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

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1 Introduction

It is commonly held that maternal education is an important determinant of children's health. While numerous studies have documented a positive correlation between mother's schooling and child health (see a review in Grossman, 2006), the evidence showing causal effects is scarce. Most studies on the effects of maternal schooling on infant health look at extensions of schooling at the end of the school trajectory and results are mixed.

This paper studies the effect of an expansion of public preschool facilities in Uruguay on health at birth of the next generation. I use an infrastructure program implemented in the mid 1990's 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 in enrollment of 52% (ANEP, 2007). I evaluate health at birth of the offspring of mothers that were exposed to the reform when they were 4 years old. The identification strategy exploits variation induced by the differential timing and intensity of the construction across regions. I assess the robustness of my estimates to the recent developments in the two-way fixed effects regressions' literature using the method proposed by De Chaisemartin and d'Haultfoeuille (2020).

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, including higher mortality within the first year of life and lower educational attainment and earnings in adulthood (Black et al., 2007).[1] In addition, the literature has found that the health income gradient among adults 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 children's 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 (Becker, 1960). 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 causal effects of maternal schooling on infant health.[2] Most of the evidence comes from studies that look at schooling

[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] A large set of studies document a positive correlation between mother's schooling and child health (Grossman, 2006) but this correlation should, however, not be interpreted causally. Selection bias arises, for example, if

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; Dinçer et al., 2014).[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 on infant health. Preschool education is designed to prepare children for school and fosters the development of cognitive and non-cognitive skills. These early inputs may increase the productivity of investments made later.[4] Evaluations of the long-run impacts of some preschool programs have shown positive effects on schooling. Experimental and non-experimental studies of the Perry Preschool Program and Head Start have found positive effects on outcomes such as educational attainment and earnings (Currie, 2001, Heckman et al., 2010, Garces et al., 2002). In Norway, Havnes and Mogstad (2011) find positive effects of a large-scale expansion of subsidized preschool on educational attainment.

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 the exact date of birth. The authors find that starting school early has only small effects on infant health and does not affect fertility or prenatal behaviors such as smoking rates and the use of 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 during age 4 impacts the health of

mothers of better quality 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.

[4] In the economics literature, Cunha and Heckman (2007) make a strong case that early investments benefit from self-productivity and dynamic complementarities.

her offspring. The main database used in the analysis compiles information from vital statistics natality micro-data for the years 2008-2017. This database has information of pregnancy outcomes and parents' characteristics of all registered births in Uruguay. I combine natality data with a measure of availability of preschool places by region and year that I construct using school level data provided by the National Administration of Public Education.

I find that the the expansion of preschool facilities improved health at birth of the next generation. 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 incidence of extreme prematurity decreases among first-born children of mothers exposed to the preschool expansion. When exploring potential pathways, I find that mothers that were exposed to the reform have more years of completed education and a larger probability of having more than seven prenatal checkups during pregnancy.

My findings also indicate that the expansion of preschool places had an impact on fertility. I find that age of the mother at birth increases and that the probability of motherhood decreases. Importantly, the latter effect appears mainly because of a reduction in teenage pregnancies. Fertility is an issue of interest in its own right, but it can create concerns for the interpretation of health at birth outcomes. Because changes in fertility in the direction observed in this paper can naturally lead to an improvement in health at birth results, I estimate the sensitivity of my health at birth estimates to changes in fertility. Following Lee (2009) I conduct a bounding analysis in which I assume different health at birth scenarios for missing babies. Overall, I find that the estimated coefficients on health at birth are qualitatively the same as those observed in the main analysis.

The remainder of this paper is organized as follows. Section 2 describes the schooling reform used in the analysis. 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. Around half of the country's 3.2 million inhabitants are concentrated in the capital city, Montevideo, and the rest of the population is distributed across 18 other regions. 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's, the Uruguayan Government decided to implement a reform to alleviate two features of the education system in Uruguay: grade retention and early dropout. The main pillar of the reform was the creation of extra preschool places with the aim of achieving universal preschool for children aged 4 and 5 (ANEP-CODICEN, 2000). The hope was that the reform would increase the number of years of schooling, not including preschool years, 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 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 the 1,144 existing primary schools and were allocated across the different Uruguayan regions 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 regions.

The reform was successful in increasing preschool participation. Enrollment and attendance rates for children aged 4 and 5 increased 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 relatively stable (the number of pupils increased from 19846 to 20806). The attendance rates of children aged 4 and 5 increased 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 were around 20% for the lowest income 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 describes the treatment and outcome variables.

3.1 Data Sources

Pregnancy and delivery data comes from the vital statistics natality micro-data for the period 2008-2017. 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 region 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 Enseñanza 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's: (i) location, (ii) enrollment by level, number of groups and group size, and (iii) student's 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 2008-2017. Due to a low number of births for women younger than 15 (less than 1% of the sample), I restrict the sample to mothers older than 14 years of age which leaves 134,140 births of mothers aged 15 to 29 with complete information for the birth outcomes used in the analysis. Table A1 in the Appendix shows the corresponding age for each pair of birth-cohort and year of observation in the sample.

I also restrict the sample to first-time mothers which leaves a final restricted sample of 66,592 observations. This sample includes only women that are giving birth for the first time and, hence, constitutes a more homogeneous group than the full sample. Moreover, 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.

