Preschool and Maternal Labor Supply: Evidence from a Regression Discontinuity Design

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Abstract: Employment rates are relatively lower for mothers with young children in developing countries. This paper analyzes how preschool attendance affects maternal labor supply in Argentina. Using pooled household surveys, we show that four year-olds with birthdays on June 30 have sharply higher probabilities of preschool attendance than children born on July 1, given enrollment-age rules. Regression-discontinuity estimates using this variation suggest that preschool attendance of the youngest child in the household increases maternal labor supply in the intensive and extensive margins. We find no effect on maternal labor outcomes when a child that is not the youngest in the household attends preschool.

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1. Introduction

After the second World War, female labor participation rates rose steadily in the developed and developing world. However, participation rates in many countries are still relatively low for mothers with young children. Not surprisingly, expanding preschool education is an oft-cited goal in both developed countries (OECD, 2002) and Latin America (Myers, 1995; Schady, 2006). It provides an implicit child care subsidy, while also, perhaps, improving child outcomes (Blau and Currie, 2006). While a subsidy specifically designed to achieve one of these goals will usually be relatively ineffective at accomplishing the other goal, the hope is that free public preschool could attain both. Nonetheless, the empirical evidence on the effects of pre-primary education is still limited, especially for developing countries.[1]

Exploiting a natural experiment for Argentina, Berlinski et al. (2006) find a positive effect of pre-primary school attendance on third grade standardized Spanish and Mathematics test scores and on primary school pupils' behavioral outcomes such as attention, effort, class participation, and discipline. Berlinski et al. (2007), using data from the Uruguayan household survey that collects retrospective information on preschool attendance, find small gains in school attainment from preschool attendance at early ages that are magnified with age. Identification relies on within household variability in the context of a rapid expansion in the supply of pre-primary places. Less is known, however, about the effects of preschool on maternal labor supply.

[1]There is, however, substantial empirical evidence for the U.S. that intensive early education interventions targeted specifically to disadvantaged children yield benefits in the short and in the long run (see, among an extensive literature, the surveys by Blau and Currie, 2006 and Currie, 2001). On the limited evidence in Latin America, see Schady (2006).

A major challenge in identifying the causal effect of pre-primary school attendance on parental outcomes is non-random selection into early education. To address this problem, our paper employs the fact that Argentine children—as in many countries—must reach a given age prior to a preschool enrollment cutoff date. The school year extends from March to December, and enrollment in the final year of preschool is mandatory for children that turn five years old by June 30. Children born on July 1 must wait one year to enroll in kindergarten. Using pooled household surveys that report exact birth date, we show that children born on July 1 have sharply lower probabilities, by about 0.3, of attending school. We exploit this discontinuity in the probability of attendance to identify the effect of early school attendance on maternal labor supply.

The parameters we study, however, differ from those of much research in the childcare and female labor supply literature. Though many studies have estimated the sensitivity of maternal employment to child care costs, their elasticity estimates cannot be easily generalized to predict the effects of expanding preschool education on maternal labor supply (see Anderson and Levine, 2000; Blau and Currie, 2006).[2] Additionally, in the absence of credible instruments, identification of the elasticities of maternal employment to child care costs is challenging (Browning, 1992).

Our regression-discontinuity estimates suggest that, on average, 13 mothers start to work for every 100 youngest children in the household that start preschool (though, in our preferred specification, this estimate is not statistically significant at conventional levels). Furthermore, mothers are 19.1 percentage points more likely to work for more than 20 hours a week (i.e., more time than their children spend in school) and to work, on

[2] For the mothers that would work fewer hours than the school day, public schools provide a 100 percent marginal price subsidy for childcare of fixed quality while for the mothers that would otherwise work more hours the price subsidy is inframarginal (Gelbach, 2002).

average, 7.8 more hours per week as consequence of their youngest offspring attending preschool. We find no effect on maternal labor outcomes when a child that is not the youngest in the household attends preschool. Finally, we find that at the point of transition from kindergarten to primary school there are also some employment effects, even though school attendance is nearly universal. This can either be explained by the fact that finding jobs takes time or by a mother's decision to work once the youngest child transitions to primary school.

Our preferred estimates condition on mother's schooling and other exogenous covariates, given evidence that mothers' schooling is unbalanced in the vicinity of the July 1 cutoff in the sample of four year-olds. Using a large set of natality records, we found no evidence that this is due to precise birth date manipulation by parents. Other explanations, like sample selection, are also not fully consistent with the data, and we remain agnostic on this point.

Gelbach (2002) is the closest to the exercise we pursue in this paper. He uses U.S. census data to estimate the effect of public school enrollment for a woman's five-year-old on measures of labor supply, instrumenting enrollment with quarter of birth dummy variables. The idea is that the estimated parameters circumvent the problems of endogenity mentioned above while being informative about whether large subsidies in the form of limited, directly provided pre-primary education influence maternal labor supply. A related literature from several countries reports differences-in-differences estimates of preschool effects on maternal labor supply, relying on geographic and temporal variation in policies that affect preschool attendance.[3]

[3] Cascio (2008), exploits variation across US states in the funding of kindergarten initiatives in the late 60s and early 70s. She finds positive effects of kindergarten enrollment on maternal labor supply for single

Of particular relevance to this paper, Berlinski and Galiani (2007) examine an Argentine infrastructure program, begun in 1993, that built pre-primary classrooms for children aged three to five. Using the fact that the construction exhibited variation in its intensity across provinces, difference-in-differences estimates suggest that the take-up of new preschool vacancies is perfect. The estimates further suggest that when a child is exogenously induced to attend preschool by the supply expansion, the likelihood of maternal employment increases between 7 and 14 percentage points, similar to this paper's point estimates. However, Berlinski and Galiani only find a small, imprecisely estimated effect on hours worked.

The rest of the paper is organized as follows. Section 2 provides background information on the education system in Argentina and describes the datasets used in this paper. Section 3 describes our identification strategy. Section 4 provides the empirical results, and section 5 concludes.

2. Background and Data

A. Background Information

Argentina is a middle-income developing country with a long tradition of free public schooling. The school system is divided into pre-primary, primary and secondary education. Primary school attendance between 6 and 12 years old is nearly 100%. However, pre-primary school attendance among children aged 3 to 5 years old is well

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mothers of five years-old with no younger children. Baker, Gruber and Milligan (2005) study the expansion of subsidized provision of childcare for children zero to four in the Canadian province of Quebec. They also find that childcare use has a positive effect on maternal labor supply for married mothers. Finally, Schlosser (2005) studies the impact on labor supply of the gradual implementation of compulsory preschool laws for children aged three to four in Israel. She also finds that the provision of preschool education in Arab towns increases enrollment and maternal labor supply.

below universal coverage, only 64% in 2001 (Berlinski et al., 2006). Pre-primary education is divided in to three levels: level 1 (age 3), level 2 (age 4), and level 3 (age 5). In general, pre-primary classes occur within existing primary schools. Like primary schools, they typically operate in two shifts (morning and afternoon) with children attending only one of these shifts for three and a half hours a day, five days a week, during a nine-month school year.

Primary school starts at age 6 and has been compulsory since 1885. The Federal Education Law of 1993 further mandated attendance between level 3 of pre-primary education and the second year of secondary school. Its implementation was to have occurred gradually between 1995 and 1999, but it was not rigidly enforced. First, there is no penalty in place for non-compliers. Second, primary school enrollment is not impeded by lack of pre-primary schooling. Finally, there are still large dropout rates at ages 13 and older.

In Argentina, the academic school year starts early in March and finishes in December. Like many countries, there is a cutoff date that defines who can enroll in a given academic year. School age is defined by the age attained on June 30 of the current academic year (see, for example, Art. 39, Resolución CABA: Nº 626/1980). Children can enroll in level 3 of preschool if they turn five years old before July 1 of the current school year.

