	(53.785)
Low birth weight (<2500g)	-0.024	(0.023)	-0.046*	(0.028)
Very low birth weight (<1500g)	0.001	(0.009)	-0.002	(0.013)
Extreme low birth weight (<1000g)	-0.007*	(0.004)	-0.011*	(0.006)
Premature (<37 weeks)	-0.029	(0.022)	-0.029	(0.028)
Very premature (<32 weeks)	-0.003	(0.010)	-0.007	(0.012)
Extreme premature (<28 weeks)	-0.008**	(0.004)	-0.014**	(0.005)
Panel B: Parental characteristics and prenatal care				
Mother and father of the child live together	0.090**	(0.041)	0.071	(0.048)
Avg. years of education of mother and father	0.361**	0.150	0.259*	0.152
Age of the mother at birth	0.032	(0.037)	0.127***	(0.046)
More than 7 prenatal controls during pregnancy	0.079**	(0.037)	0.127***	(0.044)
Care in first trimester	0.028	(0.037)	0.068	(0.054)

Note: * p<.1, ** p<.05, *** p<.01. Table reports results for the estimation of Equation 2 for several dependent variables using OLS. β_2 corresponds to the estimated coefficient for the independent variable available preschool places per child. Estimations include cohort fixed effects, department fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level. Number of observations for the full sample is 95450 and number of observations for subsample of first-time mothers is 52831.

5.3 Effects of the expansion of preschool places on fertility

Changes in fertility decisions could be a potential channel by which education can affect health at birth. The expansion of preschool places per child may have influenced decisions such as the probability of motherhood and the timing of childbearing and, in turn, this may have led to an improvement in infant health indicators. In this subsection, I estimate the impact of the expansion of preschool places per child on the overall probability of motherhood and on the probability of motherhood by age. The results, which are presented in Table 7, indicate that the impact of the reform on overall fertility of first-time mothers is negative and significant and that the effect essentially comes from a reduction in teenage births at ages 16 and 17.

The evidence presented in Table 7 suggests that the composition of births in my sample changes across cohorts. This implies that in the previous subsection I have identified a parameter that, although it is informative, is not the average treatment effect on the treated. In order to analyze the sensitivity of my estimates to compositional changes, I conduct a bounding analysis which I describe in detail in the next paragraphs.

In my sample, I observe 54,133 births from first-time mothers and I estimate that the reform had a negative impact of 8 percentage points on fertility. Therefore, in the absence of the reform, I would expect to have observed 4,331 additional births of first-time mothers. If the births that are not observed in my sample were the more healthy ones then I would expect that, in the absence of the reform, the improvement on health at birth would have been even larger than the one reported in Table 6. In that case, my estimates of the effect of the reform on low birth weight, extreme low birth weight and extreme prematurity are a lower bound (in absolute terms) of the true effect. If, on the contrary, the births that are not observed in my sample were the more unhealthy births, then, in the absence of the reform, I would expect to find a smaller improvement in health at birth than the one found.

To bound the effect of the reform on birth outcomes, I add 4331 observations to my sample assuming that these births were evenly distributed across the period 2008-2013 and that, per year, the ratio of births

Table 7: Effect of the expansion of preschool places per child on probability of motherhood

Age group	Coef	s.e.	Sample mean	N
15	-0.008	(0.012)	0.011	95
16	-0.039***	(0.013)	0.026	114
17	-0.037**	(0.017)	0.034	133
18	-0.024	(0.017)	0.040	152
19	-0.022	(0.017)	0.039	171
20	-0.023	(0.016)	0.039	152
21	0.020	(0.014)	0.038	133
22	0.019	(0.018)	0.034	114
23	-0.005	(0.023)	0.031	95
24	-0.027	(0.033)	0.026	76
25	0.001	(0.046)	0.025	57
26	0.049	(0.040)	0.022	38
27	-0.007	(0.016)	0.015	19
Overall fertility	-0.080*	(0.046)	0.256	171

Note: * p<.1, ** p<.05, *** p<.01. Table shows results of the OLS estimation of the effect of the expansion of preschool places per child on overall fertility and on the probability of motherhood by age group. Overall fertility and probability of motherhood by age group are defined in Table 3. The coefficient reported corresponds to the independent variable available preschool places per child. Estimations include cohort fixed effects and department fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level. N corresponds to number of observations included in the estimations.

in each department and by mother's age was the same as in the original sample.

Next, I assume different birth-weight-scenarios for the observations that were added. In particular, I assume that the birth weight of the missing observations was either 800 grams, 1000 grams, 2000 grams or 3000 grams.