3.2 Treatment Variable

The school-level data enables the construction of a measure for availability of public preschool places for children aged 4.[5] Availability of preschool places per child is a measure for treatment intensity that 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 years old by an average of 25 students per group and dividing this number by

[5]I consider data of schools and kindergartens.

the population of the corresponding age in that department and year.[6] [7] Even though exposure varies according to the department where the mother lived at the age of 4, department of birth is preferable to assign treatment intensity because it is not subject to endogenous migration.

Table 1 shows the availability of preschool places per child by year and department. On average, available preschool places per child were 0.4 for 4-year-olds in the period 1992-2000. The growth of preschool places between 1992 and 2000 averaged 0.3 preschool places per child and it 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), while Rocha increased its availability of preschool places per child by 55% (from 0.34 to 0.53). The availability of preschool places for 5-year-olds also increased in some departments during the reform, but in a considerable smaller magnitude (see Table A2 in the Appendix).

3.3 Outcome Variables

In Table 2 I define the outcome variables used in the analysis. Birth outcomes are the main dependent variables. To measure health at birth, I focus on low birth weight, a measure that generally is considered as an indicator for intrauterine growth retardation during pregnancy and/or being born premature. In particular, I use indicators for low birth weight, very low birth weight, extreme low birth weight, premature, very premature, and extremely premature.[8] I define the latter 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 and paternal characteristics, maternal health behavior, and fertility decisions.

[6] I exclude data from rural schools. These are extremely small schools located in the countryside. On average, rural schools have 4 students in preschool and 22 students in the 6 grades of primary education while other schools have 39 and 188 students respectively. Rural schools represent 45% of schools in my sample, but they cover a very small fraction of students (6% of preschool students).

[7] Population data comes from the Uruguayan Population Projections by year and age provided by the Uruguayan National Institute of Statistics.

[8] Severe cases of low birth weight such as very low birth weight and extreme low birth weight are linked to higher risk of death during the newborn period, poor school performance and adverse outcomes during adulthood (Hack et al., 2002, 1994; Hack and Fanaroff, 1999).

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

Region	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 region 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: Description of variables

Variable	Definition
Birth outcomes	
Birth weight	Weight in grams.
Low birth weight indicators	Binary variables. Each variable 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 at birth.
Prematurity indicators	Binary variables. Each variable 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 extremely premature, respectively.
Parental characteristics	
Mother's years of education	Completed years of education of the mother.
Father's years of education	Completed years of education of the father.

Table 3 shows sample statistics for birth outcomes, maternal and paternal characteristics, and maternal health behavior. The average weight of newborns is 3226 grams and the incidence of low birth weight for the thresholds of 2500, 1500 and 1000 grams is 8%, 1.2% and 0.5% respectively. The likelihood that the baby is born before the 37th, 32nd and 28th week of gestation is 8.8%, 1.3% and 0.5% respectively. The average number of gestational weeks is approximately 38.6 weeks. In terms of maternal and paternal characteristics, both mothers and fathers on average have completed 9 years of education completion. In this sample, females are aged 21 years on average when giving birth for the first time. This value is lower than the population average age of first motherhood in the period 2008-2017 which is 24. Slightly more than half of the mothers report living with the father of the child, which is in accordance with population statistics.[9] In terms of prenatal care, 69% of mothers have a prenatal control in the first trimester of pregnancy. On average, 78% of first-time mothers visit the doctor more than 7 times during pregnancy.

[9]According to the Uruguayan National Survey of Nutrition, Child development and Health of 2018, the percentage of first-time mothers younger than 28 years that live with the father of the child during the first year after giving birth is 53%.

Table 3: Descriptive statistics

	Mean	s.d.	N
Birth outcomes			
Weight (in grams)	3226.138	553.851	66592
Low birth weight (<2500g)	0.079	0.270	66592
Very low birth weight (<1500g)	0.012	0.110	66592
Extreme low birth weight (<1000g)	0.005	0.068	66592
Premature (<37 weeks of gestation)	0.088	0.283	66592
Very Premature (<32 weeks of gestation)	0.013	0.115	66592
Extreme Prematurity (<28 weeks of gestation)	0.005	0.068	66592
Gestational weeks	38.570	1.952	66592
Parental characteristics			
Mother's education in years	9.266	2.607	66592
Father's education in years	8.870	2.586	37794
Average years of education between mother and father	9.314	2.238	37794
Mother's age at birth	20.557	3.003	66592
Mother and father of child live together	0.544	0.498	66592
Maternal health behavior			
Prenatal care in first trimester of pregnancy	0.693	0.461	66592
More than 7 prenatal checkups during pregnancy	0.781	0.413	66592

Note: s.d.=standard deviation, N=number of observations. 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 checkups=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 children of exposed mothers. 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 region and cohort fixed effects: region fixed effects control for constant characteristics at the region level that are fixed over time and cohort fixed effects control for unobserved differences across cohorts. I also include year of child's birth fixed effects to control for common effects shared by mothers giving birth at the same time such as the economic conditions or particular policies.

I 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 using Ordinary Least Squares (OLS):

$$Y_{icdt} = \alpha_1 + \delta_{1d} + \gamma_{1c} + \rho_{1t} + \beta_1 Stock_{cd} + \varepsilon_{icdt} \tag{1}$$

where Y_{icdt} is the outcome of interest for the birth of child i, whose mother was born in cohort c and region d, and is observed in year t; δ_{1d} are region fixed effects; γ_{1c} are cohort fixed effects and ρ_{1t} are year of child's birth fixed effects; and $Stock_{cd}$ is a measure for the availability of preschool places per child. β_1 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 region times cohort level.

