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DISCUSSION PAPER SERIES

IZA DP No. 16676

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Cristina Borra
Universidad de Sevilla and IZA

Libertad González
Universitat Pompeu Fabra, Barcelona School of Economics and IZA

David Patiño
Universidad de Sevilla

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ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

School Starting Age and Infant Health

We study the effects of school starting age on siblings' infant health. In Spain, children born in December start school a year earlier than those born the following January, despite being essentially the same age. We follow a regression discontinuity design to compare the health at birth of the children of women born in January versus the previous December, using administrative, population-level data. We find small and insignificant effects on average weight at birth, but, compared to the children of December-born mothers, the children of January-born mothers are more likely to have very low birthweight. We then show that January-born women have the same educational attainment and the same partnership dynamics as December-born women. However, they finish school later and are (several months) older when they have their first child. Our results suggest that maternal age is a plausible mechanism behind our estimated impacts of school starting age on infant health.

JEL Classification: I12, J12, J13

Keywords: school starting age, infant health, maternal age, school cohort

Corresponding author:
Cristina Borra
Facultad de Ciencias Económicas y Empresariales
Universidad de Sevilla
Ramón y Cajal 1
41018 Seville
Spain
E-mail: cborra@us.e

1. Introduction

We use strict school entry cutoff dates together with population data on all births in Spain from 1996 to 2018 to estimate the effect of age at school start on newborn health, with a with a particular focus on clarifying the associated mechanisms.

Most education systems implement a single cutoff date to determine eligibility for compulsory schooling. Students born before the cutoff date enter school on a given year, while students born after the cutoff date must wait until the start of the next school year. These cutoffs effectively cause that children of virtually the same biological age are placed in different school cohorts and start school one year apart.

Should we care at all whether children start school early or late? Clearly, in view of economic theories of child skill formation and empirical studies looking at early childhood interventions such as Head Start, early enrollment facilitates accumulation of skills which may be important for later life outcomes (Almond et al., 2018; Cunha and Heckman, 2007). However, enrolling a child prematurely before he or she is ready for formal education may be detrimental for the child's development (Bedard and Dhuey, 2012; Deming and Dynarski, 2008). Research shows that there are widespread short-term academic performance benefits from late enrolment, that is, from being older in a school cohort (Bedard and Dhuey, 2006; Dhuey et al., 2019; Mühlenweg and Puhani, 2010; Peña, 2017). Evidence is more mixed with respect to the longer term benefits from being older in a cohort, such as educational attainment, earnings, and crime involvement, among others (Black et al., 2011; Dobkin and Ferreira, 2010; Fredriksson and Öckert, 2014; Røed Larsen and Solli, 2017). It is thus surprising that despite this lack of convincing evidence regarding the benefits of holding children back to start school older, redshirting has become widespread in the US and in many European countries such as Germany, Switzerland, and Denmark (Balestra et al., 2020; Dee and Sievertsen, 2018; Deming and Dynarski, 2008; Dhuey, 2022; Görlitz et al., 2022). In this paper we aim to contribute to the debate of whether there are any long-term potential costs or benefits associated to starting school later by looking at its impacts on offspring's health at birth. Learning about the effect of potential mothers' school starting age on infant health is not only interesting in itself, but also because low birth weight and preterm delivery may impact intergenerational outcomes, including adult health and mortality, test scores, educational attainment, employment, and earnings (Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2007; Figlio et al. 2014; Royer 2009 and the references included in Almond, Currie, and Duque, 2018).

Our estimation strategy compares birth outcomes of women of the same biological age, born a few days apart shortly before and after the school cohort cutoff date, which in Spain is January 1st, and placed in different school cohorts. We use population data from Spanish birth registers to, first, set up our estimation strategy, and second, estimate the causal impact of starting school later on children's health at birth. We first use data from Spanish Vital Statistics from 1980 to 1995 to show that women born before and after the school cutoff date are balanced in covariates before entering school. In doing so, we rule out concerns such as those raised by Buckles and Hungerman's (2013) regarding seasonality in family characteristics at birth. Subsequently, we use Vital Statistics from 1996 to 2018 to estimate the causal impact of school starting age on fetal health. We find that January-born mothers are 11% more likely to have an early pre-term birth (before 34 weeks), and 18% more likely to give birth to a very low birthweight (<1,500g.) child.

We then explore potential mechanisms. We consider five likely explanations that may mediate the relationship between school starting age and infant health, although the list may not be exhaustive. First, mothers born after the cutoff may drop out of school before graduating from high school and may have a lower educational attainment at the time of motherhood. Reduced maternal education can negatively impact infant health. (McCrary and Royer, 2011). Conversely, older mothers may have a maturity advantage during school that allows them to accumulate more skills later on, and have healthier babies (Bedard and Dhuey, 2006). Second, mothers born after the cutoff may be more mature and have younger peers that exert less pressure when faced with potentially risky behaviors such as smoking, substance use, or even crime (Johansen, 2021; Landersø et al., 2017). They may also have better noncognitive skills and less mental health problems (Black et al., 2011; Peña, 2020). Reduced maternal risky health and criminal behaviors, along with improved maternal mental health, can lead to better offspring health at birth. Third, assortative mating may entail that older women in a school cohort form unions with older, more educated partners (McCrary and Royer, 2011). Also, women may form partnerships alongside their peers in their school cohort, that is, at a later biological age if they are born after the cutoff and at an earlier biological age if they are born before the cutoff (Skirbekk et al., 2004). Both these facts may impact the likelihood of having a partner and the quality of this partner. Variations in paternal characteristics can also lead to disparities in newborn health. (Fredriksson et al., 2022). Fourth, older women in the school cohort may be more risk averse and only select themselves to become mothers if they have better health or economic conditions. This potential positive selection may imply that women born after the cutoff are less likely to become mothers but are more likely to have healthier babies. (McCrary and Royer, 2011). Finally, school starting age may impact age at motherhood. Older women in the school cohort may choose to pursue romantic relationships and start families at the same time as their peers and/or after they have finished their desired education level, at a later biological age (Johansen, 2021; Skirbekk et al., 2004). Postponing childbearing may give rise to complications, such as low birth weight and prematurity, as documented in the medical and non-medical literature (Fredriksson et al., 2022; Lean et al., 2017).

We use administrative data from university admissions from 2003 to 2016, together with hospital discharge records from the Spanish National Health System for years 2004 to 2015, and survey data from the Spanish Labor Force Survey (LFS) from 2000 to 2018 to probe the mechanisms behind the relationship between school starting age and infant health. We fail to find consistent empirical evidence that educational attainment, maternal health behaviors, partnership status and quality, and the selection into motherhood are primary drivers for reduced infant health in children born to mothers born after the cutoff. Instead, we find suggestive evidence of maternal age at birth playing a non-negligible role. Being born after the cutoff, instead of immediately before, significantly and robustly increases average maternal age by about 3 months, suggesting that increased maternal age is a plausible channel for the negative impacts of being born after the cutoff on offspring health at birth. We also present additional information to help assess the relevance of this potential channel against prior evidence reported in the medical literature.

Our study contributes to an increasing literature looking at the long-term impacts of school starting age. Much of this literature has focused on educational attainment, occupation, earnings, crime involvement, and risky behaviors, including papers by Black et al. (2011), Dobkin and Ferreira (2010), Fredriksson and Öckert (2014), Johansen (2021), Landersø et al. (2017), Muller and Page (2016), and Røed Larsen and Solli (2017). Together with the studies by McCrary and Royer (2011) and Fredriksson et al. (2022), ours is one of the few studies to look at the intergenerational consequences of school starting age. McCrary and Royer (2011) study the education systems in Texas and California, where redshirting is common.[1] They focus on the impact of school starting age on the infant health of children born to young mothers, who give birth during school enrollment or in the years immediately following high school graduation. Fredriksson et al. (2022) study the education system in Finland, where non-compliance with school starting age rules affects about 5% of the student population. They study women of childbearing age. Our paper complements this literature by providing evidence on a rather exhaustive list of potential mechanisms using mainly administrative population data of mothers and non-mothers for an education system where parents are not allowed to anticipate or delay school entry and failing to enroll children in school during compulsory education is considered a misdemeanor. In particular, we are able to explore educational attainment, education performance, marriage and partnership dynamics, partner quality, risky health behaviors, abortion, selection into motherhood, and age at first birth.

Our work also contributes to the medical and non-medical literature describing the relationship between maternal age and infant health (Aizer et al., 2020; Fraser et al., 1995; Lean et al., 2017; Royer,

[1] See for instance Dobkin and Ferreira (2010).