Until 1994 Argentina was a relatively low unemployment country with unemployment rates never exceeding 10 percent. However, unemployment increased substantially after a macroeconomic shock in 1995 with an average rate of 14.5 for the rest of the nineties. Annual hours worked are high and female participation is at Southern

European level. In 1998, the female employment rate for the group aged 18 to 49 was 48 percent.

The Argentine labor market is not very rigid. Tax rates in Argentina are comparable to those in a typical non-European OECD country. Unions are an important feature of economic life with around half the workers having their wages bargained collectively and 45 percent of employees being union members. However, National minimum wages are set at a relatively low level and probably do not have much impact on employment. Finally, employment protection is at about the average OECD level (Galiani and Nickell, 1999).

B. Data

We use data from the Argentine household survey, the *Encuesta Permanente de Hogares* (EPH), a biannual survey of about 100,000 households managed by Argentina's National Institute of Statistics and Censuses (INDEC). The survey is representative of the urban population of Argentina. It is conducted since 1974 in the main urban clusters, or "agglomerates," of each province of the country (excepting Rio Negro)[4] and the Autonomous City of Buenos Aires. A unique feature of this household survey is that from May 1995 to May 2003 it records the exact date of birth for each individual in the sample. We pool repeated cross-sections of individual-level data from both waves of the survey covering the 1995 to 2001 period. We do not use the information for 2002

[4] Urban Rio Negro was only incorporated to the survey in 2001. See, www.indec.gov.ar for detailed information on the Argentine Household Survey (EPH).

onwards because of the macroeconomic collapse of 2002[5] and its economics and distributional consequences (Galiani et al., 2003; Mussa, 2002).[6]

For our main results, we use a sample of households with mothers aged 18-49 and at least one child age 4 on January 1 of the survey year. The survey collects information on the family relationship between household members and the head of household. Our analysis focuses on children of either male or female household heads, because only in such households the mother of a child can be identified in the EPH. We further restrict the sample to individuals with full information on date of birth, school attendance[7], mother's age and education, and siblings' age. When children of the same household are born on the same day we only include one of the children in the household.

In the first panel of Table 1 we summarize the information contained in the EPH sample. On average, 57 percent of children aged 4 on January 1 of survey year attend school. Thirty-seven percent of the mothers worked the previous week, with 30 percent working 20 or more hours per week (i.e., more time than their children spend in school). The average number of hours worked last week is 12.17. The employment rate for the mothers of children that attend preschool is 38 percent, versus 35 percent for those that do not.

We also use natality data, drawn from birth certificates, compiled by Argentina's Ministry of Health.[8] It is not compulsory for provinces to report exact date of birth, but—with the exception of the Province of Buenos Aires—all other provinces provide this

[5] GDP declined 20% and unemployment peaked at 24%.
[6] The results using the 1995 to 2003 sample are similar to (though not surprisingly less precise than) those reported here and are available from the authors upon request.
[7] EPH does not provide information on whether children are attending a public or private institution.
[8] Further information can be found at the *Dirección de Estadísticas e Información de Salud* website: http://www.deis.gov.ar/.

information for the period 2002-2005.[9] From this data we extracted all births to mothers aged 14-45 (i.e., those who will be 18-49 when their children turns 4). In the second panel of Table 1 we summarize the information contained in the natality records.

3. Empirical Strategy

We seek to identify the impact of preschool attendance on maternal employment. This parameter sheds light on the potential effect of expanding preschool education on maternal employment under the assumption that the take-up rate of new preschool vacancies is perfect (for supporting evidence, see Berlinski and Galiani, 2007). This is an important question, particularly in developing countries where enrollment rates in preschool education are far from universal. An alternative interpretation for the parameter is that of an equivalent cash childcare subsidy. We find this interpretation less straightforward as preschool education as a means of childcare imposes fixed costs on parents (e.g., children have to be taken and collected from school at certain times) that a childcare cash subsidy may not. Parents likely also value the learning of children during pre-primary education. Finally, the population that is affected by these two thought experiments is likely to be different.

Consider the following linear model for a child age 4 on January 1 of year *t* (i.e., a child who is going to turn 5 in the current survey year):

$$(1) \qquad Y_{ijs} = \alpha S_{ijs} + \beta' X_{ijs} + \lambda_j + \mu_s + \varepsilon_{ijs}$$

where Y_{ijs} is a measure of labor supply for the mother of child *i*, residing in region *j*, and observed in survey round *s*.[10] S_{ijs} is a dummy variable indicating school attendance, X_{ijs}

[9] Natality data before 2002 do not contain exact date of birth.

is a vector of exogenous covariates that affect maternal labor supply, and ε_{ijs} is an error term assumed to be independent and identically distributed. The model further includes fixed effects for agglomerates (λ_j) and survey rounds (μ_s). The parameter of interest, α, represents the mean effect of a child's preschool attendance on the maternal labor supply outcome.[11] Because the effects of preschool attendance on maternal labor outcomes could differ between households whose youngest child enters kindergarten with respect to those who have younger children we estimate separate models and parameters for these groups.

If we estimate the model in equation (1) by Ordinary Least Squares (OLS), the estimate $\hat{\alpha}$ is likely to be inconsistent. First, maternal labor supply and child's school attendance are jointly determined, introducing simultaneity bias. Second, omitted variables such as a mother's cognitive ability are plausibly correlated with both labor supply and children's kindergarten attendance.

To disentangle the causal effect of preschool attendance, we use an instrument that induces plausibly exogenous variation in S_{ijs}, but has no direct effect on Y_{ijs}. We use the fact that in Argentina, children must turn 5 years old on or before June 30 of the school year in which they enroll in (compulsory) kindergarten. Those born one day later must wait a full year to enroll. Define a variable B_{ijs} that indicates a child's day of birth during the calendar year. It equals -182 on January 1, 0 on July 1, and 183 on December 31. Further define $Z_{ijs} = 1\{B_{ijs} \geq 0\}$, an indicator function which equals to one for children born on or after July 1, and zero otherwise.

[10] A survey round is the interaction of year and survey wave.

[11] In the case of dichotomous measures of labor supply, we also apply OLS and interpret coefficient estimates as marginal probabilities.

We can model school attendance as

(2) $$S_{ijs} = \delta_0 Z_{ijs} + \delta_1 B_{ijs} + \delta_2 B_{ijs}^2 + \delta_3 Z_{ijs} \times B_{ijs} + \delta_4 Z_{ijs} \times B_{ijs}^2 + \lambda_j + \mu_s + \upsilon_{ijs} .$$

The parameter δ_0 measures the discontinuity in preschool attendance on July 1. In our sample of Argentine 4 year-olds, we anticipate that $\delta_0 < 0$, since children born on or after July 1 do not fulfill the minimum age requirement for enrollment in kindergarten. Still, the estimate is probably greater than -1 (a so-called “fuzzy” discontinuity), since younger children may already be enrolled in the previous, non-compulsory level of preschool, and some older children may ignore compulsory attendance rules.

Similarly, we can model maternal labor supply as

(3) $$Y_{ijs} = \alpha_0 Z_{ijs} + \alpha_1 B_{ijs} + \alpha_2 B_{ijs}^2 + \alpha_3 Z_{ijs} \times B_{ijs} + \alpha_4 Z_{ijs} \times B_{ijs}^2 + \lambda_j + \mu_s + \eta_{ijs} ,$$

where the estimate of α_0 captures the reduced-form effect of July 1 birthdays on labor outcomes. Since we anticipate that $\delta_0 < -1$, the estimate of α_0 must be rescaled by the estimate of δ_0 to recover the effect of school attendance on labor outcomes. In practical terms, this parameter is computed by estimating equation (1) via two-stage least squares (TSLS), conditioning on the interacted polynomials of B_{ijs} and instrumenting S_{ijs} with Z_{ijs} (see, e.g., van der Klaauw, 2002; Imbens and Lemiuex, 2008).