The reform may have influenced decisions such as the number of children and the timing of childbearing. To study the impact of the reform on fertility 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 using the following equation:

$$F_{cd} = \alpha_2 + \delta_{2d} + \gamma_{2c} + \beta_2 Stock_{cd} + \varepsilon_{cd} \tag{2}$$

where F_{cd} corresponds to total fertility or fertility by age. Total fertility is defined as the number of first-borns by cohort and region until 2015 divided by the population of the corresponding cohort. This measure is aggregated at the region-cohort level. Fertility by age is the number of first-borns by age, cohort and region until 2015 divided by the population of the corresponding cohort. β_2 captures the average effect of an extra place available per child on the outcome variable. Standard errors (ε_{cd}) are clustered at the region times cohort level. I also adjust standard errors to account for multiple hypotheses using the Romano-Wolf correction described in **???** and report adjusted p-values.[10]

The identification strategy relies on the parallel trends assumption, which implies that the trend in the outcome variable should not have been systematically different in regions where the program constructed more preschool places and regions where the program constructed fewer preschool places prior to the expansion policy. In other words, the main identification assumption is that in the absence of an increase in the availability of preschool places, changes in health at birth would not have been systematically different between mothers from regions/cohorts with low versus high exposure. To verify the robustness of my estimates, I will conduct several checks that I describe below.

The treatment intensity of implementation of the policy could be correlated with specific pre-treatment trends between regions. 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 region in my estimations. Equation 1 transforms into:

$$Y_{icdt} = \alpha_3 + \delta_{3d} + \gamma_{3c} + \rho_{3t} + \beta_3 Stock_{cd} + \sum_{d=1}^{19}(1(D=d) * t_c)\phi_{3d} + e_{icdt} \tag{3}$$

where $1(D=d) * t_c$ represents the interaction of a dummy for region d and a cohort indicator (t_c) that takes values from 1 (for year 2008) to 6 (for year 2013).

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 region-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 region in 1995 with fixed effects by cohort. Equation 1 is adjusted in the following way:

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

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

Finally, I perform additional placebo regressions to verify the robustness of my estimates. I estimate Equation 1 using pre-treatment cohorts. If trends between regions with different treatment intensity are the same in the pre-treatment period, the expectation is that there

[10] I consider two families of outcomes: one that includes the outcome birth weight and the low birth weight indicators and the other that includes the prematurity indicators.

should be no impact of the expansion of preschool places on health at birth on those years.

A recent literature has questioned the validity of regressions using two-way fixed effects. These papers point out that in such identification strategy, if only common trends are assumed, the estimated treated effect is a weighted sum of the effect of treatment in each group and time period. Some of the weights can be negative and this may bias or even change the sign of the true average treatment effect. De Chaisemartin and d'Haultfoeuille (2020), propose computing the DID_M estimator, that only relies on the common trends assumption and is valid even if the treatment effect is heterogeneous over time and between groups. The DID_M estimator can be computed using the *didmultiple_gt* Stata package (de Chaisemartin et al., 2019). In Section 5.4.2 I analyze the sensitivity of my estimates to this adjustment.

5 Results

In this section I present the results of the analysis. First, I provide evidence of the effects of the reform on health at birth. Second, I report the effects of the expansion of preschool places on potential pathways such as parents' characteristics, prenatal care and fertility. Third, I study heterogeneous effects. Finally, I analyze the robustness of my estimates.

5.1 Effects of the Expansion of Preschool Places on Birth Outcomes

Table 4 shows the results of estimating Equation 1 (Column (1)), Equation 3 (Column (2)), and 4 (Column (3)) for birth outcomes. The expansion of preschool places implied an improvement in health at birth as measured by extreme prematurity. For every preschool place opened per child, the probability of giving birth to a child before week 28 of gestation decreased by 1.3 percentage points (see Column (1)). This effect maintains significance at the 1% level when considering standard errors that are adjusted for multiple hypothesis testing,[11] when controlling for differential trends by region and when controlling for possible omitted time-varying region-level factors that may be correlated with pre-program enrollment rates. As the average increase in preschool places between the years 1992 and 2000 is 0.3, the magnitude of the coefficient can be interpreted as follows: an increase of 0.3 preschool places per child leads to a decrease in extreme prematurity of 0.4 (0.013 x 0.3) percentage points. There is also a significant reduction in the likelihood of low birth weight in the range of 4.5 percentage points when estimating Equations 1 and 4, and a significant reduction in the likelihood of extreme low birth weight in the range of 1 percentage points when estimating Equation 3, but these effects become insignificant when estimating Equation 3, and 1 and 4, respectively.

[11]The effect also maintains significance at the 1% level when grouping all outcomes together in one family.