Table 8 shows the results of estimating Equation 2 using the augmented sample of first-time mothers for the different birth-weight-scenarios of the missing observations. As outcome variables, I consider birth weight, low birth weight, very low birth weight and extremely low birth weight. The results suggest that for every preschool place per child that was opened, the effect on low birth weight and extreme low birth weight of children born to first-time mothers is mostly negative and significant. The only exception is when considering the most extreme scenario (Scenario 1), in which the included observations are assumed to be of children born with 800 grams. In the latter case, the effect of the reform on extreme low birth weight is not significant. However, the estimated coefficient remains quite stable.

In light of this evidence, my argument is that the estimates presented in Table 6 are not substantially affected by compositional changes. The largest discrepancy between the coefficients estimated with the augmented and the observed samples appear when considering an extreme scenario (Scenario 1) that is highly unlikely considering that the mean birth weight in the observed sample is 3222 grams.

Table 8: Effect of the expansion of preschool places per child on birth outcomes using an augmented sample of first time mothers

Dependent variable	Scenario 1: 800 grams		Scenario 2: 1000 grams		Scenario 3: 2000 grams		Scenario 4: 3000 grams	
	β_2	s.e.	β_2	s.e.	β_2	s.e.	β_2	s.e.
Birth weight (in grams)	10.063	(54.116)	9.566	(53.443)	7.081	(51.035)	4.597	(50.374)
Low birth weight (<2500g)	-0.045*	(0.027)	-0.045*	(0.027)	-0.045*	(0.027)	-0.046*	(0.028)
Very low birth weight (<1500g)	-0.004	(0.014)	-0.004	(0.014)	-0.002	(0.013)	-0.002	(0.013)
Extreme low birth weight (<1000g)	-0.013	(0.012)	-0.011*	(0.006)	-0.011*	(0.006)	-0.011*	(0.006)

Note: * p<.1, ** p<.05, *** p<.01. Table reports results for the estimation of Equation 2 for several dependent variables, assuming different scenarios for missing observations. Estimations use OLS. β_2 corresponds to the estimated coefficient for the independent variable available preschool places per child. Estimations include cohort fixed effects, department fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level.

5.4 Heterogeneous impacts

Given that the expansion was not targeted to a specific group of the population, we may wonder whether there are differential effects in some groups. In this section I explore whether the effects on birth outcomes are specific to a certain group of women or are generalizable. More specifically, I focus on whether the results vary according the socioeconomic level of the department of birth of the mother in the period previous to the reform. It is important to know whether the reform benefited disadvantaged regions more or less than other regions. As a proxy for the disadvantagedness of regions, I use the unemployment rate. Results are reported in Table 9.

In Table 9, I consider heterogeneous effects according to the level of unemployment in the period prior to the reform. I split the sample into departments with higher and lower than the median unemployment in the period 1992-1994. In the full sample, I find that the impact of the reform on extreme low birth weight is negative and significant for women that were born in departments of high unemployment rates in the years prior to the reform while the impact of extreme prematurity is negative and significant for mothers born in a low unemployment department. Potential mechanisms behind these effects could be that, in high unemployment regions, the expansion of preschool places per child had a positive impact on average parental education and on the likelihood that the mother has more than 7 prenatal controls. In low unemployment regions the reform had a positive effect on the likelihood that the mother and the father of the child live together. In the case of first-time mothers, the impact of the reform on the likelihood of extreme low birth weight is negative and significant in high unemployment regions. The effect of more available preschool places per child on extreme prematurity is negative and significant irrespective of whether the mother was born in a high or low unemployment department. Potential mechanisms behind these effects could be that, in low unemployment regions, the expansion of preschool facilities had a positive effect on the likelihood that the mother received prenatal care in the first trimester of pregnancy and that she had more than 7 prenatal controls during pregnancy, while in high unemployment regions the reform had a positive impact on the age of the mother at birth and on the likelihood that the mother had more than 7 prenatal controls during pregnancy.

Table 9: Heterogeneous impacts by unemployment level in department of birth of the mother in the period previous to the reform