For this to identify the parameter of interest, Z_{ijs} must be correlated with S_{ijs}, but with no direct effects on the outcomes of interest, i.e. $\text{cov}(Z_{ijs}, \varepsilon_{ijs}) = 0$. Since parents can choose the time conception, a child’s season of birth is plausibly correlated with unobserved variables like child health and family income, any of which could directly influence maternal labor supply (Bound, Jaeger, and Baker 1995; Bound and Jaeger 2000). To address this, the TSLS specifications control for smooth functions of B_{ijs}, estimated separately on either side of the cutoff date. The specification above assumes a piecewise quadratic polynomial, but we verify it through visual inspection of means taken within day-of-birth cells, in addition to obtaining estimates with higher-order polynomials.

Although the polynomials capture smooth, seasonal differences in birth date manipulation, they cannot capture precise manipulation, near the cutoff date, via cesarean sections or induced births. In the next section, we use natality data from birth certificates to show that this is an unlikely source of bias. Another possible threat to the internal validity of our estimates could come from child subsidies varying with school age. In this scenario, we could confound the effect of preschool attendance with the effect of these subsidies. To our knowledge, in Argentina, no such discontinuities exist in the design of the welfare system. Finally, parents may lie to educational authorities on their children date of birth in order to enroll them in preschool earlier. This practice is difficult to implement in Argentina as enrollment in school requires a national identification card which includes the date of birth recorded on the birth certificate. Moreover, this will only constitute a threat to identification if parents lie to the household survey on their children date of birth as well which they have no incentive to do.

The estimated regression functions do not fully saturate the model. Lee and Card (2008) show that one can interpret the deviation between the true conditional expectation function and the estimated regression function as random specification error that introduces a group structure into the standard errors for the estimated treatment effect. Thus, we report standard errors clustered by day of birth.

4. Main Results

A. Evidence on Birth Timing

We first investigate the validity of our identifying assumption by examining whether there is manipulation of the running variable, perhaps via cesarean section or induced labor.[12] It is certainly plausible in Argentina, where one-quarter of births are via cesarean section, with rates between 36 and 45 percent in private hospitals (Belizán et al. 1999). Presuming that timed births do not occur from a random draw of the population, it is plausible that the clustering of such parents just to the left (or right) of the July 1 cutoff could introduce a correlation between Z_{ijs} and ε_{ijs} in equation (1).

We use two strategies to diagnose systematic manipulation of the running variable (Imbens and Lemieux, 2008; Lee, 2008). First, we examine the density of birthdates around the July 1 cutoff for suspicious clustering near July 1. Second, we examine the distribution of observed socioeconomic and birth characteristics around the enrollment cutoff, interpreting sharp changes in these variables as suggestive of nonrandom birth date manipulation.

Figure 1 reports a histogram of births using our sample from the natality files. Black bars indicate the July 1 cutoff, as well non-floating holidays in Argentina, given evidence that birth frequencies are lower on such days. There is a strong case that families are *able* to time births (McEwan and Shapiro, 2008). The upper-left panel shows proportionally fewer births on weekends, and that mothers of such births have less schooling. This pattern, common across many countries, has been shown to be correlated with the use of

[12] In Chile, with a similarly high rate of cesarean sections (Belizán et al. 1999), McEwan and Shapiro (2008) found no evidence of birth timing around a July 1 enrollment cutoff. In the U.S., McCrary and Royer (2006) find no evidence of sorting around birthdate cutoffs in Texas and California. In other countries, evidence suggests that parents have manipulated birth dates in order to avoid taxes (Dickert-Conlin and Chandra, 1999) and obtain monetary bonuses (Gans and Leigh, 2007).

cesarean sections and induced labor (Dickert-Conlin and Chandra, 1999). To determine whether such birth timing might occur around July 1, the upper-right panel reports a histogram of all births. Because of the large sample, we restrict it to a 6-month window around July 1, but the results are robust to a larger window. While there are visible dips in births on three national holidays, consistent with the ability to time births, there is no evidence of clustering of births on either side of July 1.

In the bottom panels of Figure 1, we summarize the relationship between children's birth date and two variables: weeks of gestation and mother's schooling. The circles represent the unadjusted means of these variables within daily cells. The superimposed lines are fitted values from a piecewise quadratic specification on date of birth. There is, interestingly, evidence that declines in birth frequencies on holidays are associated with lower values of the covariates (indicated by the solid dots). However, there is no visual evidence of breaks around July 1.

In Table 2, we present the regression analogue of the visual evidence at the bottom of Figure 1. This table confirms the finding for gestation and mother's schooling, as well as for a larger set of covariates that include mother's age, low birth weight, and whether the mother lives with a partner. In sum, the natality data provide no evidence of systematic manipulation of birth dates around the July 1 cutoff.

B. Day of Birth, Preschool Attendance, and Covariate Smoothness

We turn now to the analysis of preschool attendance and covariate smoothness using the household survey (EPH). In Figure 2, we summarize the relationship between children's birth date and preschool attendance. The first panel represents results among 3 and 4 year-olds who are the youngest in the household and the results in the second panel among those that are not the youngest in the household. In this and all subsequent analyses, children's age is calculated on January 1 of the survey year in which they are observed. The circles represent unadjusted means of school attendance within daily cells. The superimposed lines are fitted values from a piecewise quadratic specification.

Figure 2 shows, as expected, that preschool attendance is low among 3 year-olds. There is a small break in attendance, with children born on or just after July 1 slightly less likely to attend than children born just before. A larger break is evident among 4 year-olds, consistent with the fact that kindergarten (i.e., level 3 of preschool) is compulsory. Four year-olds born on July 1 are just below the minimum age requirement for kindergarten and must delay enrollment by one year, while 4 year-olds born on June 30 are eligible to enroll as the youngest kindergarteners.

Table 3 reports the empirical analogue of the visual evidence. Panel A presents the results for the youngest children in the household, and Panel B the results for children who are not the youngest in the household.[13] In column (1), where we only condition on a piecewise quadratic polynomial, we find that among 4 year-olds, the coefficient on birthdays after June 30 is a large, negative, and highly significant estimate of -0.31. It is much smaller (-0.048) among 3 year-olds, albeit significant. In column (2), we add a full set of controls, including dummy variables for each survey round, sample cluster (or agglomerate), day of week of birthday, birthdays on non-floating holidays, linear and quadratic terms of mother's age, a dummy variable indicating female children, and dummies for mother's years of schooling. The results are not statistically different than

[13] A regression of a youngest in the household dummy on a born on/after July 1 dummy and a piecewise quadratic polynomial shows no correlation between being the youngest in the household and the July 1 dummy.

those in Column (1). The results for the sample of children that are not the youngest in the households are similar.

Before turning to study the effect of preschool attendance on various measures of maternal labor supply we scrutinize whether, in the EPH sample, covariates are smooth around the cutoff point. In Table 4, we report successive estimates of an equation like (3) for a dummy variable indicating whether the child is female, mother's age, and mother's years of schooling. Panel A in this table is for the youngest child in the household and Panel B for the sample of children who are not the youngest in the household. The most obvious pattern is that mother's schooling is systematically lower among the youngest children in each household born on or after July 1 (that is, among children less likely to attend school). The basic result is robust to the inclusion of dummy variables for agglomerates, surveys, day-of-week, and holiday births. Interestingly, mother's schooling is balanced around the cutoff for the sample of children that are not the youngest in the household and for those children aged 3. In Figure 3, we present the corresponding visual evidence for mother's schooling.