Table 4: Effects of the expansion of preschool places per child on birth outcomes

Dependent variable	(1)	(2)	(3)
Birth weight (in grams)	-14.249	-27.723	-10.283
	(46.742)	(53.966)	(46.727)
	[0.911]		
Low birth weight (<2500g)	-0.043*	-0.034	-0.052**
	(0.025)	(0.031)	(0.025)
	[0.089]		
Very low birth weight (<1500g)	-0.003	-0.009	-0.006
	(0.011)	(0.014)	(0.011)
	[0.910]		
Extreme low birth weight (<1000g)	-0.008	-0.012*	-0.007
	(0.006)	(0.007)	(0.006)
	[0.158]		
Premature (<37 weeks)	-0.019	-0.032	-0.019
	(0.025)	(0.032)	(0.025)
	[0.604]		
Very premature (<32 weeks)	-0.005	-0.018	-0.006
	(0.012)	(0.014)	(0.011)
	[0.604]		
Extreme prematurity (<28 weeks)	-0.013***	-0.018***	-0.012***
	(0.005)	(0.006)	(0.005)
	[0.010]		
Interaction of region fixed effects and cohort indicator	No	Yes	No
Interaction of stock in 1995 and fixed effects by cohort	No	No	Yes

Note: Table reports results for the estimation of Equation 1 (Column (1)), Equation 3 (Column (2)), and 4 (Column (3)) for several dependent variables using OLS. Estimations include cohort fixed effects, region fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the region times cohort level. I report p-values that are adjusted for multiple hypothesis testing in squared brackets. Number of observations is 66592. * p<.1, ** p<.05, *** p<.01.

5.2 Mechanisms

5.2.1 Maternal and paternal characteristics and prenatal care

Table 5 shows the results of estimating the effect of the preschool expansion on parental characteristics as well as on prenatal care. Mother's education increased by 0.6 years (see Column (1)) for every preschool place opened per child.[12] The effect maintains significance at 1% when controlling for the interaction of the stock of preschool places in 1995 with fixed effects by cohort and at 10% when controlling for the interaction of region fixed effects with a cohort indicator. Moreover, the expansion of preschool increased the age at motherhood by 0.13 years for every preschool place that was opened per child. In terms of father's characteristics, there is no effect of the reform on the number of completed years of education, and the effect of the reform on the probability that mother and father live together is not robust to the different specifications considered in the analysis. Regarding prenatal care, the likelihood that the mother had more than 7 prenatal checkups increased by 10 percentage points per preschool place opened per child (or by 3 percentage points - 0.10 x 0.3 - for every 0.3 preschool places opened per child). These effects are robust to the specifications detailed in Equation 3 and Equation 4.

[12] 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 siblings and that this works through a fall in grade retention rates in the school trajectory and a reduction in dropout rates. The effect of the expansion of preschool places at age 15 seems larger than the one found in this paper, suggesting that the gap between treated and untreated children widens during adolescence.

Table 5: Effects of the expansion of preschool places per child on maternal and parental characteristics and prenatal care

Dependent variable	(1)	(2)	(3)
Mother's years of education	0.613***	0.379*	0.597***
	(0.182)	(0.196)	(0.191)
Father's years of education	0.370	-0.107	0.326
	(0.253)	(0.255)	(0.252)
Average years of education of mother and father	0.365*	0.036	0.272
	(0.203)	(0.213)	(0.207)
Mother and father live together	0.094*	-0.058	0.153***
	(0.051)	(0.043)	(0.050)
Age of the mother at birth	0.126***	0.091**	0.139***
	(0.040)	(0.037)	(0.042)
More than 7 prenatal checkups during pregnancy	0.101***	0.075*	0.114***
	(0.033)	(0.040)	(0.034)
Care in first trimester	0.051	0.013	0.058
	(0.044)	(0.050)	(0.046)
Interaction of region fixed effects and cohort indicator	No	Yes	No
Interaction of stock in 1995 and fixed effects by cohort	No	No	Yes

Note: Table reports results for the estimation of Equation 1 (Column (1)), Equation 3 (Column (2)), and 4 (Column (3)) for several dependent variables using OLS. Estimations include cohort fixed effects, region fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the region and cohort level. Number of observations is 66592 for all outcomes except for father's years of education which is 37794. * p<.1, ** p<.05, *** p<.01.

5.2.2 Fertility

Table 6 shows the effect of the expansion of preschool places per child on the probability of motherhood. I estimate Equation 2 and report the pooled coefficient for each age and for the overall sample. The number of observations in each regression are reported in the last column. For example, 171 observations were used in the regression that considers fertility of women aged 19 as the outcome variable. There is one observation for each combination of the 19 regions and 9 cohorts. I also show the mean number of women that gave birth to their first child at each age. For example, 2.4% of all women born between 1988 and 1996 that gave birth to their first child between 2008 and 2017, were aged 27 when giving birth (see the third column under age

27).[13] The estimate of the reform on the probability of motherhood when considering all ages is negative and significant at the 5% level. For every preschool place opened per child, overall fertility rate decreases by 11 percentage points. The effect essentially comes from a reduction in teenage births (at ages 16 and 17) and at age 28. Indeed, when I estimate the impact of the reform on overall fertility without considering births of females aged 16, 17 and 28, I find that the coefficient is insignificant.

[13]Note that only those born in 1988-1990 could have given birth at age 27 between 2008 and 2017 (see Table A1 in the Appendix). Women born in later cohorts are observed at younger ages in my sample.