Dependent variable	Full sample				First time mothers			
	Low unemployment		High unemployment		Low unemployment		High unemployment	
	β_2	s.e.	β_2	s.e.	β_2	s.e.	β_2	s.e.
Panel A: Birth outcomes								
Birth weight (in grams)	-45.956	(76.388)	16.211	(56.213)	-26.717	(113.559)	31.905	(61.661)
Low birth weight (<2500g)	-0.040	(0.040)	-0.009	(0.030)	-0.047	(0.041)	-0.031	(0.035)
Very low birth weight (<1500g)	0.011	(0.012)	0.001	(0.012)	0.017	(0.018)	-0.007	(0.018)
Extreme low birth weight (<1000g)	-0.001	(0.008)	-0.011*	(0.006)	0.003	(0.012)	-0.017**	(0.008)
Premature (<37 weeks)	-0.041	(0.045)	-0.040	(0.027)	-0.041	(0.050)	-0.042	(0.033)
Very Premature (<32 weeks)	-0.016	(0.017)	-0.006	(0.012)	-0.011	(0.020)	-0.018	(0.017)
Extreme premature (<28 weeks)	-0.012*	(0.006)	-0.007	(0.005)	-0.017**	(0.008)	-0.014*	(0.008)
Panel B: Parental characteristics and prenatal care								
Mother and father of the child live together	0.119*	(0.061)	0.057	(0.046)	-0.040	(0.076)	0.023	(0.056)
Avg education between mother and father	-0.261	(0.311)	0.332**	(0.165)	0.085	(0.402)	0.234	(0.192)
Age of the mother at birth	0.008	(0.063)	0.059	(0.053)	0.154	(0.093)	0.179***	(0.052)
More than 7 prenatal controls during pregnancy	0.067	(0.057)	0.124**	(0.047)	0.175**	(0.072)	0.160***	(0.055)
Care in first trimester	0.054	(0.054)	0.007	(0.046)	0.203**	(0.087)	0.063	(0.058)

Note: * p<.1, ** p<.05, *** p<.01. Table reports results for the estimation of Equation 2 for several dependent variables for groups of mothers born in higher or lower than the median unemployment regions. Estimations use OLS. β_2 corresponds to the estimated coefficient for the independent variable available preschool places per child. Estimations include cohort fixed effects, department fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level.

5.5 Robustness

In the main analysis I have controlled for cohort, department and year-of-birth fixed effects. In this subsection, I test the robustness of my estimates to alternative specifications.

5.5.1 Differential trends by department

First, I report the results of two specifications that allow for the possibility that there are differential trends at the department level. Table 10 shows the results of estimating Equation 3 and Equation 4. The results should be compared to those reported in Table 6.

In the full sample, the results for extreme prematurity are robust to the two alternative specifications that consider time-varying trends at the department level. The negative impact of the reform on low birth weight is still significant when estimating Equation 3 but becomes insignificant when estimating Equation 4. While in Table 6 I observed a negative and significant impact on extreme low birth weight for every preschool place opened per child, the estimate is no longer significantly different from zero. Regarding the impact of the reform on parental characteristics and prenatal care, the estimates of the impact of available preschool places per child on the outcomes considered are robust to the inclusion of a control for the level of available preschool places per child in 1995 and a time trend by department.

In the subsample of first-time mothers, there is a negative and significant impact of the educational reform both on the likelihood of extreme low birth weight and on extreme prematurity, which suggests that the coefficients observed in Table 6 are robust to the inclusion of differential time trends by departments. Panel B, shows that the impact of the reform on prenatal controls is robust to both specifications (detailed in Eq. 3 and Eq. 4), the impact on the average education between the mother and the father of the child is robust when controlling for the level of available preschool places per child in 1995 while the impact on the age of the mother is robust when including a time trend by department.

Table 10: Effects of the expansion of preschool places per child controlling for differential trends by department

Dependent variable	Full sample				First-time mothers			
	Equation 4		Equation 3		Equation 4		Equation 3	
	β_4	s.e.	β_3	s.e.	β_4	s.e.	β_3	s.e.
Panel A: Birth outcomes								
Birth weight (in grams)	28.538	(45.122)	5.051	(54.464)	-16.240	(54.148)	-3.810	(59.858)
Low birth weight <2500g)	-0.035	(0.023)	-0.053*	(0.028)	-0.001	(0.026)	-0.034	(0.032)
Very low birth weight (<1500g)	-0.005	(0.009)	-0.006	(0.012)	-0.001	(0.011)	-0.008	(0.015)
Extreme low birth weight (<1000g)	-0.006	(0.004)	-0.009	(0.005)	-0.009*	(0.005)	-0.015**	(0.007)
Premature (<37 weeks)	-0.032	(0.023)	-0.027	(0.030)	-0.037	(0.027)	-0.045	(0.033)
Very premature (<32 weeks)	-0.005	(0.010)	-0.007	(0.012)	-0.013	(0.012)	-0.021	(0.014)
Extreme premature (<28 weeks)	-0.008**	(0.004)	-0.012**	(0.005)	-0.009*	(0.005)	-0.019***	(0.006)
Panel B: Parental characteristics and prenatal care								
Mother and father of the child live together	0.136***	(0.042)	0.131***	(0.050)	-0.023	(0.042)	-0.052	(0.048)
Average education between mother and father	0.436***	(0.161)	0.275*	(0.163)	0.295*	(0.151)	0.157	(0.187)
Age of the mother at birth	0.060	(0.037)	0.143***	(0.049)	-0.004	(0.043)	0.107**	(0.045)
More than 7 prenatal controls during pregnancy	0.086**	(0.041)	0.144***	(0.045)	0.110**	(0.043)	0.103**	(0.049)
Care in first trimester	0.029	(0.039)	0.088	(0.056)	0.020	(0.041)	0.030	(0.061)

Note: * p<.1, ** p<.05, *** p<.01. Table reports results for the estimation of Equations 3 and 4 for several outcome variables using OLS. β_3 and β_4 correspond to the estimated coefficients for the independent variable available preschool places per child. Estimations include cohort fixed effects, department fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level. Number of observations is 95,450 in the full sample and 52,831 for first time mothers.