In Table 5, we present a number of robustness check both for the results on preschool attendance and mother's schooling. For brevity, we focus on the sample of 4 year-olds. We start by looking at what an extent the quadratic specification is driving our results. Column (1) is the benchmark; for preschool attendance we reproduce the estimates of column 2 in Table 3, where we have the full-set of controls, and for mother's schooling those of column (5) in Table 4[14]. In columns (2) and (3), we use cubic and quartic piecewise polynomials respectively. In the remaining columns of Table 5, we use a

[14] The results for the schooling equation are similar if we include other controls and use as benchmark column (6) of Table 4.

piecewise quadratic polynomial but within different samples. In column (4), we focus on the sample of children born between April/September. In column (5), we drop a one week window at both sides of the cutoff. In column (6), we use survey weights. Neither changing the polynomials or the samples affects the results we have presented so far.

In the final two columns of Table (5), we present the results of a placebo experiment. In column (7), we take the sample of children born between January 1 and June 30. We center the date of birth variable on April 1 and we create a dummy for being born on or after April 1 that we interact with a quadratic polynomial. We report the coefficient of this dummy variable. In column (8), we run a similar experiment for those children born between July 1 and December 31. The dummy variable is now being born on or after October 1. We find no statistically significant correlations between the outcomes and these dummy variables. However, it is worth noting that the coefficient for mother's years of schooling can be large (the p-value is 0.115) as the result in column (7) shows.

The fact that covariates are smooth around July 1 in the natality data reduces the plausibility of systematic manipulation of birth dates as an explanation for the robust correlations just observed. One alternative explanation is sample selection. This could happen, for example, if relatively less-educated mothers of children born before July 1 are induced to work more than the mothers of children born on or after July 1, and also are less likely to be interviewed by the household survey as a result. Though a potentially compelling explanation, the point estimates are large enough to render it less plausible. On average, the mothers' of children age 4 completed 9.37 years of education with 64.4% of these mothers completing 9 or less years of education[15]. Suppose that the mothers with

[15] The distribution of years of education for the mothers of children age 4 is: 0 (0.85%), 3 (11.16%), 7 (30.00%), 9 (22.53%), 12 (16.74%), 14 (2.29%), 15 (11.46%) and 17 (5.08%).

less than the average level of education are selecting out of the sample at the same rate over the whole education distribution. In this case, we need approximately 38% of these mothers to disappear from the sample to generate a difference of 0.8 years of education[16].

Furthermore, we also find large differences in the mothers' schooling among the mothers of the non-youngest children aged 1 and 2 years old (the results are not reported in the tables). Almost none of these children attend school, so it is unlikely to result from labor-supply induced sample selection. As it stands, the most likely explanation is noise, though we cannot rule out the presence of sample selection. As a result, our preferred estimates below control for the education of the mother. Unconditional estimates will likely be biased upwards because of the positive correlation between education and labor market outcomes.

C. School Attendance and Maternal Labor Outcomes

In Table 6, we report estimates from reduced-form regressions of mother's labor supply on a dummy variable indicating births on or after July 1. The measures of labor supply include whether mothers were employed last week, whether mothers worked for at least 20 hours last week ("full-time"), and the number of hours worked last week. All regressions control for a piecewise quadratic of birth date, while regressions in even columns control for regional dummies, survey round dummies, holiday dummies, day of week of birth dummies, a quadratic on mother's age, and dummies for child gender and mother's schooling. Because children born on or after July 1 are less likely to attend

[16] The distribution of years of education if 38 percent of the mothers with less than 9.317 years of education drop out from the sample would be: 0 (0.70%), 3 (9.08%), 7 (24.63%), 9 (18.50%), 12 (22.17%), 14 (3.03%), 15 (15.18%) and 17 (6.73%). Therefore, average years of education is 10.166.

school, we expect a negative effect for maternal labor outcomes of being born on or after July 1.

In the samples of 3 year-olds, none of the coefficients are statistically distinguishable from zero. This is perhaps not surprising given the relatively small difference in school attendance around the July 1 cutoff for such children. For children aged 4 that are the youngest in the household, the coefficients in Table 6 range between -0.066 and -0.038 when a dichotomous indicator of mother's labor supply is the dependent variable. Coefficients range between -0.085 and -0.058 when the dependent variable is work for more than 20 hours a week. In columns (5) and (6), mothers of children born in the second semester of the year work between 3.4 and 2.4 less hours in the previous week. Not surprisingly, given the evidence from the previous section, the lower range of the coefficients corresponds to the fully-specified models. The pattern of the reduced-form results just described is corroborated in Figure 4, which presents unsmoothed means and fitted values for children aged 4, for the youngest children (upper panels) and not youngest (lower panels).

Despite the fact that there is a significant increase in preschool attendance among children aged 4 that are not the youngest in the household, there is no evidence of labor supply effects for their mothers. In theory, it is plausible that there could be a labor supply effect for the mothers of these children as the childcare provided by preschool attendance of at least one of their children contributes towards reducing the total cost of childcare. However, the result implies that this contribution is small relative to the cost of childcare for the other children and does not affect the labor supply decision of the mother.

Table 7 reports two-stages least squares estimates, which are simply the reduced-form estimates from Table 6 divided by the changes in the probability of attendance estimated among 4 year-olds in Table 3. Among the sub-sample of youngest children (panel A), the model with the full set of controls suggests that mothers with children attending kindergarten are 12.7 percentage points more likely to work, though the estimate is not precise. This means that 13 mothers start working for every 100 youngest children in the household that start preschool. Furthermore, mothers are 19.1 percentage points more likely to work more than 20 hours of week and to work, on average, 7.8 more hours per week as consequence of their youngest offspring attending preschool. Both estimates are statistically significant at the 10 percent level. The point estimates of the binary employment measures are consistent with the upper end of estimates from Berlinski and Galiani (2007), who used a different sample from EPH, in concert with temporal and regional variation in preschool construction, to identify labor supply effects.

The fact that the estimates for being employed more than 20 hours a week are bigger than those for participation (although the difference is not statistically significant) implies that for the compliers in this experiment preschool attendance affects female labor supply on two margins. Some mothers that were not employed before their youngest child entered to preschool find employment and some other mothers that were working for less than 20 hours a week start working for longer hours.

In Table 8, we report a set of robustness checks for the two-stages least squares estimates. Column (1) is the benchmark, reproducing estimates from columns (2), (4) and (6) in Table 7. In columns (2) and (3), we use cubic and quartic polynomials respectively. In the remaining columns of Table 5, we use a piecewise quadratic polynomial but within different samples. In column (4), we focus on the sample of children born between April/September. In column (5), we drop a one week window at both sides of the cutoff. In column (6), we use survey weights.[17] The results in Panel A are similarly-signed, but less precise than in the benchmark specification. The most noticeable difference is that with survey weights the results for the non-youngest children in the household (panel B) tend to be similar to the results for the youngest children.

D. Day of Birth and Maternal Outcomes for Primary School Children

The previous estimates reflect the effect of the transition of a child from no schooling to preschool attendance on the current employment decision of the mother. However, some of these effects may persist in the medium run if it takes time to either find employment or make the necessary arrangements to start working. Also, unlike a direct cash subsidy for childcare, the transition from preschool to primary school of the youngest child in the household may be interpreted by mothers as a signal that it is time to begin working. Therefore, we may be able to find labor market effects at ages 5 and 6.