Table 6: Effect of the expansion of preschool places per child on probability of motherhood

Age	(1)	(2)	(3)	(4)
Panel A: Fertility by age				
15	-0.008	0.012	0.011	95
16	-0.039***	0.013	0.027	114
17	-0.037**	0.017	0.034	133
18	-0.023	0.016	0.040	152
19	-0.022	0.018	0.042	171
20	-0.019	0.014	0.042	171
21	-0.009	0.014	0.038	171
22	-0.004	0.012	0.034	152
23	-0.000	0.014	0.031	133
24	0.016	0.014	0.026	114
25	-0.008	0.027	0.026	95
26	0.025	0.027	0.025	76
27	0.051	0.035	0.024	57
28	-0.104**	0.038	0.020	38
29	0.007	0.017	0.011	19
Panel B: Overall fertility				
All ages	-0.110**	0.054	0.320	171
All ages except age 16, 17 and 28	-0.026	0.047	0.271	171
Panel C: Fertility when including additional births				
Age 16 including added births	-0.027	0.016	0.031	114
Age 17 including added births	-0.031	0.019	0.038	133
Age 28 including added births	0.136	0.166	0.032	38
All ages including added births	-0.029	0.060	0.328	171

Note: 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 2. Column (1) reports the coefficient that corresponds to the independent variable available preschool places per child. Estimations include cohort fixed effects and region fixed effects. Standard errors, reported in Column (2), are clustered at the region times cohort level. Sample means are reported in Column (3). Number of observations included in the estimations are reported in Column (4). * p<.1, ** p<.05, *** p<.01.

Changes in fertility decisions could be a potential channel by which education can affect health at birth. In order to analyze the extent to which changes in fertility explain the findings reported in Table 4, I conduct two analyses. First, I estimate the impact of the reform on health at birth of the next generation excluding mothers that gave birth at ages 16, 17 and 28. Second, I conduct a bounding analysis. Overall, the takeaway from these analyses is that fertility is not a strong pathway underlying the health at birth estimates. A detailed description of both analyses can be found below.

When considering first-time mothers of all ages excluding those aged 16, 17 and 28, the impact of the reform on fertility is non-significant (see Table 6). In this sample we should not expect that fertility is a mechanism behind the observed effects on health at birth. In Table 7 I estimate the effect of the reform on health at birth of first-borns of mothers that are not aged 16, 17 and 28. The results are qualitatively very similar to those found when including mothers of all ages in the estimation. Hence, my interpretation is that fertility does not play a significant role when considering the whole age distribution.

Table 7: Effects of the expansion of preschool places per child on birth outcomes excluding mothers aged 16, 17 and 28

	Coefficient	s.e.
Birth weight (in grams)	33.506	(53.912)
Low birth weight (<2500g)	-0.051*	(0.029)
Very low birth weight (<1500g)	-0.009	(0.012)
Extreme low birth weight (<1000g)	-0.010*	(0.006)
Premature (<37 weeks)	-0.043	(0.030)
Very premature (<32 weeks)	-0.008	(0.012)
Extreme prematurity (<28 weeks)	-0.011**	(0.005)

Note: Table reports results for the estimation of Equation 1 for several dependent variables using OLS. Estimations include cohort fixed effects, region fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the region times cohort level. Number of observations is 66592. * p<.1, ** p<.05, *** p<.01.

The second approach to analyze the sensitivity of my estimates to changes in fertility is a bounding analysis. Given the negative impact of the reform on fertility, in the absence of the reform, I would expect to have observed more births of first-time mothers.[14] To bound the effect of the reform on birth outcomes, I add more children to the regions where the expansion was

[14]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 4. In that case, my estimates of the effect of the reform on low birth weight and 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.

largest. In particular, I include births to regions that more than doubled their availability of preschool places per child between 1992 and 2000. In Table A3 I report the number of children that I add to each of these regions.[15] When estimating the effect of the reform on the age-specific fertility rate for ages 16, 17 and 28 including added births, the effect is no longer significant (see Table 6). In addition, the effect on the overall fertility rate when including the additional children is no longer significant either.

Next, I assume different birth-weight-scenarios for the observations that were added. In particular, I assume the following five situations regarding the health at birth of the children that I incorporate: (1) birth weight is 900 grams and gestational length is 27 weeks, (2) birth weight is 1400 grams and gestational length is 31 weeks, (3) birth weight is 2400 grams and gestational length is 36.5 weeks, (4) birth weight is 3200 grams and gestational length is 39 weeks, and (5) birth weight is 4000 grams and gestational length is 40 weeks. In this sense, scenario (1) is an extreme case in which the unobserved births are very unhealthy while scenario (5) assumes the opposite.

Table 8 shows the results of estimating Equation 1 using the augmented sample of first-time mothers for the different birth-weight-scenarios of the missing observations. As outcome variables, I consider health at birth indicators. In Scenario 4 and Scenario 5, the results are qualitatively the same than those reported in Table 4: the incidence of low birth weight and extreme prematurity decreases among mothers that were exposed to the reform. In Scenarios 2 and 3, the effects of the reform on extreme prematurity remains but the effect on low birth weight disappears. When including observations of extremely unhealthy babies (Scenario 1), all the effects observed in Table 4 disappear. There are some significant effects but these have the opposite sign than expected. Considering that the mean birth weight among mothers aged 16, 17 and 28 is 3173 grams, my preferred Scenario is number 4. In light of the evidence of Table 8 and the analyses shown above, my conclusion is that fertility is not a strong pathway underlying the health at birth estimates.

[15] I assume that the births that I include belong to mothers that where born in 1996 and that gave birth in 2015.