2004). The medical literature has emphasized the increased likelihood of adverse birth outcomes for older mothers, such as placenta previa, stillbirth, preterm delivery, and low birthweight (Carolan and Frankowska, 2011; Jolly et al., 2000; Lean et al., 2017; te Velde and Pearson, 2002).[2] Other studies using family fixed effects models have reached more mixed evidence, reporting worse outcomes for older parents in Norway but no differences in Finland (Goisis et al., 2017; Hvide et al., 2021).[3] Together with the study by Fredriksson et al. (2022), ours is the first study to provide evidence on the association between maternal age and infant health using school starting age methods.

2. Institutional Background

Compulsory education in Spain lasts 10 years (ages 6 to 16). The school entry cutoff is January 1st and compliance with the cutoff rule is very high. Since 1973 the Criminal Code considers a misdemeanor failing to enrol children in school during compulsory education from 6 to 14 years of age and parents have consequently refrained from redshirting their children, as shown by Berniell and Estrada (2020). Low- performing students can be retained, but, as shown by Calsamiglia and Loviglio (2020), grade repetition is uncommon in primary education, and more frequent in secondary education. To access university, students take a national university entry test after two years of post-secondary studies. University studies are mostly financed from public funds, although tuition fees are in line with French or Italian universities, and higher than those in German or Nordic universities (OECD, 2018).

During the four decades during which we observe potential mothers (1980-2020), the Spanish education system raised compulsory schooling from 14 to 16 years, and increased the availability of public education slots for 3 to 6 year-olds (see for instance Felfe, Nollenberger, and Rodríguez-Planas 2015). We show that these institutional changes are not behind our estimated impacts.

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[2] According to a recent meta-analysis of population-based studies, for first-time mothers, giving birth after age 35 is associated with almost a 30% higher risk of stillbirth, more than double the risk of low birthweight, and over 50% higher risk of preterm delivery (Lean et al., 2017).

[3] See Rosenzweig and Wolpin (1995) and Behrman and Rosenzweig (2004) on the limitations of the family fixed effects approach in the context of idiosyncratic responses to the previous birth.

Spain has a national universal health service established in 1986, that offers high-quality medical care during pregnancy. There has not been any important change in the health services covered during this period, with the exception of in-vitro fertility treatments. We also show below that our results are not driven by an increase in the number of multiple births born to older mothers in the school cohort, typical of these treatments (Goisis et al., 2019; Kulkarni et al., 2013).

Crude birth rates in Spain have fallen steadily since 1941 (Andrés et al 2015, see also Panel A of Figure A.1 in the Appendix). Additionally, marriage rates have decreased as the social acceptance of non-marital cohabitation schemes has surged over time (Rutigliano and Esping-Andersen, 2018). Furthermore, there has been a significant increase in fertility outside of marriage, rising from 11% in 1995 to 36% in 2010 and exceeding 50% in 2018. Consequently, similarly to other western countries, marriage decisions are increasingly dissociated with childbearing decisions (Bailey et al., 2014; Lesthaeghe, 2014). Female labor force participation rates have also increased (see Panel B in Figure A.1).[4] A 1980 reform increased the minimum legal working age from 14 to 16 years (see Bellés-Obrero, Jiménez-Martín, and Vall-Castello 2017). We also show that our results are not driven by these institutional changes.

3. Data

To compare birth outcomes for the children of women born around the school entry cutoff of January 1st, we use Vital Statistics Data from 1996 to 2018 from the Spanish National Statistical Institute. These population-level data provide detailed information on infant mortality, birthweight, gestation weeks, and parental demographic characteristics for the universe of births taking place annually in Spain, as recorded in the official national registry (see Borra, González, and Sevilla 2019). We supplement the publicly

[4] The definition of the unemployed was modified in the EU in 2000, so that the data up until that year are not directly comparable with those of later periods, explaining the jump in the series.

available files with the exact date of birth of each newborn and his/her mother, purchased from the Spanish National Statistical Institute.

We select all first births to Spanish mothers aged 15 to 44 years born up to 12 weeks before and after January 1st from 1996 to 2018. We focus on first births to obtain unbiased impacts of school starting age on maternal age at birth and infant health. As emphasized by Rosenzweig and Wolpin (1995) and McCrary and Royer (2011), the health of the first child may influence the decision of having another child and parental investments for the second child prior or during pregnancy. Any estimating strategy including second and higher order births will be unable to distinguish the impact of maternal age at birth from the impact of the health of the first-born.[5]

We include in our analysis the first baby born in a multiple birth, but also include a robustness check including only singleton births (see Section 5.1). In addition, we focus exclusively on Spanish mothers to assure that they faced the Spanish school starting age cutoff of January 1st. For mothers born in Spain who then moved to a foreign country during the school years, that might not be the case but given the low proportion of return migration rates of Spanish nationals, this should not be a problem.[6] We use data from 1996 onwards because this is the first year for which maternal country of birth was recorded in birth records. We consider mothers in their childbearing age, 15 to 44 years old. We perform this selection by cohort of birth each year, instead of by age at time of birth to assure a balanced sample of mothers before and after the cutoff each year of data. There are less than 0.001% of births to mothers under 15 and about 0.02% of births to mothers over 44 years of age. In Section 5 we show that our results hold for an unrestricted sample of all first births to Spanish mothers.

[5] In fact, in Section 8, we show that a within-family model, estimated in the sample of children who have at least one full sibling born within the 1996-2018 period, suggests insignificant impacts of maternal age on infant health.
[6] According to our own calculations using 2011 Census microdata, just 0.3% of Spanish females born in 1985 to 1995 lived out of Spain in 2001, during their compulsory schooling years.

Our main outcome variables are mortality, birthweight, indicators for low birthweight (below 2,500 grams) and very low birthweight (below 1,500 grams), gestation weeks, and indicators for premature birth (before 37 weeks) and early pre-term birth (before 34 weeks). Table 1 shows that our main sample is composed by about 4 million observations, where the mother is about 30.6 years of age on average (11,196/365), the baby's weight at birth is over 3,000 grams, and gestation lasts about 39 weeks. Also, about 7% of babies are born with low birthweight, 0.8% with very low birthweight, and almost 4% with high birthweight. About 7% are born pre-term, and almost 2% are early pre-term births.

To test the validity of the RD design, we study women's health and family background at birth, using Vital Statistics from 1980, first year that birth records include health data, to 1995 We explore birthweight, gestation weeks and parental demographic characteristics of potential mothers born around the school entry cutoff. Panel A in Table A.1 in the Appendix shows descriptive statistics for these variables.

To explore potential mechanisms, we use three additional datasets: administrative data from Andalusian University Entry exams from 2003 to 2016, administrative data from Spanish Hospital Discharge Records from 2004 to 2015, and survey data from the Spanish Labor Force Survey from 2000 to 2018.

We use administrative data on all University Entry exams from Andalusia, the largest region in Spain, from 2003 to 2016 to study educational attainment at both the extensive and the intensive margins.[7] The Andalusian university system is made up of one private and ten public universities, including two founded in the early 16th century. In total there are 38 campuses and 159 university centers

[7] Andalusia is the largest Autonomous Community in Spain. Its population in 2022 is 8,472,407 inhabitants (Spanish National Statistical Institute (2022)), which represents 17.9% of the country's population and places it as the most populated Autonomous Community (AC) and its extension is 87,592.7 km2 (Institute of Statistics and Cartography of Andalusia (2022)), 17.3% of the country's surface, being the second largest AC. If Andalusia were an EU country, it would rank 15th by population, very close to Austria and the 13th country by size, above Austria and slightly below Portugal (Eurostat (2022)).

that add up to a total of 244,210 students, almost 20% of all students in Spain (Spanish Government. Ministry of Universities 2020). All Andalusian students enrolled in post-secondary education who aim at pursuing university studies either in Andalusia or in other regions sit the university admissions test.

We use Admissions data to study the total number of students taking the test, together with indicators for passing the test and grading for those who passed. Because of confidentiality concerns, for each student we have information on sex and exact date of birth, but we do not have access to school or location identifiers. There was a change in the grading system in 2010 and therefore we study grades for 2003 to 2009 and for 2010 to 2016. In addition, the richness of the data allows us to identify those students passing the test in the ordinary call.[8] Panel B in Table A.1 also shows descriptive statistics for the variables in this dataset.