In Table 9, we reproduce estimates for school attendance, mother's schooling, mother's employment, employment for more than 20 hours last week, and hours worked last week for children aged 5 and 6 on July 1 of the survey year. School enrollment is very high at these ages and there is, therefore, no enrollment effect from being born on or after July 1. The correlation between maternal schooling and being born in the second semester still persists. We find that for children aged 5 that are the youngest in the household the maternal labor outcomes coefficients in Table 9 tend to be of similar sign

[17] Because in the placebo experiments from Table 5 the effect on attendance is close to zero the corresponding two-stage least squares estimates are close to zero as well.

and magnitude than those of the youngest children aged 4 we reported in Table 6. We find no systematic effects for children aged 6 or for those children that are not the youngest in the household.

Why do we observe employment effects for children aged 5 when there is no school attendance discontinuity? The first possible explanation is that the result is only due to a spurious correlation induced by the lack of balancing in the observables. Although this is certainly plausible it does not explain why such a correlation does not exist at age 6. An alternative explanation is that although some mothers find employment concurrently with their children starting kindergarten for others it takes time to find suitable employment so that some labor supply effects appear at age 5. Given the similarity in the magnitude of the coefficients between ages 4 and 5 this cannot be the whole story.

An alternative explanation is that, at age 5, the dummy for being born on or after July 1 picks up the difference between being enrolled in kindergarten and primary school. If the maturity of a child plays a role on the decision of a mother to go work, the transition from kindergarten to primary school may be interpreted by families as a clear signal that in-home care is no longer necessary.[18] This is consistent with the fact that no such effect appears at age 6.

5. Conclusion

Expanding preschool education has the dual goals of improving child outcomes and work incentives for mothers. This paper provides evidence on the second, identifying the impact of preschool attendance on maternal employment in Argentina. A major challenge

[18] A more practical explanation is that primary schools may have longer school days and hence provide a larger implicit subsidy. Nevertheless, primary schools, like preschools, also operate mainly on a two-shift schedule.

in identifying the causal effect of preschool attendance on parental outcomes is non-random selection into early education. We address this by relying on plausibly exogenous variation in preschool attendance that is induced by children born on either side of Argentina's enrollment cutoff date of July 1. Children born just before July 1 are 0.3 more likely to attend school. Our regression-discontinuity estimates compare maternal employment outcomes of children on either side of this cutoff.

Our findings suggest that, on average, 13 mothers start to work for every 100 youngest children in the household that start preschool (though, in our preferred specification, this estimate is not statistically significant at conventional levels). Furthermore, mothers are 19.1 percentage points more likely to work for more than 20 hours a week (i.e., more time than their children spend in school) and to work, on average, 7.8 more hours per week as consequence of their youngest offspring attending preschool. We find no effect on maternal labor outcomes when a child that is not the youngest in the household attends preschool. Finally, we find that at the point of transition from kindergarten to primary school some employment effects also exist. This can either be explained by the fact that finding jobs takes time or by a mother's decision to move into work once the youngest child starts primary school.

Our preferred estimates condition on mother's schooling and other exogenous covariates, given evidence that mothers' schooling is unbalanced in the vicinity of the July 1 cutoff in the sample of 4 year-olds. Using a large set of natality records, we found no evidence that this is due to precise birth date manipulation by parents. Other explanations, like sample selection, are also not fully consistent with the data, and we remain agnostic on this point. Despite this shortcoming, the credibility of the estimates is partly enhanced by the consistency of point estimates with Argentine research using a different EPH sample and sources of variation in preschool attendance (Berlinski and Galiani, 2007).

A growing body of research suggests that pre-primary school can improve educational outcomes for children in the short and long run. This paper provides further evidence that, ceteris paribus, an expansion in preschool education may enhance the employment prospects for mothers of children in preschool age.

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Table 1: Variable Definitions and Descriptives Statistics

	Definition	Mean (standard deviation)	Minimum/ Maximum	N
Panel A: *Encuesta Permanente de Hogares*, May 1995 to October 2001 (4 year-olds)				
Birth day	Child's day of birth in the calendar year, defined to address leap years: 1/1=-182; 7/1=0; 12/31=183.	-0.40 (105.92)	-182/183	22,974
Born on/after cutoff	Child's day of birth is 7/1 or later.	0.50	0/1	22,974
Attends school	Child attended any level of school at time of survey.	0.58	0/1	22,974
Female	Child is female.	0.48	0/1	22,974
Mother's age	Mother's age in decimal years.	32.81 (6.24)	18.01/49.94	22,974
Mother's schooling	Mother's years of formal schooling.	9.37 (3.90)	0/17	22,974
Mother works	Mother worked last week.	0.37	0/1	22,974
Mother works full-time	Mother worked 20 or more hours last week.	0.30	0/1	22,841
Hours worked	Mother's hours worked in all jobs last week.	12.17 (19.05)	0/84	22,841
Panel B: Natality files, 2002-2005				
Birth day	Same as above.	-0.14 (53.18)	-91/91	888,033
Born on/after cutoff	Same as above.	0.50	0/1	888,033
Saturday	Born on Saturday.	0.12	0/1	888,033
Sunday	Born on Sunday.	0.10	0/1	888,033
Holiday	Born on a non-floating holiday: 1/1, 1/5, 25/5, 9/7, 8/12, 24/12, 25/12, 31/12	0.01	0/1	888,033
Mother's age	Mother's age in integer years.	26.37 (6.50)	14/45	888,033
Mother's schooling	Mother's years of formal schooling.	9.36 (3.84)	0/16	874,904
Low birthweight	Birthweight is less than 2500 grams.	0.07	0/1	877,551
Gestation	Weeks of gestation.	38.78 (1.89)	14/45	857,330
Public facility	Child born in a public health facility.	0.58	0/1	886,909
Lives with partner	Mother lives with a partner.	0.84	0/1	864,879

Notes: The Encuesta Permanente de Hogares sample (Panel A) includes children from surveys between May 1995 and October 2001 who were 4 years-old on January 1 of survey year and had a mother present aged 18-49. The natality files (Panel B) include births from the 2002-2005 files, of mothers aged 14-45 that occurred between April 1 and September 30.

Table 2: Day of Birth and Baseline Variables, Natality Data

	Mother's schooling	*Mother's age*	*Low Birth weigth*	*Gestation*	*Public facility*	*Lives with partner*
	(1)	(2)	(3)	(4)	(5)	(6)
Born on/after cutoff	0.022	0.087*	-0.001	0.012	-0.003	0.001
	[0.025]	[0.051]	[0.002]	[0.015]	[0.004]	[0.002]
Sunday	-0.517***	-0.944***	0.006***	0.011	0.105***	-0.025***
	[0.019]	[0.036]	[0.001]	[0.010]	[0.003]	[0.001]
Saturday	-0.295***	-0.633***	0.007***	-0.008	0.060***	-0.018***
	[0.019]	[0.034]	[0.001]	[0.008]	[0.003]	[0.002]
Holiday	-0.322***	-0.377***	0.001	0.026**	0.053***	-0.010***
	[0.044]	[0.075]	[0.001]	[0.011]	[0.006]	[0.003]
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	874,904	888,033	877,551	857,330	886,909	864,879

Source: Natality records, 2002-2005.
Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. Additional controls include dummies for birth year, province of birth, and day-of-week of birthday (excluding Monday). All regressions include a piecewise quadratic polynomial of birth date. Sample includes all observations for mothers 14-45 with non-missing values of dependent variable and exact birth date, between April 1 and September 30.

Table 3: Day of Birth and School Attendance by Age Group

	Dependent Variable: *School Attendance* (1)	(2)
Panel A: Youngest in Household		
3 year-olds	-0.048**	-0.057***
	[0.021]	[0.019]
	12,299	12,299
4 year-olds	-0.310***	-0.299***
	[0.029]	[0.029]
	10,990	10,990
Panel B: Not Youngest in Household		
3 year-olds	-0.052**	-0.071***
	[0.021]	[0.020]
	9,890	9,890
4 year-olds	-0.321***	-0.331***
	[0.027]	[0.026]
	11,984	11,984
Controls?	No	Yes

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: Cells report the coefficient estimate of a dummy variable indicating birthdays on or after July 1, based on equation (2). *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. Controls include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays (see text), in addition to linear and quadratic terms of mother's age, a dummy variable indicating female children and dummies for mother's years of schooling.