Table 8: Effects of the expansion of preschool places per child on birth outcomes considering different scenarios for missing observations

	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
	900 grams	1400 grams	2400 grams	3200 grams	4000 grams
	27 gestational weeks	31 gestational weeks	36.5 gestational weeks	39 gestational weeks	40 gestational weeks
Birth weight (in grams)	-388.369*	-306.675*	-143.288*	-12.578	118.131
	(216.459)	(170.991)	(84.463)	(45.332)	(90.441)
Low birth weight (<2500g)	0.108	0.108	0.108	-0.055**	-0.055**
	(0.091)	(0.091)	(0.091)	(0.025)	(0.025)
Very low birth weight (<1500g)	0.159*	0.159*	-0.004	-0.004	-0.004
	(0.093)	(0.093)	(0.011)	(0.011)	(0.011)
Extreme low birth weight (<1000g)	0.155*	-0.008	-0.008	-0.008	-0.008
	(0.092)	(0.006)	(0.006)	(0.006)	(0.006)
Premature (<37 weeks)	0.129	0.129	0.129	-0.035	-0.035
	(0.087)	(0.087)	(0.087)	(0.026)	(0.026)
Very premature (<32 weeks)	0.156*	0.156*	-0.007	-0.007	-0.007
	(0.092)	(0.092)	(0.012)	(0.012)	(0.012)
Extreme prematurity (<28 weeks)	0.150	-0.013***	-0.013***	-0.013***	-0.013***
	(0.093)	(0.005)	(0.005)	(0.005)	(0.005)

Note: Standard errors, reported in parentheses, are clustered at the region times cohort level. * p<.1, ** p<.05, *** p<.01.

5.3 Heterogeneous Impacts

Given that the expansion was not targeted to a specific group of the population, in this subsection I explore whether there are differential effects in some groups. I study 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 to the socioeconomic level of the region of birth of the mother in the period previous to the reform. This analysis sheds light on 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. I split the sample into regions with higher and lower than the median unemployment rate in the period 1992-1994.

Table 9 reports results from estimating heterogeneous impacts by unemployment level in the region of birth of the mother in the period previous to the reform. I find that there is a statistically significant difference in the likelihood of extreme low birth weight between mothers that were born in regions with different socioeconomic levels. The effect of the reform on extreme low birth weight was larger in absolute terms among mothers born in regions with high unemployment. For the other outcomes I cannot reject that the effects are the same across the socioeconomic level of the region of birth of the mother, however, heterogeneous effects cannot be ruled out entirely due to a low statistical power to detect differential effects.

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

	Low unemployment		High unemployment		Difference	
	Coefficient	s.e.	Coefficient	s.e.	Difference	s.e.
	(1)	(2)	(3)	(4)	(5)	(6)
Birth weight (in grams)	-75.416	(101.465)	38.845	(48.908)	114.261	(112.016)
Low birth weight (<2500g)	-0.038	(0.054)	-0.043	(0.030)	-0.005	(0.062)
Very low birth weight (<1500g)	0.003	(0.018)	-0.003	(0.015)	-0.006	(0.023)
Extreme low birth weight (<1000g)	0.011	(0.010)	-0.015**	(0.007)	-0.026**	(0.012)
Premature (<37 weeks)	-0.028	(0.046)	-0.032	(0.030)	-0.004	(0.055)
Very Premature (<32 weeks)	-0.021	(0.020)	-0.012	(0.015)	0.009	(0.025)
Extreme prematurity (<28 weeks)	-0.017**	(0.007)	-0.012*	(0.007)	0.005	(0.010)

Note: Table reports results of estimating Equation 1 for several dependent variables for groups of mothers born in higher or lower than the median unemployment regions. Estimations use OLS. Estimations include cohort fixed effects, region fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the region times cohort level. * p<.1, ** p<.05, *** p<.01.

5.4 Robustness

In the main analysis I have controlled for cohort, region and year-of-birth fixed effects, and I have also considered a specification that includes differential trends by region and a specification that controls for the interaction of the stock of preschool places in 1995 with fixed effects by cohort. In this subsection, I test the robustness of my estimates to alternative specifications.

5.4.1 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. If trends between regions with different treatment intensity are the same in pre-treatment cohorts, there should be no association of the expansion of preschool places with the health at birth of children born to women who were 4 years old before the reform. In this subsection, I estimate Equation 1 using only pre-treatment cohorts. The sample contains females born in cohorts 1988-1990. These women were aged 17 to 29 when observed giving birth. Those born in 1988 were 7 years old when the expansion takes place.

In Table 10 I show that the impact of the reform on prematurity outcomes is insignificant and that I cannot reject equality from zero. For birth weight outcomes I find a pair of significant effects but coefficients have the opposite sign than expected.

Table 10: Effects of the expansion of available preschool places for children per child on pre-treatment cohorts

Dependent variable	Coefficient	s.e.
Birth weight (in grams)	-405.592***	(139.808)
Low birth weight (<2500g)	0.022	(0.077)
Very low birth weight (<1500g)	0.101*	(0.052)
Extreme low birth weight (<1000g)	-0.014	(0.023)
Premature (<37 weeks)	0.128	(0.101)
Very premature (<32 weeks)	0.040	(0.048)
Extreme prematurity (<28 weeks)	0.006	(0.017)

Note: Table reports results of estimating Equation 1 for several dependent variables using only pre-treatment cohorts. Estimations include cohort fixed effects, region fixed effects and year fixed effects. Standard errors, reported in parentheses, are clustered at the region times cohort level. Number of observations is 18429. * p<.1, ** p<.05, *** p<.01.