TABLE 1

We use population data from the Hospital Discharge Registry from 2004 to 2015 to study healthcare outcomes related to mental health and risky health behaviors of potential mothers. The registry records all overnight stays in the network of hospitals of the Spanish National Health System. Each record corresponds to an individual visit or stay and, together with main and secondary diagnoses following the International Classification of Diseases (ICD-9-CM), it provides basic patient characteristics such as sex and exact date of birth.[9] We conduct the analysis at the date of birth level by computing the number of hospitalizations of potential mothers aged 15 to 44. We study lung cancer ("Malignant neoplasm of trachea, bronchus, and lung" ICD-9-CM code 162), liver problems ("Chronic

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[8] There are two entry-exam calls in Spain, one in June (ordinary call) and another in September (extraordinary call). The last one is typically sat by students not being able to sit or not passing the first call.

[9] Comparable analyses using more recent data are complicated because of a change in the system of classification of diseases from 2016 onwards (ICD-10 rather than ICD-9 CM).

liver disease and cirrhosis" code 571), mental health ("Mental disorders" codes 651–59), and aggressions ("Homicide and injury purposely inflicted by other persons" codes E960-E969). Panel C in Table A.1 also shows descriptive statistics for the variables in this dataset.

We complement the above administrative information by looking at educational attainment, fertility, marriage, and partnership outcomes in adulthood for all women born around the January 1st cutoff, using the Spanish Labor Force Survey from 2000 to 2018. We first examine indicators for having secondary studies and university studies as highest educational attainment at different ages. We then look at the likelihood of being married and living in a partnership at different ages. We finally study the probability of giving birth before 18, 23, 28, 33, 38, 43, and 48 years of age. Panel D in Table A.1 in the Appendix provides summary statistics.[10]

4. Empirical Strategy

We want to estimate the impact of maternal school starting age on infant health outcomes. As in McCrary and Roger (2011), we follow a Regression Discontinuity (RD) design to compare offspring health outcomes of women of approximately the same biological age who were born in adjacent school cohorts around the school cutoff date. We estimate the following reduced-form equation:

$$Y_{idt} = \alpha + \beta Treat + \gamma_1 f_1(Date) + \gamma_2 f_2(Date) * Treat + \tau_t + \varepsilon_{idt} \quad (1)$$

where *Y* denotes our infant health outcomes of interest for mother *i* born on day of the year *d* in cohort *t*. More specifically, we study the following measures of health at birth: 24-hour mortality, birthweight, low birthweight, very low birthweight, high birth weight, gestation weeks, pre-term birth, and early pre-term birth. The variable *Treat* is an indicator for births on or after January 1st (and before July 1st), each year; *Date* is the running variable, day of the year, defined as the difference between the date of birth of the potential mother and the January 1st cutoff; $f_i(.)$ is a function of the running variable; and τ_t are cohort

[10] As a robustness check we also study educational attainment using this dataset by computing indicators for primary education or less, secondary education, and university education.

fixed effects computed for each year beginning in July till the following June. Our coefficient of interest is β, which captures the potential discrete jump in outcomes due to the school starting age legislation.

We estimate the reduced form equation in (1) using different optimal bandwidth selection methods and different functions of the running variable, as suggested by Cattaneo et al (2019).

In order to assess the validity of our identification strategy, we test for potential manipulation of the running variable and balance in covariates in maternal characteristics at the time of potential mothers' birth (See section 4.1 below). Absent exact date of birth for these outcomes, we estimate the following equation, using the local randomization framework for RD designs (Calonico et al., 2019):[11]

$$Y_{imt} = \alpha + \beta Treat + \tau_t + \varepsilon_{imt} \tag{2}$$

where *Y* denotes the outcome for mother *i* born on month *m* in cohort *t*., more specifically, mortality likelihood, birthweight, premature birth, normal birth, maternal age, whether the mother is married, whether she is employed, and whether the child has a known father. The variable *Treat* is an indicator for women born on or after January 1st of each year; and τ_t are cohort fixed effects computed for each year beginning in July till the following June. Again, β is the coefficient of interest.

We estimate equation in (2) using windows of one month around the cutoff, as suggested by Cattaneo et al. (2019) when continuity assumptions of the running variable do not hold.

4.1. Validity of the Research Design

Before looking at the impact of maternal age on infant health outcomes, we check whether women at either side of the school-entry cutoff were comparable with respect to other characteristics before entering the school system. In particular, we show that potential mothers' birthdates can be considered quasi-random around the cutoff.

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[11] Equation (1) is, however, used when looking at the impact of being born after the cohort on potential mothers' health outcomes and student participation and performance on university entry exams (see Section 5).

Figure 1 shows that there was no bunching of births around December 31 during the 1980s and 1990s. This evidence is consistent with the idea that families were unable or unwilling to control the exact date of birth around the school entry cutoff. If we follow a local randomization approach, for a one-month window around the cutoff, the number of women born before and after the cutoff should be approximately 50%. The observed share of women born in January vs. December is exactly 0.500 (259,772 women in December and 259,041 women in January) and we fail to reject the null hypothesis that the sample has been randomly assigned by a binomial function of a 0.5 success probability (p-value 0.311). We thus find no evidence of "sorting" around the cutoff in the one-month window. The number of treated and control observations in this window is entirely consistent with what would be expected if birthdates were assigned randomly. Table A.2 in the Appendix further shows that, at the time of birth, there are no significant differences among potential mothers' and their families' characteristics by treatment status. All in all, unlike the evidence presented by Buckles and Hungerman (2013) for the US, we find no evidence suggesting that Spanish mothers tried to conceive or give birth at specific dates, around the school entry cutoff.

FIGURE 1

5. School Entry Rules and Infant Health

We now turn to our main results on the impact of being born after the school entry cutoff on birth outcomes.

Figure 2 and Table 2 report our main reduced form results of the impact of being born after the cutoff, instead of immediately before, on infant health (see also Figure A.2 in the Appendix). In general, there are not many differences in the health outcomes of first births of mothers born before and after the cutoff. We may conclude that for the average mother, going to school one year later poses no risk for the health of the child. However, we find a significant increase in the likelihood of children born with very low birthweight. The likelihood of having a newborn with less than 1,500 grams increases by approximately 0.16 percentage points (between 0.15 and 0.18 percentage points, depending on the specification). Given that in the population there are only 0.88 children per 100 born with very low birthweight, being born after the school cutoff date, instead of immediately before, increases the likelihood of having a very low birthweight newborn by 18 percent.[12] This very significant and robust result is also coincident with a significant decrease in gestation length, in particular, in the likelihood of having a child before 34 weeks of gestation, which is however significant only at the 10 percent level in some of the specifications (see Figure A.3 in the Appendix). On average, mothers being born after the cutoff face an increase in the likelihood of having an early preterm birth of 0.20 percentage points (between 0.17 and 0.22), that is, about 11 percent. Most specifications also find a corresponding reduction in gestation weeks of about 2.5 percentage points (0.06 percent).[13]

FIGURE 2

TABLE 2

5.1. Robustness Checks

In this section, we conduct different supplementary analyses to show that our main results are robust to several changes in sample selection and model specification. In particular, we rule out that the estimated impacts of school starting age on infant health outcomes, first, are not driven by sample selection of mothers aged 15 to 44, second, are robust to controlling for pre-determined variables, third, remain when only singleton births are selected, and fourth, are not due to other concurrent changes in education or

[12] We computed p-values using the Romano and Wolf correction procedure to take into account that we were testing multiple hypotheses for the same child, and we found that the coefficient for very low birth weight remained statistically significant at the 95% level (p-value = 0.0150).

[13] One potential concern is that our dataset lacks enough statistical power to test economically interesting hypothesis, due to an insufficient number of observations local to the cutoff (McCrary and Royer, 2011). In Table A.5 in the Appendix, we show that we have enough power to detect effect sizes of economically significant impacts for most of our health outcomes. We follow Geruso and Spears (2018) and adopt an ad-hoc conservative value of 5% of the sample mean. We find that we do not have enough power to detect effect sizes of that magnitude for mortality or early pre-term birth, but we do have power to detect effect sizes larger than 5% of the sample mean for birthweight, low birthweight, high birthweight, gestation weeks, and pre-term birth.

labor legislation that may be impacting children outcomes.

Table 3 displays the results for these exercises. Column 1 in Table 3 reproduces the estimates of our main analysis in Column 1 in Table 2. Column 2 presents estimates for the unrestricted population of all Spanish mothers and shows that the RD estimates do not change with the sample selection. In particular, being born after the school entry cutoff increases the likelihood of a very low birthweight birth by 19 percent and reduces the gestation period by about 0.06 percent. Column 3 adds all available pre-determined covariates: marital status, no registered dad, maternal employment in a high skilled industry, child's sex, and multiple birth. Results remain again virtually unchanged. That is, the likelihood of having a very-low-birthweight newborn increases by 19 percent, the likelihood of having an early pre-term birth increases by 10 percent, and gestation weeks drop by 0.05 percent.