Table 4: Day of Birth and Baseline Variables by Age Group

	Dependent Variable:					
	Female		*Mother's age*		*Mother's schooling*	
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Panel A: Youngest in Household</u>						
3 year-olds	-0.006	0.001	-0.017	0.013	-0.243	-0.169
	[0.036]	[0.036]	[0.479]	[0.483]	[0.266]	[0.260]
	12,299	12,299	12,299	12,299	12,299	12,299
4 year-olds	0.044	0.050	-0.105	-0.052	-0.810***	-0.760***
	[0.034]	[0.033]	[0.466]	[0.464]	[0.232]	[0.222]
	10,990	10,990	10,990	10,990	10,990	10,990
<u>Panel B: Not Youngest in Household</u>						
3 year-olds	-0.039	-0.039	0.206	0.175	-0.065	-0.086
	[0.038]	[0.038]	[0.397]	[0.400]	[0.258]	[0.271]
	9,890	9,890	9,890	9,890	9,890	9,890
4 year-olds	-0.059*	-0.060*	-0.385	-0.442	0.004	-0.096
	[0.032]	[0.032]	[0.446]	[0.444]	[0.219]	[0.216]
	11,984	11,984	11,984	11,984	11,984	11,984
Controls?	No	Yes	No	Yes	No	Yes

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: Cells report the coefficient estimate of a dummy variable indicating birthdays on or after July 1, based on equation 2. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. Controls include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays (see text).

Table 5: Day of Birth, School Attendance and Mother's Schooling by Age Group. Alternative Specifications and Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 4 years-old, Youngest in household								
Attend school	-0.299***	-0.297***	-0.252***	-0.274***	-0.291***	-0.288***	0.014	-0.014
	[0.029]	[0.038]	[0.049]	[0.039]	[0.035]	[0.048]	[0.028]	[0.035]
	10,990	10,990	10,990	5,454	10,587	10,990	5,286	5,704
Mother's schooling	-0.810***	-0.853***	-0.921**	-0.911***	-0.692**	-0.836*	0.503	-0.047
	[0.232]	[0.313]	[0.384]	[0.330]	[0.275]	[0.476]	[0.317]	[0.400]
	10,990	10,990	10,990	5,454	10,587	10,990	5,286	5,699
Panel B: 4 years-old, Not youngest in household								
Attend school	-0.331***	-0.327***	-0.307***	-0.302***	-0.338***	-0.232***	-0.005	0.027
	[0.026]	[0.033]	[0.042]	[0.035]	[0.032]	[0.051]	[0.033]	[0.036]
	11,984	11,984	11,984	5,906	11,515	11,984	6,183	5,801
Mother's schooling	0.004	-0.164	-0.433	-0.224	0.233	0.384	0.161	-0.208
	[0.219]	[0.259]	[0.296]	[0.264]	[0.285]	[0.332]	[0.331]	[0.297]
	11,984	11,984	11,984	5,906	11,515	11,984	6,183	5,801
Picewise polynomial	Quadratic	Cubic	Quartic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic
Sample	Full	Full	Full	Born April - September	Born 7/1-7 & 6/24-30 excluded	Full with survey weights	Born 12/1 - 6/30	Born 7/1 - 12/31

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.

Notes: The “Full” sample and estimates from (1) are based on Tables 3 and 4, columns (2) and (5), respectively. In column (7), the date of birth variable is centered on April 1 and we report coefficient of a dummy equal to 1 if born on April 1 or after and zero otherwise. In column (8), the date of birth variable is centered on October 1 and we report the coefficient of a dummy equal to 1 if born on October 1 or after and zero otherwise. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. All regressions for school attendance include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays (see text), as well as controls for linear and quadratic terms of mother’s age, a dummy variables indicating female children and dummies for mother’s years of schooling.

Table 6: Day of Birth and Maternal Labor Outcomes by Age Group

	Dependent Variable:					
	Mother works		*Mother works full-time*		*Hours worked*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Youngest in Household						
3 year-olds	0.005	0.009	-0.007	-0.005	-0.124	-0.082
	[0.032]	[0.029]	[0.029]	[0.026]	[1.242]	[1.174]
	12,299	12,299	12,221	12,221	12,221	12,221
4 year-olds	-0.066**	-0.038	-0.085**	-0.058*	-3.339**	-2.358*
	[0.033]	[0.031]	[0.033]	[0.031]	[1.396]	[1.350]
	10,990	10,990	10,911	10,911	10,911	10,911
Panel B: Not Youngest in Household						
3 year-olds	-0.012	-0.011	0.009	0.009	0.468	0.437
	[0.029]	[0.031]	[0.029]	[0.030]	[1.213]	[1.260]
	9,890	9,890	9,843	9,843	9,843	9,843
4 year-olds	-0.002	0.003	-0.012	-0.007	-0.771	-0.553
	[0.027]	[0.026]	[0.025]	[0.023]	[1.059]	[1.026]
	11,984	11,984	11,930	11,930	11,930	11,930
Controls?	No	Yes	No	Yes	No	Yes

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: Cells report the coefficient estimate of a dummy variable indicating birthdays on or after July 1 (based on equation (2)). *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. Controls include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays, in addition to linear and quadratic terms of mother’s age, a dummy variable indicating female children and dummies for mother’s years of schooling.

Table 7: Preschool Attendance and Maternal Labor Outcomes, 4 Year-Olds: Two-Stage Least Squares Estimates

	Dependent Variable:					
	Mother works		*Mother works full-time*		*Hours worked*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Youngest in Household						
4 year-olds	0.213*	0.127	0.270**	0.191*	10.642**	7.779*
	[0.110]	[0.106]	[0.111]	[0.104]	[4.595]	[4.556]
	10,990	10,990	10,911	10,911	10,911	10,911
Panel B: Not Youngest in Household						
4 year-olds	0.006	-0.008	0.037	0.021	2.420	1.670
	[0.084]	[0.078]	[0.079]	[0.070]	[3.347]	[3.114]
	11,984	11,984	11,930	11,930	11,930	11,930
Controls?	No	Yes	No	Yes	No	Yes

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: Cells report the coefficient estimate of a dummy variable indicating school attendance from a TSLS regression, based on equation (1) and including a piecewise quadratic polynomial of birthdate. The excluded instrument is Z. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. The sample size of each regression is reported below coefficients and standard errors. Controls include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays, in addition to linear and quadratic terms of mother's age, a dummy variable indicating female children and dummies for mother's years of

Table 8: Preschool Attendance and Maternal Labor Outcomes, 4 Year-Olds: Two-Stage Least Squares Estimates. Alternative Specifications and Samples