5.4.2 DID_M estimator

Two-way fixed effects has been widely-used as econometric method in recent years. De Chaisemartin and d'Haultfoeuille (2020) conduct a survey of all empirical papers between 2010 and 2012 in the American Economic Review and find that 20% of them use two-way fixed regressions to measure the effect of a treatment on an outcome.[16] In two-way fixed effects regressions one would be typically regressing an outcome that is aggregated at the group and time level on group fixed effects, time fixed effects and an independent variable that is aggregated at the group and time level.

A recent body of work questions the validity of the two-way fixed effects estimator in the presence of heterogeneous treatment effects. De Chaisemartin and d'Haultfoeuille (2020) show that if treatment effects are not constant between units or over time then two-way fixed effects regressions identify the expectation of a weighted sum of the treatment effects in every group and every time period. Some of the weights are strictly negative and this may lead to a strictly negative estimated effect even if the treatment effect is strictly positive in every group and at every time period. When the treatment is not binary, negative weights arise from the fact that the identification strategy compares the outcome evolution in groups whose treatment increases more and in groups where treatment increases less (**?**).

De Chaisemartin and d'Haultfoeuille (2020) propose an alternative estimator DID_M whose building blocks are difference in differences that compare the outcome evolution in groups going from untreated to treated in both dates (switchers in) and groups untreated at both dates (never treated) and differences in differences comparing the outcome evolution in groups going from treated to untreated in both dates (switchers out) and groups that are treated at both dates (always treated). Therefore DID_M compares switchers to non-switchers making sure that the controls used for a switcher have the same treatment as a switcher in $t-1$. This ensures that the estimator only relies on parallel trends rather than homogeneous treatment effects. The estimator identifies the treatment effect of the switchers at the time they switch.

To analyze the sensitivity of my estimates to the adjustment proposed in De Chaisemartin and d'Haultfoeuille (2020) I compute the DID_M estimator using the *didmultiple_gt* Stata package (de Chaisemartin et al., 2019). In the context of this paper, the DID_M is a weighted average of diff-in-diff estimators comparing the evolution of health at birth between cohort $c-1$ to cohort c, in regions whose treatment changes from $stock_{cd}$ to some other value from $c-1$ to c, and in regions whose treatment is equal to $stock_{cd}$ for both cohorts. The estimator uses regions whose treatment does not change between consecutive cohorts as controls.

Given that my treatment is continuous, it is not possible to find any pair of consecutive cohorts between which the treatment of at least one region remains perfectly stable. de Chaise-

[16] **?** update this survey using a slightly different survey method and find that 26% of the most cited American Economic Review papers between 2015 and 2019 use two-way fixed effects regressions.

martin et al. (2019) propose to specify a threshold of stable treatment so that the *didmultiple_gt* Stata command can use that value to determine which regions are used as controls. Moreover, when the treatment takes many values, de Chaisemartin et al. (2019) propose to bin some values of the treatment together when determining the groups whose outcome evolution are compared.

Table 11 shows the effects of the expansion of preschool places per child using the DID_M estimator. My preferred threshold of stable treatment is 0.09 which corresponds to the average increase in the availability of preschool places between one cohort and the next in regions that increased the stock of preschool places for children aged 4. I also consider other thresholds for robustness. The treatment variable is grouped in quartiles. By and large, the results of the DID_M estimator qualitatively coincide with those of the main analysis of this paper. Consistently with the fact that two-way fixed effects regressions bias coefficients towards zero, the magnitude of the effects in Table 11 is slightly larger. The effect on extreme prematurity using the DID_M estimator is in the range of -0.3 while the one found with a two-way fixed effects regression was -0.2. When using the methodology of de Chaisemartin et al. (2019), I find that for every 0.3 preschool places opened per child extreme prematurity decreases by 0.8 (0.028 x 0.3) percentage points.

Table 11: Effects of the expansion of preschool places per child using the DID_M estimator

	Threshold of stable treatment				
	0.080	0.085	0.09	0.095	0.10
	(1)	(2)	(3)	(4)	(5)
Birth weight	-35.368	-104.615	-94.380	-68.874	-150.635
	(110.333)	(102.768)	(61.047)	(66.709)	(101.628)
Low birth weight	-0.119**	-0.038	-0.042	-0.063	0.020
	(0.059)	(0.049)	(0.049)	(0.050)	(0.075)
Very low birth weight	-0.009	0.002	-0.001	-0.011	-0.021
	(0.048)	(0.029)	(0.027)	(0.037)	(0.030)
Extreme low birth weight	-0.011	-0.011	-0.013	-0.018**	-0.025
	(0.023)	(0.015)	(0.023)	(0.008)	(0.020)
Premature (<37 weeks)	0.036	-0.012	-0.007	-0.022	0.006
	(0.129)	(0.053)	(0.044)	(0.053)	(0.040)
Very premature (<32 weeks)	-0.035	-0.021	-0.025	-0.033	-0.039
	(0.042)	(0.039)	(0.031)	(0.027)	(0.034)
Extreme prematurity (<28 weeks)	-0.023**	-0.024	-0.028**	-0.032**	-0.039**
	(0.011)	(0.016)	(0.012)	(0.012)	(0.019)

Note: Table shows results of the expansion of preschool places on birth outcomes using the DID_M estimator as suggested in De Chaisemartin and d'Haultfoeuille (2020). To compute the DID_M estimator, the treatment variable (availablility of preschool places per child aged 4) is grouped into quartiles. The preferred threshold of stable treatment is 0.09 (Column (3)), but I also report results for other thresholds in Columns (1)-(2) and (4)-(5) for robustness.