Column 4 in Table 3 conducts the analysis leaving out of the sample all multiple births, about 65,000 observations (1.5 percent of the sample). Fertility treatments, recently made publicly available in Spain, tend to increase the chances of multiple births (Buckles, 2013). By examining just singleton births we aim at ruling out technological improvements related to infertility as an alternative source for our health at birth outcomes. We continue to find very similar impacts of being born after the cutoff on all infant health measures, which indicates that our results are not likely to be due to changes in fertility treatments availability. Specifically, as reported in our main analysis in Table 2, the likelihood of having a very low birthweight baby increases by 16 percent and the likelihood of having an early pre-term birth increases by 9 percent, though is only marginally significant.

Lastly, columns 5 and 6 in Table 3 show that neither the 1980 change in the minimum working age nor the 1990 increase in compulsory education are behind the estimated impacts of school starting age on infant health. In Column 5 we leave out of the analysis mothers born in 1965, 1966, and 1967, potentially affected by the Workers Statute reform on 1980 (Law 8/1980). Similarly, to our main results (reproduced in Column 1), we continue to find that early preterm births increase by 13 percent and the likelihood of having a child with very low birthweight increases by 19 percent. In Column 3, we now leave out of the analysis cohorts 1979 to 1983, potentially affected by the staggered introduction of the 1990 new education law (LOGSE). Results are very similar to those reported in Table 2. The likelihood of very low birthweight increases by 16 percent. The likelihood of early preterm birth increases by 8 percent but is no longer significant.

All in all, the results prove robust to sample selection, identification, and potentially concurring technological and policy changes.

TABLE 3

6. Potential Mechanisms

Thus far, we have provided evidence of significant impacts of school starting age on very-low birthweight and possibly early pre-term birth. In this section we investigate the most prominent channels through which school starting age may affect offspring health at birth. We address educational attainment and educational performance, mental health and risky health behaviors, selection into partnership and partners quality, selection into motherhood, and maternal age at birth.

6.1. Educational attainment and performance

Research has shown that older students in the cohort tend to perform better during primary education (Bedard and Dhuey, 2006; Calsamiglia and Loviglio, 2020; Dhuey et al., 2019). The extent to which these initial differences translate into long-term differences in educational attainment depends mostly on the specific features of the education system involved. In societies where children can leave school at a specific age, such as the United States, older students in the cohort can drop out before ending compulsory education. As a result, the school leaving age legislation creates a mechanical difference in educational attainment, where individuals born after the cutoff tend to acquire fewer years of schooling than individuals born before. Researchers have used this mechanical difference to study the causal impact of education on longer term outcomes: wages (Angrist and Krueger 1991), employment (Dobkin and Ferreira, 2010), and fertility and children's health at birth (McCrary and Royer, 2011). In Northern

European countries, the law specifies that students must complete a minimum number of years of education, and the impact of school starting age comes from absolute or relative maturity and not from being able to drop out. In countries such as Sweden with ability-tracking education systems, the initial advantage of being relatively older in the school cohort increases educational attainment -though not wages (Fredriksson and Öckert, 2014). However, in Finland and the Netherlands, with a tracking system that allows changes at a later point, or in Norway, with no tracking during compulsory schooling, initial differences do not translate into higher educational attainment (Black et al., 2011; Fredriksson et al., 2022; Oosterbeek et al., 2021).[14]

Previous literature has reported higher grades for students born after the school entry cutoff date in Spain, although the difference decreases from primary to lower secondary school (Calsamiglia and Loviglio, 2020).[15] If this initial advantage translates into different levels of educational attainment at either side of the cutoff, maternal education could be an important channel for estimated differences in health outcomes at birth. We provide evidence showing that date of birth does not significantly impact either the extensive or the intensive margins of potential mothers' educational outcomes.

Using administrative population data on all university admission test from 2003 to 2016, we show that the initial advantage is virtually non-existent at the end of postsecondary schooling, when students are 18 years old. To start with, date of birth does not predict the probability of taking the admission test. As shown in Panel A of Figure 3, there is no evidence of any differences in the likelihood of dropping

[14] School starting age policies will also mechanically affect potential after-school experience, given that women born after the cutoff will finish schooling a year later. We are not aware of any evidence linking maternal potential labor market experience (of after-school experience) to birth outcomes, other than through an indirect effect on maternal earnings (Mocan et al., 2015). However, we also address this potential alternative explanation in Section 7.2.

[15] Red-shirting young students is very uncommon in Spain. Since 1973 the Criminal Code considers a misdemeanour failing to enrol children in school during compulsory education from 6 to 14 years of age. Parents have refrained from redshirting their children. For instance, according to official data from the Spanish Ministry of Education, the percentage of students of 7 years of age enrolled in the first year of primary studies ranges from 4.1% in school year 1982-1983 to 2.5% in school year 1992-1993 (Spanish Government. Ministry of Education and Vocational Training, 2022). However, these figures include not only red-shirted students but also repeaters.

out of high school, nor any indication that students are self-selected out of the university entrance test in our data. We also test for the continuity of the density function for the distribution of birthdates around the January 1st cutoff and find that the difference in the density of observations before and after the cutoff is non-significant and therefore there is no evidence of systematic manipulation of the date of birth around January 1st.[16] These results are also consistent with those obtained estimating equation (2) on LFS data for the likelihood of having secondary or university studies as highest educational attainment (Panels B and C of Figure 3). The LFS survey potentially includes all women in Spain, regardless of whether they took any university admissions test. The mechanical effect of not being old enough to have finished university is apparent for younger ages, but, after approximately age 23, month of birth stops predicting the highest qualification achieved, either secondary schooling or university degree. This evidence suggests that differences in education at the extensive margin are unlikely to be responsible for the estimated impacts of school starting age on infant health.

FIGURE 3

We also explore the intensive margin of educational attainment estimating our RDD model in equation (1) for different outcomes of educational performance in the University Admissions data. Consistent with the findings by Calsamiglia and Loviglio (2020) for younger children, panels A and B in Table A.4 and Figure A.4 show that older students are about 8 pp (36%) less likely to have repeated a previous school year and about 0.4 pp (100%) more likely to be advanced for their age. However, Panels C to H in Table A.4 and Figure A.4 show that there are no significant differences in scores obtained on the university entry test by date of birth, irrespective of the call, ordinary or extraordinary, and the examination system, pre- or post- 2010. While the point estimates are not very precise, we can reject that

[16] The t-statistic for the density manipulation test is 0.8256 (p-value: 0.4090) when using a linear uniform kernel function.

older students in the cohort (born after January 1st) obtain university entry scores that are significantly different from scores from younger students born before the cutoff. These findings are in line with the results reported by Dobkin and Ferreira (2010) for California and Texas and Black, Devereux, and Salvanes (2011) for Norway who do not find evidence that school entry laws affect college attendance and completion or educational attainment.

Overall, the results in Figures 3 and A.4 and Table A.4 do not support the notion that the reduced health at birth of the offspring of mothers born after the cutoff is primarily driven by maternal educational disparities at the extensive or the intensive margins.

6.2. Risky health behaviors and mental health

Potential mothers born after the cutoff are also older than their peers during the adolescent years. Research has shown that their relative maturity may keep them away from risky behaviors, such as smoking, drinking, and considering crime, and mental health disorders (Balestra et al., 2020; Johansen, 2021; Landersø et al., 2017; McCrary and Royer, 2011; Peña, 2020). If such relative maturity impacts have long-lasting effects on Spanish potential mothers, our estimates for the impact of maternal age on health outcomes may be biased downwards.

We estimate the impact of school starting age on different annual hospitalization rates using our RDD model in equation (1) and population data from Spanish Hospital Discharge Records. In particular we study hospitalizations due to lung neoplasms as a proxy for smoking, hospitalizations due to liver problems as a proxy for excessive drinking behavior, hospitalizations due to mental health problems to capture psychological wellbeing, and hospitalizations due to aggressions to capture crime involvement. Figure 4 shows that being born after the cutoff, instead of immediately before, does not impact hospitalizations due to lung cancer, liver problems, mental health issues, or non-accidental injuries for potential mothers aged 16 to 44. This evidence fails to support the hypothesis that the estimated differences in children's infant health by maternal date of birth are biased due to differences in maternal behaviors or mental health problems.