5.5.2 Pre-treatment cohorts

As a further robustness check, I restrict the sample to those cohorts that were not exposed to the treatment. The assumption of the identification strategy is that, in the absence of treatment, the trends in outcomes would be parallel between regions that were intensively treated and regions that were less treated. In pre-treatment cohorts, we should expect the common trend assumption to hold. In this subsection, I estimate Equation 2 for each of the outcomes considered using only pre-treatment cohorts. In Table 11 I show that the impact of the reform on the outcomes is mainly insignificant. The only exception is the impact on average parents' education which is negative in the sample of first-time mothers.

Table 11: Effects of the expansion of available preschool places for children per child on pre-treatment cohorts

Dependent variable	All mothers		First time mothers	
	β_2	s.e.	β_2	s.e.
Panel A: Birth outcomes				
Birth weight (in grams)	-120.649	(133.183)	-200.573	(156.328)
Low birth weight (<2500g)	0.094	(0.064)	-0.002	(0.079)
Very low birth weight (<1500g)	0.045	(0.036)	0.099	(0.067)
Extreme low birth weight (<1000g)	-0.005	(0.013)	-0.021	(0.028)
Premature (<37 weeks)	0.026	(0.081)	0.039	(0.117)
Very premature (<32 weeks)	0.007	(0.032)	0.033	(0.061)
Extreme premature (<28 weeks)	0.016	(0.016)	0.010	(0.020)
Panel B: Parental characteristics and prenatal care				
Mother and father of the child live together	0.130	(0.124)	0.157	(0.187)
Average years of education between mother and father	0.527	(0.622)	-1.146*	(0.673)
Age of the mother at birth	-0.092	(0.140)	-0.186	(0.193)
More than 7 prenatal controls during pregnancy	-0.173	(0.121)	-0.176	(0.154)
Care in first trimester	0.040	(0.123)	0.128	(0.125)

Note: * p<.1, ** p<.05, *** p<.01. Estimations include cohort fixed effects, department fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the department and cohort level. Number of observations is 35,570 for the full sample and 14,733 for first time mothers.

6 Conclusion

This paper presents estimates of the effects of additional schooling at the beginning of the school trajectory on health at birth of the next generation. I exploit a schooling reform that involved a large construction of preschool places in Uruguay and that occurred at differential rates by department and time.

Using data of availability of preschool places between 1992 and 2000 and birth outcomes in the period 2008-2015 I find an improvement in health at birth of the offspring of those women that were more exposed to the schooling reform. The results suggest a reduction in extreme low birth weight and extreme prematurity for first-time mothers and extreme prematurity for all females. My estimates are robust to several checks. The findings highlight the importance of education at early years as they show that preschool education has long lasting benefits that can be transmitted across generations. In addition, the evidence in this paper points to preschool education as a way to reduce the intergenerational transmission of poverty due to poor health at birth.

One potential channel of the observed effects is that exposed mothers in my sample are more likely to have more than seven prenatal controls during their pregnancy. Prenatal controls can be regarded as an indicator of whether a woman is willing to invest in the pregnancy and is an indicator of other healthy behaviors (Currie and Moretti, 2003). Another potential channel is fertility. The reform could have affected health at birth by influencing the decision of women to have fewer children of higher quality and to have children at older ages. This result is in line with a large literature that documents an association between education and fertility choices of women (see Strauss and Thomas, 1995).

My findings differ from those in McCrary and Royer, 2011, the only other study to date that examines the effect of additional schooling at the beginning of the school trajectory. My analysis uses a different identification strategy – DiD vs RD – and pertains to a different country – Uruguay vs US. Further research of the effect of mothers starting school earlier on health at birth of the next generation could shed light on the reasons behind the discrepancy of the results.

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Appendix

Table A1: Age of females in the sample by birth cohort and year of observation

Birth cohort	Year							
	2008	2009	2010	2011	2012	2013	2014	2015
1988	20	21	22	23	24	25	26	27
1989	19	20	21	22	23	24	25	26
1990	18	19	20	21	22	23	24	25
1991	17	18	19	20	21	22	23	24
1992	16	17	18	19	20	21	22	23
1993	15	16	17	18	19	20	21	22
1994	.	15	16	17	18	19	20	21
1995	.	.	15	16	17	18	19	20
1996	.	.	.	15	16	17	18	19