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 4 years-old, Youngest in household						
Mother works	0.127	0.050	0.086	0.019	0.197	0.056
	[0.106]	[0.137]	[0.202]	[0.153]	[0.131]	[0.187]
	10,990	10,990	10,990	5,454	10,587	10,990
Mother works full-time	0.191*	0.076	0.181	0.075	0.245*	0.208
	[0.104]	[0.137]	[0.206]	[0.157]	[0.125]	[0.155]
	10,911	10,911	10,911	5,416	10,511	10,911
Hours worked	7.779*	5.122	8.642	4.190	10.217*	13.741**
	[4.556]	[6.101]	[9.177]	[6.870]	[5.496]	[6.731]
	10,911	10,911	10,911	5,416	10,511	10,911
Panel B: 4 years-old, Not youngest in household						
Mother works	-0.008	0.067	0.142	0.091	-0.030	0.294*
	[0.078]	[0.097]	[0.128]	[0.114]	[0.095]	[0.173]
	11,984	11,984	11,984	5,906	11,515	11,984
Mother works full-time	0.021	0.006	0.105	0.056	0.014	0.110
	[0.070]	[0.085]	[0.111]	[0.096]	[0.087]	[0.143]
	11,930	11,930	11,930	5,879	11,465	11,930
Hours worked	1.670	0.627	5.492	3.148	0.867	11.526
	[3.114]	[3.979]	[5.404]	[4.552]	[3.716]	[7.819]
	11,930	11,930	11,930	5,879	11,465	11,930
Picewise Polynomial	Quadratic	Cubic	Quartic	Quadratic	Quadratic	Quadratic
Sample	Full	Full	Full	Born April - September	Born 7/1-7 & 6/24-30 excluded	Full with survey weights

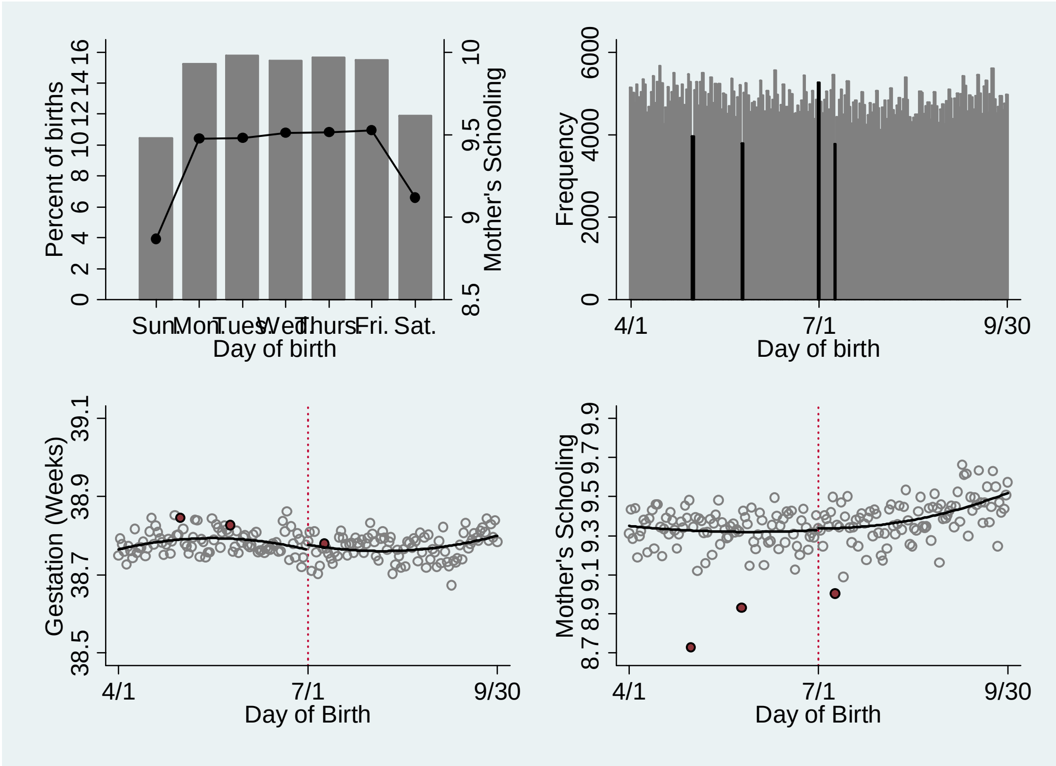
Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: The “Full” sample and estimates from column (1) are based on even columns in Table 7. Cells report the coefficient estimate of a dummy variable indicating school attendance from a TSLS regression, following the specification in Table 7. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. All regressions include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays, in addition to linear and quadratic terms of mother’s age, a dummy variable indicating female children and dummies for mother’s years of schooling.

Table 9: Day of Birth, Attendance, Mother's Schooling and Labor Market Outcomes at Ages 5 and 6

	Dependent Variable:									
	Attend		*Mother's schooling*		*Mother works*		*Mother works full-time*		*Hours worked*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Youngest in Household										
5 year-olds	-0.020	-0.014	-0.887***	-0.828***	-0.056*	-0.025	-0.082**	-0.047	-3.601***	-2.455*
	[0.014]	[0.014]	[0.324]	[0.313]	[0.034]	[0.031]	[0.034]	[0.031]	[1.370]	[1.263]
	10,126	10,126	10,126	10,126	10,126	10,126	10,052	10,052	10,052	10,052
6 year-olds	-0.005	-0.005	-0.502	-0.457	0.025	0.038	0.028	0.044	1.966	2.608*
	[0.004]	[0.004]	[0.318]	[0.322]	[0.033]	[0.031]	[0.037]	[0.035]	[1.426]	[1.347]
	9,235	9,235	9,235	9,235	9,235	9,235	9,164	9,164	9,164	9,164
Panel B: Not Youngest in Household										
5 year-olds	-0.017	-0.018	-0.051	-0.059	-0.005	0.001	-0.017	-0.016	-0.409	-0.284
	[0.015]	[0.013]	[0.240]	[0.232]	[0.030]	[0.026]	[0.027]	[0.024]	[1.095]	[1.017]
	13,415	13,415	13,415	13,415	13,415	13,415	13,336	13,336	13,336	13,336
6 year-olds	-0.001	-0.002	0.235	0.305	0.034	0.031	0.02	0.017	0.706	0.645
	[0.006]	[0.006]	[0.274]	[0.261]	[0.028]	[0.025]	[0.026]	[0.023]	[1.118]	[1.010]
	14,184	14,184	14,184	14,184	14,184	14,184	14,096	14,096	14,096	14,096
Controls?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Source: Encuesta Permanente de Hogares, May 1995 to October 2001.
Notes: Cells report the coefficient estimate of a dummy variable indicating birthdays on or after July 1, based on equation (2). *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, adjusted for clustering in day-of-birth cells, are in parentheses. Controls include dummy variables for each survey round, sample cluster (agglomerate), day of week of birthday, and holiday birthdays, in addition to linear and quadratic terms of for mother’s age; and dummy variables indicating female children and years of mother’s schooling.

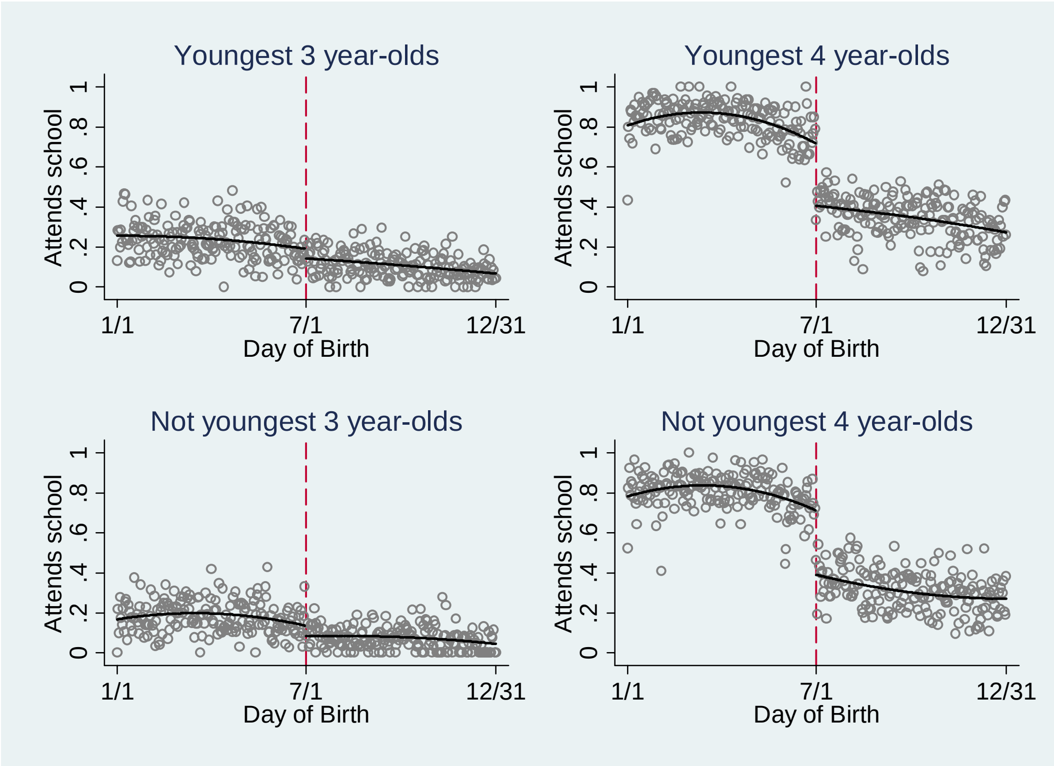
Figure 1: Evidence of Birth Day Sorting on Natality Records