FIGURE 4

6.3. Selection into partnership and partners' characteristics

Prior literature has shown that school starting age may also affect partner quality through assortative mating, given that older women may form unions with older, possibly wealthier individuals (McCrary and Royer, 2011). We would expect children from these unions to have better health at birth. Conversely, women born after the cutoff may tend to form partnerships at the same time as their peers in their school cohort and may experience life events such as living in partnership at a later biological age (Skirbekk et al., 2004). It could be that these women may be less likely to have a partner when they become pregnant, which may negatively affect their offspring's health.

Panel A in Figure 5 studies effects on partners' age and educational attainment, using our RDD model in equation (2) and data from the Spanish Labor Force Survey. All panels show no significant impact on the age or human capital characteristics of women' partners. Also, Figure A.6 in the Appendix documents that there are no differences in the probability of being in a partnership at any age nor in the probability of being married after the age of 25. Therefore, unlike the evidence documented by Skirbekk, Kohler, and Prskawetz (2004) for Sweden but similarly to the results presented by McCrary and Royer (2011) for the US, these findings show that there is no evidence that school-entry policies influence selection into partnership. Furthermore, panel B in Figure 5 shows that there are no differences in the likelihood of having a father according to the mothers' date of birth, using Vital Statistics data (equation 1). All in all, it is therefore unlikely that neither selection into partnerships, partnership status, nor partners' characteristics are behind the estimated impacts of school starting age on infant health.

FIGURE 5

6.4. Selection into motherhood

Even if long-term educational attainment, health, and partner characteristics are not significantly influenced by being older in a cohort, being born after the cutoff may affect the likelihood of becoming a mother. Women at both sides of the school entry cutoff may be equally likely to be career oriented after the age of 25, as we have seen, but being older in the cohort may impact their chances of becoming mother. If only very healthy women born after the cutoff succeed in becoming pregnant or decide to become pregnant, their children will also show healthier outcomes. In this case, comparing health at birth outcomes of children born to mothers born before and after the cohort would offer a biased picture in which children born to older mothers show better health outcomes than they would have had if all mothers had had the same chances of success in becoming pregnant independently of their date of birth.

Figure 6 (and Panel A in Table A.5 in the Appendix) shows the impact of being born after the cutoff, instead of immediately before, on the probability of being mother for the first time before specific ages, using Labor Force Survey data and our RDD model in equation (2). We find that, even if being born after the cutoff reduces the likelihood of having had a child between the ages of 18 and 40, it does not affect the chances of having had a child after the age of 40. Figure 6 shows that school entry rules influence *when* potential mothers have their children, but do not impact the probability of *ever* becoming a mother. Figure A.7 further shows that there is no bunching among those women becoming mothers at either side of the cutoff of January 1st using Vital Statistics data.[17] We therefore find no evidence of selection into motherhood as a result of being born early in a cohort. Panel B in Table A.5 in the Appendix additionally documents that there are no systematic differences in the number of children born to women born after the school entry cutoff after the age of 40, either. Like Fredriksson et al. (2022), this evidence

[17] We fail to reject the null hypothesis that the sample has been randomly assigned by a binomial function of a 0.5 success probability (p-value 0.812). We thus find no evidence of "sorting" of maternal birthdates around the cutoff. That is, the number of mothers born before and after the cutoff is entirely consistent with what would be expected if motherhood had been assigned randomly.

supports the assumption that being born early in the cohort does not influence selection into motherhood or completed fertility.[18]

FIGURE 6

6.5. Motherhood delay

Individuals tend to interact with other people in their same school cohort, that is, people of their same social age, instead of individuals of their same biological age. This social age, determined by the average age of the school cohort, may influence the timing of social behaviors such as marriage, partnership, and fertility (Røed Larsen and Solli, 2017; Skirbekk et al., 2004). We have seen that school entry policies do not influence partnership or marriage timing by much. However, consistent with recent evidence documenting the decoupling of marriage and motherhood in Spain and other Southern European countries (Lesthaeghe, 2014), school starting age could influence the timing of motherhood, that is, the biological age at which women have their first child.[19] The medical literature has emphasized the increased likelihood of adverse birth outcomes for older mothers (Lean et al., 2017). Recent cohort studies document a j-shaped relationship between maternal age and infant mortality, pre-term delivery, and low birthweight, with increases in risks after approximately ages 29-30 (Schummers et al., 2018;

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[18] Figure A.8 in the Appendix further shows that there are no differences in the likelihood of having had an abortion between women born before or after the school entry cutoff date.

[19] Figure A.9 shows how social age can influence fertility timing, creating a difference on maternal age between those women born before the cutoff and those born after the cutoff. For instance, when we compare women of the same biological age born around the school cutoff of January 1st in 1974, we may observe that 1) women born in January 1974 are oldest in their school cohort and tend to have children alongside their peers at a later age, when they are older, and 2) women born in December 1973 are youngest in their school cohort and also tend to have children alongside their peers at an earlier age, when they are younger. As a result, there may result a jump in the biological age at which women have their first child around the school entry cutoff of January 1st. Women born after the cutoff tend to "delay" childbirth, relative to women born before the cutoff.

Weng et al., 2014).[20][21] In what follows, we test whether there are differences in age at first birth between mothers born before and after the school entry cutoff date.

We estimate the impact of school starting rules on age at first birth, using Vital Statistics population data and our main RDD model in equation (1). We compute maternal age in days by subtracting the exact date of birth of the mother from the exact date of birth of the child.

Consistent with our results in Figure 6, Panel A in Figure 7 shows that being born after the school entry cutoff in Spain delays mothers' age at first birth by approximately 3 months (about 90 days). Panel B shows that this result is quite robust to the size of the bandwidth. Similarly, the point estimates in Table A.6 are significantly positive and range from 87 to 91 days of delay. This result is also highly robust to changes in bandwidth selection methods, kernel functions, and polynomial orders (Panel A in Table A.6) and similarly robust to sample selection, controlling for pre-determined co-variates, including only singleton births, and controlling for other concurrent changes in education or labor legislation that may be impacting mothers' age at first birth (Panel B in Table A.6). Overall, the evidence provided is suggestive that the main mechanism behind the worse health outcomes of children from mothers born after the school entry cutoff is increased maternal age at birth.

Given that the average first child is born to a mother aged almost 31 years, the delay involves a 0.8% increase in the age of the mother. We may wonder whether the average 3-month delay is due to some old mothers delaying by a couple of years whereas other younger mothers do not delay at all. Panel A in Figure A.11 plots the distribution of age at first birth for both treatments and controls and shows

[20] Advanced maternal age, defined as giving birth after age 35, has been associated with increased risk of maternal circulatory problems during pregnancy (placenta previa), gestational diabetes, emergency Caesarean section, stillbirth, preterm delivery, and low birth-weight (Carolan and Frankowska, 2011; Jolly et al., 2000; te Velde and Pearson, 2002). According to a recent meta-analysis of population-based studies, for first-time mothers, giving birth after age 35 is associated with almost a 30% higher risk of stillbirth, more than double the risk of low birthweight, and over 50% higher risk of preterm delivery (Lean et al., 2017).

[21] Figure A.10 shows average birth outcomes in Spain by maternal age. The J-shape indicates that both lower, but especially higher maternal ages are associated with adverse birth outcomes, such as low birthweight and prematurity.

that age at first birth increases similarly along the age distribution. Panel B shows that the regression discontinuity estimate is quite robust across deciles of the age distribution using quantile regression techniques. Hence, there is no indication that our results are driven by a few women delaying motherhood substantially.

FIGURE 7

7. Interpreting the Magnitudes in the Relationship between Infant Health and Age at Birth

We have seen that mothers born after the school entry cutoff tend to delay motherhood by about 3 months compared to women born before the cutoff. This delay is the main mechanism behind increases the likelihood of having offspring with very low birthweight (by about 18 percent) and early preterm (by about 11 percent). In this section, we benchmark our RD estimates against the corresponding cross-sectional and within-family maternal-age gradients, estimated from non-experimental variation in maternal age at birth in the same Vital Statistics population data. By comparing RD, within-family, and observational estimates computed in the same data, we are able to rule out external validity concerns as an explanation for our findings. Additionally we also compare our findings to previous estimates from quasi-experimental studies such as Goisis et al. (2017) and Fredriksson, Huttunen, and Öckert (2021).[22]

The estimated impact of school entry policies on maternal age at first birth of around 90 days, shown in Figure 6, is large compared to other quasi-experimental differences in age at first birth. For instance, Gershoni and Low (2021) report that free availability of in vitro fertility treatments in Israel increased maternal age at first birth by about 6 months. Observational evidence for the US also documents for instance that the difference in age at first birth between women in the highest and the

[22] We do not include the results from the within-sibling analysis of Hvide et al. (2021), focused on birth defects, that includes graphical evidence of the impact of parental age on low birth weight, very low birth weight and pre-term birth.

lowest quartiles of educational attainment is about 6.5 years in recent cohorts (Bailey et al., 2014). Given that the highest quartile involves approximately 6 more years of education compared to the lowest quartile, one more year of education is associated with a 12 month increase in age at first birth in the US. Our own data from the Spanish Labor Force Survey indicates that university studies are associated with a delay in motherhood of about 4.5 years, that is, about 9 months per additional year of education. Our estimated 3-month increase in maternal age at first birth as a result of school entry policies in Spain is therefore between 33 and 25 percent of the difference in maternal age due to one additional year of education.