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 region and time.

Using data of availability of preschool places between 1992 and 2000 and birth outcomes in the period 2008-2017 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 prematurity for first-time mothers. My estimates are robust to several checks, including the latest advances in the two-way fixed effects methodology.

This paper adds to the literature in several ways. First, it contributes to a nascent literature on the the intergenerational effects of early childhood large-scale interventions. Alongside Barr and Gibbs (2022) which provides some of the first evidence whether the effects of a widely offered early childhood program in the US transfer across generations, this paper shows that investments in early childhood can break the cycle of poverty in the context of a developing country.[17] Second, it contributes to the literature on the effects of maternal education on infant health. While numerous studies have documented a positive correlation between mother's schooling and child health, the evidence showing causal effects is scarce. Moreover, most studies on the effects of maternal schooling on infant health look at extensions of schooling at the end of the school trajectory. The only paper that focuses on the effect of mothers starting school earlier on infant health is McCrary and Royer (2011). I look at the effects of an expansion at an even earlier grade of education than analyzed in previous literature.

The findings from this paper highlight the importance of education at early years as a way to reduce the intergenerational transmission of poverty due to poor health at birth. Potential channels of the observed effects is that exposed mothers in my sample have more years of completed education and are more likely to have more than seven prenatal checkups during their pregnancy. Prenatal checkups 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).

The preschool reform could have affected health at birth by influencing the decision of women to have fewer children and to have children at older ages. I find that the expansion of preschool facilities increased aged at motherhood and decreased pregnancies, especially among teenagers. This result is in line with a large literature that documents an association between education

[17]Related studies include (Akresh et al. (2018), Mazumder et al. (2019)) which show evidence that increasing a mother's educational attainment produces improvements in her children's test scores and schooling duration using data from Indonesia and Rossin-Slater and Wust (2020) which find educational attainment effects in the first generation of a Danish preschool and nurse home-visiting program that persist in the second generation.

and fertility choices of women (see Strauss and Thomas, 1995). Women having fewer children could explain why children are born with higher quality. In this paper I provide evidence that when I exclude the effect on fertility at specific age groups, the observed effect on health at birth remains, suggesting that this channel does not explain the findings.

Interestingly, 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. I find that the effect of girls starting school earlier on health at birth is positive while McCrary and Royer (2011) find that treated and control females give birth to children of similar health. I argue that the improvements in child health may come from increases in prenatal care while McCrary and Royer (2011) do not find any changes in prenatal behavior due to the increased schooling. Several reasons could explain why both studies show different findings. On the one hand, the outcome variables considered are somewhat different. While my study finds intergenerational effects of education on the likelihood of extreme prematurity, McCrary and Royer (2011) only focus on low birth weight and prematurity and find no effects for these margins. On the other hand, the setup in both studies is different. McCrary and Royer (2011) claim that their results may be difficult to generalize to other populations due to specific characteristics of their study. In their setup, the authors focus on mothers that give birth before the age of 23 and that their estimates may disproportionately reflect the experience of women from low socioeconomic backgrounds. In my study I consider slightly older mothers as well as women of broader socioeconomic contexts. Lastly, my analysis pertains to a different country than the one considered in McCrary and Royer (2011). I focus on the case of Uruguay, instead of United States (US), where baseline health at birth outcomes and prenatal behavior are different. The likelihood of low birth weight is higher in my sample (8%) than in the US sample (6%) and first-time mothers in the US sample receive more prenatal care.

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Appendix

The tables included in this section supplement the information in the main text. The first table shows the age of females in the sample according to their birth cohort and the year they are observed. The second table shows the availability of preschool places per child by year and region for 5-year-old. The third table shows the number of observations added to the sample to perform the bounding analysis.

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	2016	2017
1988	20	21	22	23	24	25	26	27	28	29
1989	19	20	21	22	23	24	25	26	27	28
1990	18	19	20	21	22	23	24	25	26	27
1991	17	18	19	20	21	22	23	24	25	26
1992	16	17	18	19	20	21	22	23	24	25
1993	15	16	17	18	19	20	21	22	23	24
1994	.	15	16	17	18	19	20	21	22	23
1995	.	.	15	16	17	18	19	20	21	22
1996	.	.	.	15	16	17	18	19	20	21

Table A2: Availability of preschool places per child by year and region for 5-year-olds

Region	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 region 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).

Table A3: Number of births added to the sample to perform bounding analysis

Region	Age group		
	16	17	28
1	0	0	0
2	25	30	30
3	44	40	40
4	25	32	32
5	0	0	0
6	0	0	0
7	0	0	0
8	0	0	0
9	0	0	0
10	25	11	11
11	28	18	18
12	49	51	51
13	26	21	21
14	0	0	0
15	0	0	0
16	53	57	49
17	0	0	0
18	0	0	0
19	46	48	19