Source: Natality records, 2002-2005.

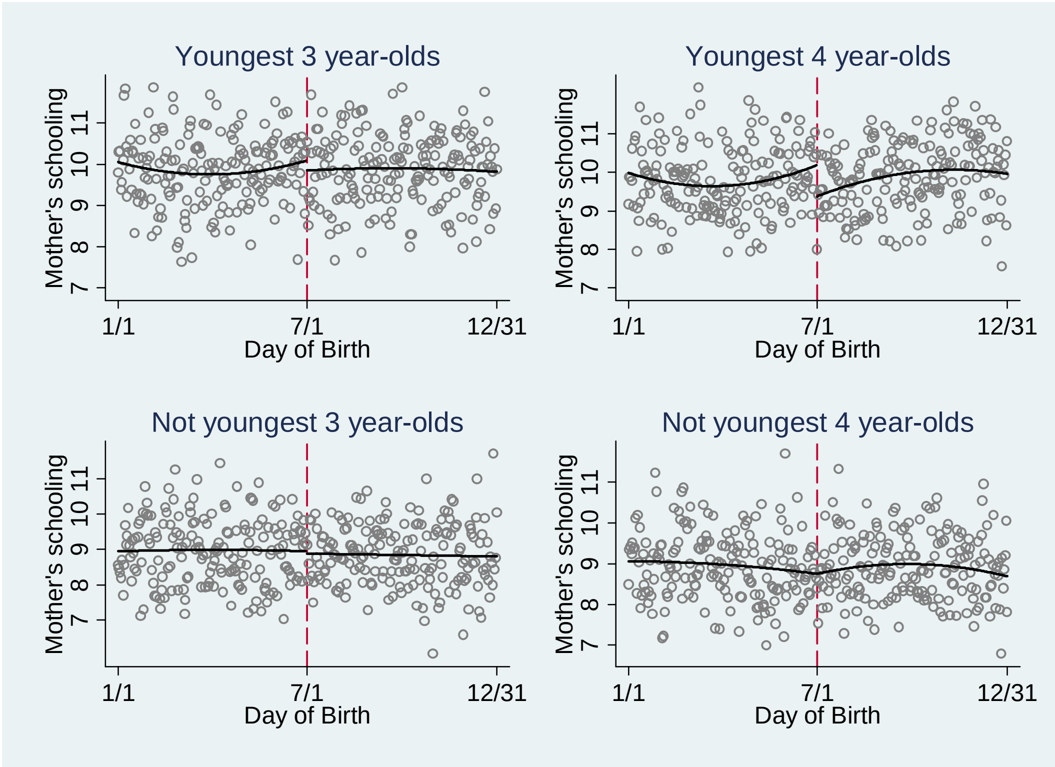
Notes: In upper-left, bars indicate the percent of total births and dots indicate mean mother's schooling. In upper-right, black bars indicate the cutoff (7/1) and three non-floating holidays (5/1, 5/25, and 7/9). In bottom panels, dots indicate means values of mother's schooling within day-of-birth cells, while solid dots indicate means on three non-floating holidays.

Figure 2: School Attendance and Day of Birth, by Age Group



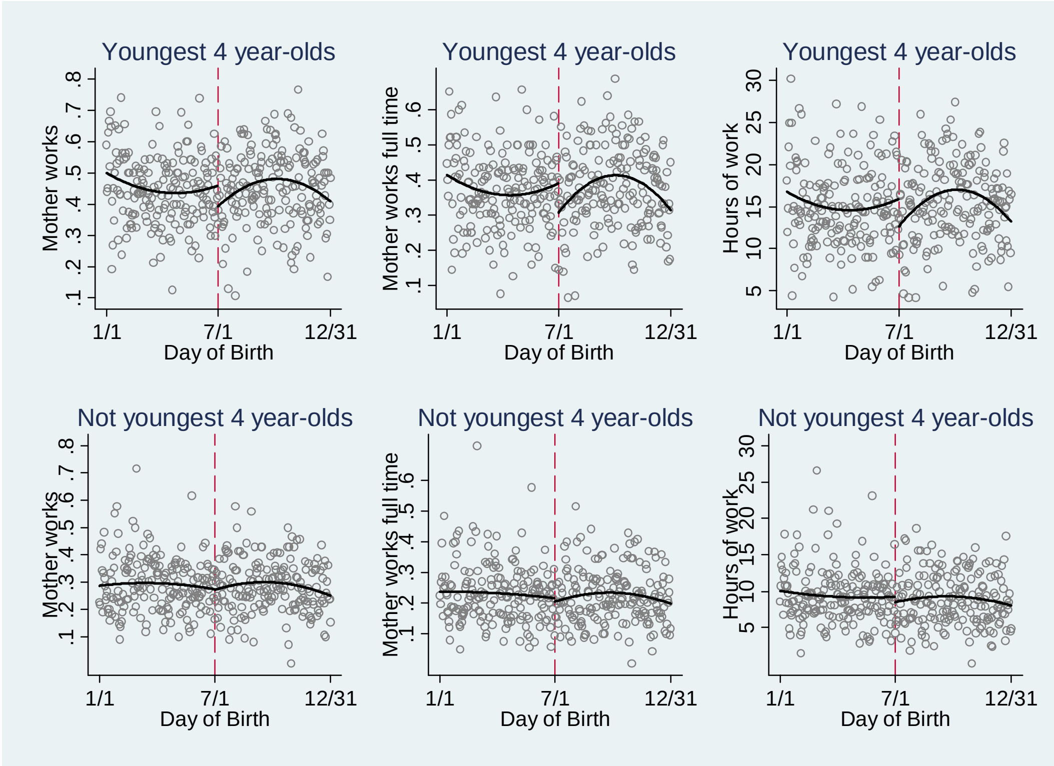
Source: Encuesta Permanente de Hogares, 1995-2001.
Notes: Dots indicate means of a dummy variable indicating school attendance within day-of birth cells. Solid lines show fitted values of piecewise quadratic polynomials

Figure 3: Mother's Schooling and Day of Birth, by Age Group



Source: Encuesta Permanente de Hogares, 1995-2001.
Notes: Dots indicate means of a dummy variable indicating school attendance within day-of-birth cells. Solid lines show fitted values of piecewise quadratic polynomials

Figure 4: Maternal Labor Outcomes and Child's Day of Birth, by Age Group



Source: Encuesta Permanente de Hogares, 1995-2001.
Notes: Dots indicate means of a dummy variable indicating school attendance within day-of-birth cells. Solid lines show fitted values of piecewise quadratic polynomials