Table 4 compares our RD-estimates from Table 2 to both cross-sectional correlations and within-family estimates in our data, as well as previous results in the literature. Three key conclusions arise from this comparison exercise. First, our small and statistically insignificant results for most measures of infant health at birth such as mortality, birthweight, and pre-term birth (in Panel A) are not consistent with correlational evidence (in Panel B) nor previous findings from the medical literature (in Panel D). For instance, previous epidemiological studies find a systematic association between maternal age and the risk of stillbirth (see for instance Flenady et al. 2011; Lean et al. 2017). In particular, studies report a 75 percent increase in the risk of stillbirth for mothers aged over 35 years compared to younger mothers (see column 1 in Panel D of Table 4). We fail to find a significant causal impact of delayed motherhood (starting school later) on infant mortality (Panel A).[23]

Second, with the exception of our estimates for the impact of maternal age on the risks of very low birthweight and early pre-term birth, our small and statistically insignificant results for birthweight, low birthweight, gestation weeks, and premature birth are consistent with previous quasi-experimental and siblings fixed-effects evidence. For instance, using within-family variation in age at birth, both our own estimates (in Panel C) and Goisis et al. (2017) (in Panel E) find statistically insignificant associations

[23] See below, however, our discussion about the lack of power in our data to detect some very small impacts.

between maternal age and the risk of low birthweight or preterm delivery. Similarly, Fredriksson, Huttunen, and Öckert (2021) show statistically but not economically significant decreases in birthweight and weeks of gestation of about 0.6 and 0.19 percent, that correspond to motherhood delays of about half a year (Panel F in Table 4). We fail to find any statistically significant impact of motherhood delay on birthweight measured as a continuous variable, but our 0.027 percentage point decrease in gestation weeks as a result of a 3-month delay in motherhood corresponds to a 0.14 percent increase in gestation weeks for 6-months, which is very similar to the 0.19 percent found by Fredriksson et al (2021).

Finally, the third conclusion we can draw from this comparison exercise is that both correlational evidence and within family models may underestimate the impact of maternal age at first birth on more extreme measures of health at birth such as the risks of very low birthweight and early pre-term birth.[24] We find an average 18-percent increase in the likelihood of a very low birthweight child due to a 3-month delay in childbirth (see Panel A in Table 4). Cross-sectional estimates are significant but small, of about 0.3% (Panel B). Furthermore, within-family estimates as reported in Panel C are small but also insignificant, consistent with the results in Goisis et al (2017), that, however, do not look at very low birthweight. Similarly, we find marginally significant increases in the likelihood of early pre-term birth as a result of 3-month delays in maternal age (Panel A), that we fail to capture in correlational or within family models (Panels B and C). In sum, the evidence gathered here suggests that, when assessing the impacts of delayed motherhood, it is crucial to look for not only exogenous changes in age at birth, but also identification strategies that include all first births.

TABLE 4

[24] The fact that we fail to find any significant impacts of maternal age on children's health at birth when we compare children born to the same mother also allow us to rule out any large impacts of maternal after-school experience on health at birth. Siblings born at different points in time differ also on maternal after-education experience. If lower after-education experience was a plausible explanation for our results, we should expect the youngest child, born to a more experienced mother, to have systematically better health at birth than the oldest child born to a less experienced mother. The fact that both have similar health outcomes suggests that after-school experience, conditional on maternal education, does not seem to be driving our results.

8. Conclusions

In Spain, women who are born in January start school a year later than those born the previous December, despite being essentially the same age. We exploit this strict school entry cutoff to study the effects of school starting age on infant health. We compare the health at birth of the children of December- versus January-born women, following a regression discontinuity design and using administrative, population-level data. We find small and insignificant effects on average weight at birth, but the children of January mothers are more likely to be born with very low birthweight. January-born mothers are 11% more likely to have an early pre-term birth, and 18% more likely to give birth to a child with very low birthweight. These impacts are quantitatively and economically significant, and consistent with the associations reported in the medical literature. However, unlike this literature, we find no impact on average birthweight, the fraction of low birthweight (<2,500g.) babies, or the risk of premature birth (before 37 weeks of gestation).

We then look at potential mechanisms. We show that, compared to women born before the school entry cutoff date, January-born women have similar educational attainment, are equally likely to be in a partnership (with similarly old and educated mates), are equally likely to become mothers, and are equally likely to have mental health problems and engage in risky health behaviors. However, January-born women finish school later and are (several months) older when they have their first child. Our interpretation is that the estimated differences in health at birth by maternal date of birth are driven by January mothers delaying motherhood by several months. Age at birth has been increasing for the past few decades in many countries, and correlations show that health at birth is worse for children with older mothers. We find that giving birth just a few months later may reduce offspring health.

Our "treated" January-born women are older at the time of their first birth. Previous literature found that older women have on average higher earnings at the time of first birth (Klevmarken and Quigley, 1976). We lack information on earnings to test whether earnings are higher for women born after the cutoff at the time of first birth. However, if older age is associated with higher earnings, which in turn may have positive effects on fetal health, our estimates would be biased towards zero, since we find that the children of older mothers have worse health at birth. In addition, these women are older than their peers during school, which may also confer non-cognitive advantages that may persist until reproductive age (Lubotsky and Kaestner, 2016). We lack information to test whether women born after the cutoff possess better non-cognitive skills at the time of birth. However, if older age is associated with better non-cognitive outcomes, which in turn have positive effects on fetal health, this would again bias our estimates towards zero.

Finally, age at first birth may also influence the start of prenatal care (McCrary and Royer, 2011). We lack information on prenatal care visits to test whether older women are more likely to receive prenatal care in the first trimester and to attend a larger number of prenatal care visits. However, we do not expect to find many differences in prenatal care by age at first birth in our data, given that healthcare is universal, public, and copayment-free in Spain. In addition, if older age is associated with earlier, more frequent prenatal care, which in turn may have positive effects on infant health, our estimates would again be biased towards zero.

We find that the distribution of age at first birth is shifted across the whole age distribution. If the effects of age at first birth on newborn outcomes follow a J-shaped pattern, as suggested by Figure A.10, then our results may be mixing null or even positive health effects at younger ages with negative ones for older mothers. Our results should thus be interpreted accordingly.

Comparing women at either side of the school-entry cutoff seems a plausible identification strategy. We found no evidence of sorting of births around the threshold or significant differences in predetermined characteristics between the two groups of women. However, we acknowledge that our estimates are specific to the subpopulation of women whose age at birth is affected by school entry policies. These women affected by school entry policies are evenly distributed along the distribution of age at birth. Therefore, our research strategy is not able to look at the specific impact of increased age at birth for advanced-maternal-age mothers. All in all, we interpret our results as suggestive of an impact of maternal age on infant health, concentrated in the left tail of the birthweight distribution, with slightly older mothers more likely to give birth to (very) premature babies.

Our results have several important implications. We show that being older in a school cohort can negatively affect offspring's health at birth. Families should be aware of potential long-term costs of holding children back to start school older. Also, policies that discourage "red shirting" could yield substantial positive effects on infant health.

Furthermore, in line with Frederikson et al. (2022), our analysis suggests that postponing childbirth by just a few months may significantly impact infant health in the next generation. As with any quasi-experimental evidence we cannot extrapolate our results beyond the 3-month variation. However, in view of the steady increase in the average age at first birth in OECD countries over the past four decades, further research will be needed to extend our analysis beyond a few months to further gain a deeper understanding of the implications of delaying motherhood.

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Table 1. Descriptive statistics. Main Sample 1996-2018. First Births from Mothers 16-44 Years Old.

	Obs.	Average	St. dev.	Median
Panel A. Infant Health Outcomes. All Mothers				
Treatment	4467784	0.4988	0.5	0
Maternal age in days	4467784	11194.2176	1898.673	11286
Mortality	4467784	0.0043	0.066	0
Birth weight	4265527	3183.5412	510.438	3200
Low birth weight	4265527	0.0738	0.262	0
Very low birth weight	4265527	0.0085	0.092	0
High birth weight	4265527	0.0377	0.19	0
Gestation weeks	3880121	39.1402	1.93	40
Pre-term birth	3880121	0.0696	0.254	0
Early Pre-term birth	3880121	0.0169	0.129	0
Pair	4467784	33.6676	7.209	33
Year the mother is born	4467784	1976.1664	7.201	1976
Month mother is born	4467784	6.4981	3.418	7
Day mother is born	4467784	15.6721	8.799	16
Panel B. Background variables				
Baby is a girl	4467784	0.4846	0.5	0
Multiple birth	4467784	0.0147	0.12	0
Married mother	4467784	0.6484	0.477	1
No registered dad	4467784	0.021	0.143	0
Mother employed	4467784	0.6952	0.46	1
Mother high skilled	4467784	0.2345	0.424	0
Father’s age	4373829	32.429	5.603	32
Father employed	4373829	0.866	0.341	1
Father high skilled	4373829	0.2356	0.424	0
Rural	4467784	0.315	0.465	0

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.
Notes: Sample includes deliveries occurring between 1996 and 2018.

Table 2. RD Birth and Infant Health Outcomes.1996-2018.

	(1)	(2)	(3)	(4)	(5)
Panel A. Mortality (mean: 0.0043)					
RD_Estimate	9.63e-05	0.000249	0.000223	0.000249	0.000279
	(0.000243)	(0.000283)	(0.000266)	(0.000310)	(0.000313)
Robust CI	[0 ; .001]	[0 ; .001]	[0 ; .001]	[0 ; .001]	[0 ; .001]
Bandwidth	53,9772	40,1824	55,7798	78,4876	85,5866
Panel B. Birthweight (mean: 3,183.5)					
RD_Estimate	0.873	4.472**	4.144**	6.082***	5.345**
	(1.759)	(1.967)	(1.782)	(2.337)	(2.081)
Robust CI	[-2.786 ; 5.052]	[0.303 ; 8.922]	[1.034 ; 8.949]	[1.457 ; 11.503]	[1.473 ; 10.563]
Bandwidth	38,8533	28,9237	37,3460	43,9595	53,2604
Panel C. Low Birthweight (mean: 0.0738)					
RD_Estimate	0.000504	-0.00211	-0.00125	-0.00308*	-0.00219
	(0.00115)	(0.00133)	(0.00135)	(0.00181)	(0.00172)
Robust CI	[-.002 ; .003]	[-.005 ; .001]	[-.005 ; .001]	[-.007 ; 0]	[-.006 ; .001]
Bandwidth	36,5067	27,1768	34,6492	40,3718	49,3956
Panel D. Very Low Birthweight (mean: 0.0085)					
RD_Estimate	0.00114***	0.00118***	0.00110***	0.00116***	0.00117***
	(0.000291)	(0.000308)	(0.000252)	(0.000383)	(0.000316)
Robust CI	[.001 ; .002]	[.001 ; .002]	[.001 ; .002]	[0 ; .002]	[0 ; .002]
Bandwidth	37,2448	27,7263	52,3994	51,0697	64,5278
Panel E. High Birthweight (mean: 0.0377)					
RD_Estimate	0.00107*	0.00113*	0.000883	0.00108	0.000836
	(0.000622)	(0.000635)	(0.000550)	(0.000704)	(0.000657)
Robust CI	[0 ; .002]	[0 ; .002]	[0 ; .002]	[0 ; .003]	[-.001 ; .002]
Bandwidth	47,7643	35,5574	58,9839	69,3981	77,6581
Panel F. Gestation weeks (mean: 39.1402)					
RD_Estimate	-0.0178**	-0.00599	-0.00879	-0.00583	-0.00202
	(0.00725)	(0.00811)	(0.00753)	(0.00991)	(0.0100)
Robust CI	[-.036 ; -.004]	[-.024 ; .01]	[-.026 ; .01]	[-.025 ; .018]	[-.021 ; .023]
Bandwidth	38,6893	28,8016	45,4256	52,1300	54,4822
Panel G. Pre-term birth (mean: 0.0696)					
RD_Estimate	3.59e-05	-0.000164	-0.000417	-0.000656	-0.000678
	(0.000959)	(0.00110)	(0.00106)	(0.00137)	(0.00138)
Robust CI	[-.002 ; .002]	[-.002 ; .002]	[-.003 ; .002]	[-.004 ; .002]	[-.004 ; .002]
Bandwidth	50,8144	37,8280	52,2764	58,5455	67,0893
Panel H. Early pre-term birth (mean: 0.0169)					
RD_Estimate	0.00125**	0.00106	0.000953**	0.00135**	0.00108
	(0.000575)	(0.000661)	(0.000483)	(0.000679)	(0.000688)
Robust CI	[0 ; .003]	[0 ; .002]	[0 ; .002]	[0 ; .003]	[-.001 ; .003]
Bandwidth	41,4555	30,8609	65,8963	63,8155	71,6907
Bw selection method	msecomb2	cercomb2	mserd	mserd	Mserd
Kernel	Uni	Uni	Tri	Uni	Tri
Polynomial order	1	1	1	2	2

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.
Notes: The dependent variable is indicated in each row header. Controls are birth cohort computed from July to June the following year. The bandwidth selection procedure msecomb2 computes the median bandwidth for each side of the cutoff of the msetwo (two different Mean Square Error (MSE)-optimal bandwidth selectors, below and above the cutoff), mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator) and msesum (MSE-optimal bandwidth selector for the sum of regression estimates) methods. Robust standard errors in parentheses (clustered by date of birth). Robust confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. RDD Birth and Infant Health Outcomes.1996-2018. Robustness Checks

	(1) Main results	(2) Unrestricted sample	(3) Controlling for covariates	(4) Dropping multiple births	(5) Dropping cohorts affected by the Workers Law Reform	(6) Dropping cohorts affected by the LOGSE Reform
Panel A. Mortality (mean: 0.0043)						
RD_Estimate	0.00138	0.0000967	0.0000570	0.0000979	0.0000769	0.0000680
	(0.00113)	(0.00025)	(0.000259)	(0.000247)	(0.000232)	(0.000282)
Panel B. Birthweight (mean: 3,183.5)						
RD_Estimate	0.770	0.964	2.285	1.930	1.583	1.478
	(1.808)	(1.745)	(1.662)	(1.749)	(1.781)	(2.105)
Panel C. Low Birthweight (mean: 0.0738)						
RD_Estimate	0.00207*	0.000609	0.000388	-0.000407	0.00115	0.000813
	(0.00116)	(0.00116)	(0.00114)	(0.00115)	(0.00119)	(0.00139)
Panel D. Very Low Birthweight (mean: 0.0085)						
RD_Estimate	0.00160***	0.00127***	0.00126***	0.000971***	0.00139***	0.000919***
	(0.000468)	(0.000289)	(0.000298)	(0.000321)	(0.000348)	(0.000323)
Panel E. High Birthweight (mean: 0.0377)						
RD_Estimate	0.00107*	0.000974*	0.000827	0.000911	0.00112*	0.000337
	(0.000622)	(0.000585)	(0.000609)	(0.000636)	(0.000637)	(0.000809)
Panel F. Gestation weeks (mean: 39.1402)						
RD_Estimate	-0.0220**	-0.0175**	-0.0113	-0.00763	-0.0183**	-0.00475
	(0.0105)	(0.00739)	(0.00737)	(0.00787)	(0.00815)	(0.00945)
Panel G. Pre-term birth (mean: 0.0696)						
RD_Estimate	0.000687	0.0000153	-0.000295	-0.000410	-0.000517	-0.000305
	(0.00124)	(0.000974)	(0.00103)	(0.000995)	(0.00118)	(0.00129)
Panel H. Early pre-term birth (mean: 0.0169)						
RD_Estimate	0.00170*	0.00128**	0.00139**	0.000955**	0.00153**	0.000551
	(0.000928)	(0.000587)	(0.000560)	(0.000501)	(0.000670)	(0.000667)
N. Obs.	4,467,784	4,483,942	4,483,942	4,384,210	3,882,468	3,331,784

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

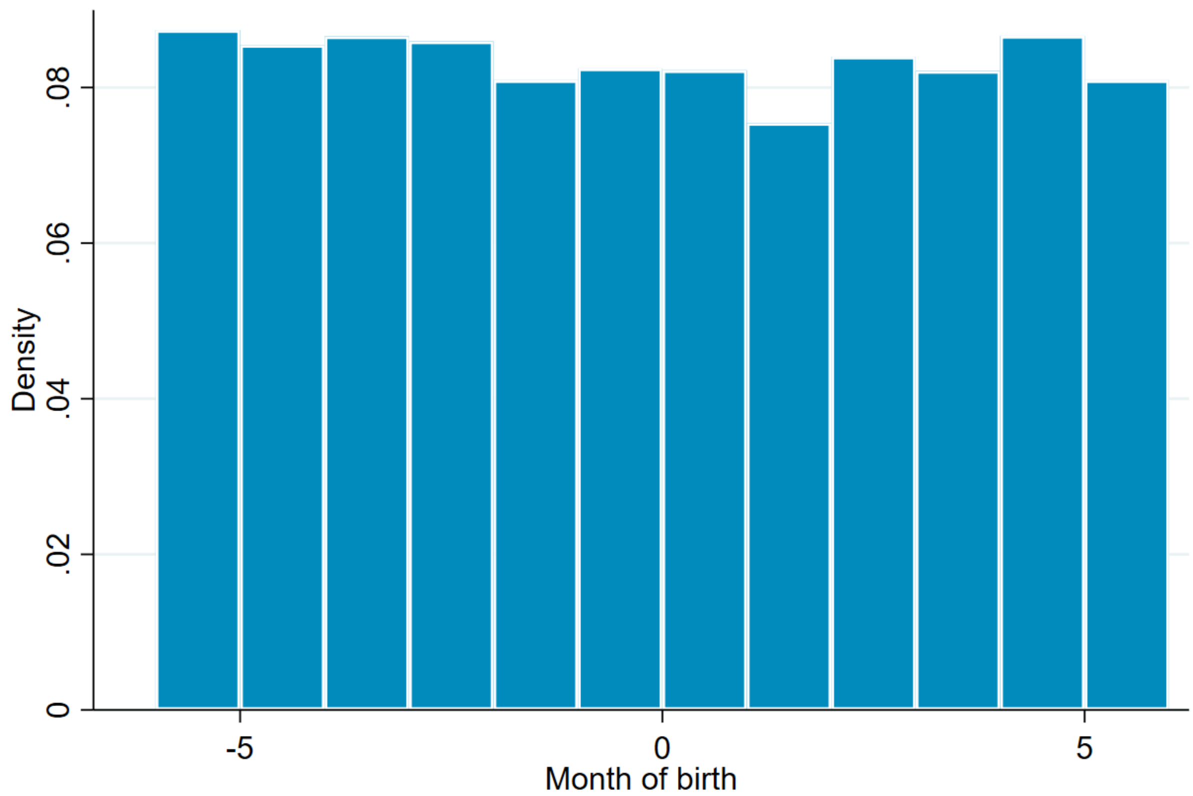
Notes: The dependent variable is indicated in each row header. Controls are birth cohort computed from July to June the following year. The coefficients were computed using a uniform kernel function, a first order polynomial, and the bandwidth selection procedure msecomb2 which computes the median bandwidth for each side of the cutoff of the msetwo (two different Mean Square Error (MSE)-optimal bandwidth selectors, below and above the cutoff), mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator) and msesum (MSE-optimal bandwidth selector for the sum of regression estimates) methods. Robust standard errors in parentheses (clustered by date of birth). *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Comparison to Cross-Section Correlations and Within-Family Estimates in our Data and Previous Birth and Infant Health Outcomes in the Literature.

VARIABLES	(1) Mortality	(2) Birth weight	(3) Low bw	(4) Very low bw	(5) Gestation weeks	(6) Pre-term birth	(7) Early Pre-term
Panel A: Our estimates from Column 1 in Table 2							
RD estimate	0.00173	0.946	0.00220*	0.00170***	-0.0266**	0.000755	0.00173*
	(0.00127)	(1.795)	(0.00116)	(0.000468)	(0.0103)	(0.00107)	(0.000944)
Mean/Sd	0.00437	3,184/510.5	0.0739	0.00855	39.14/1.93	0.0696	0.0169
Estimated percent change	39.59%	0.03%	2.98%	19.88%	-0.07%	1.08%	10.24%
Panel B: Cross-sectional maternal-age gradients in our data							
OLS estimate (87.1 days)	4.23e-07	0.309***	9.78e-05***	2.78e-05***	-0.00351***	6.09e-05***	3.59e-05***
	(1.05e-06)	(0.00871)	(4.44e-06)	(1.57e-06)	(3.50e-05)	(4.79e-06)	(2.43e-06)
Estimated percent change	0.00%	0.01%	0.13%	0.32%	-0.01%	0.88%	0.21%
Panel C: Within-family maternal-age gradients in our data							
FE estimate (87.1 days)	3.11e-05	2.702***	-0.000188	-5.44e-05	0.000263	0.000121	0.000129
	(1.91e-05)	(0.284)	(0.000158)	(5.11e-05)	(0.00123)	(0.000180)	(8.28e-05)
Estimated percent change	0.01%	0.84%	-0.00%	0.01%	0.00%	0.00%	0.01%
Panel C: Lean et al. (2017) meta-analysis							
Over 35 indicator (odds-ratios)	1.75***		1.37***	1.59**		1.45***	
	(0.07)		(0.06)	(0.48)		(0.04)	
Estimated percent change	75.00%		37.00%	59.00%		45.00%	
Panel D: Goisis et al. (2017) within family model							
Over 35 indicator			-0.20			0.20	
			0.40			0.38	
Estimated percent change			-9.09%			5.40%	
Panel F: Fredricksson et al (2021) school cutoff model							
RD estimate		-21.015***			-0.515***		
		(7.683)			(0.179)		
Estimated percent change		-0.60%			-0.19%		

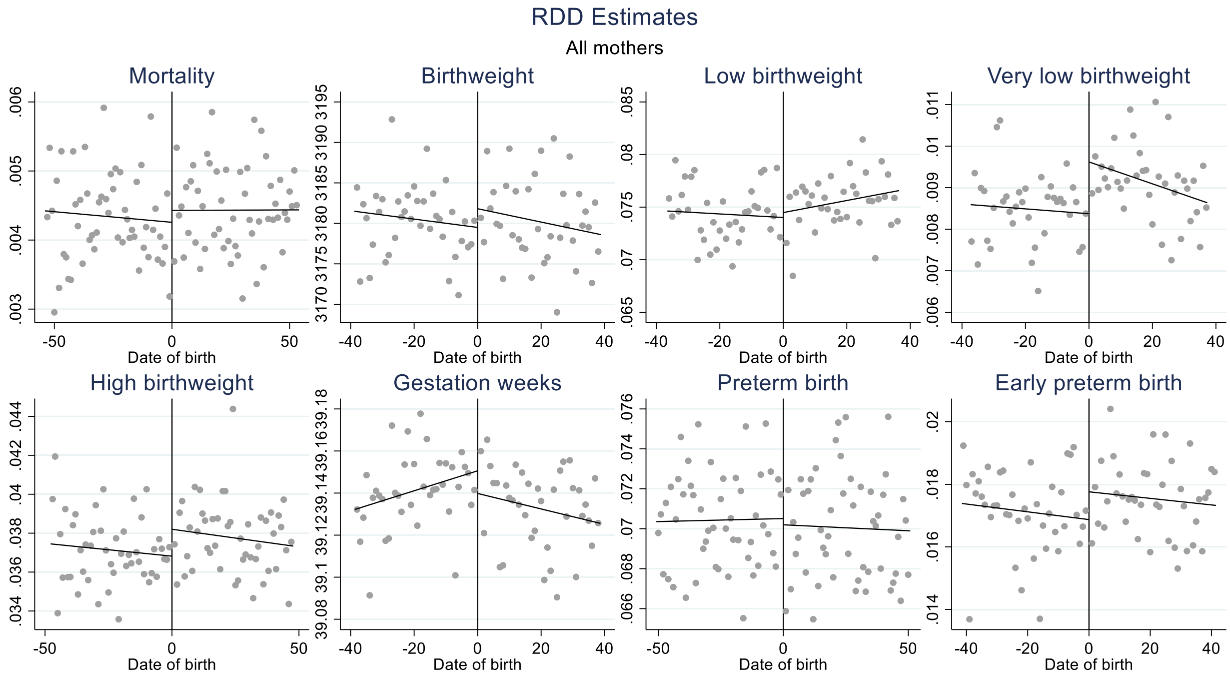
Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.
Notes: The dependent variable is indicated in each column header. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Distribution of Months of Birth Dates of Potential Mothers



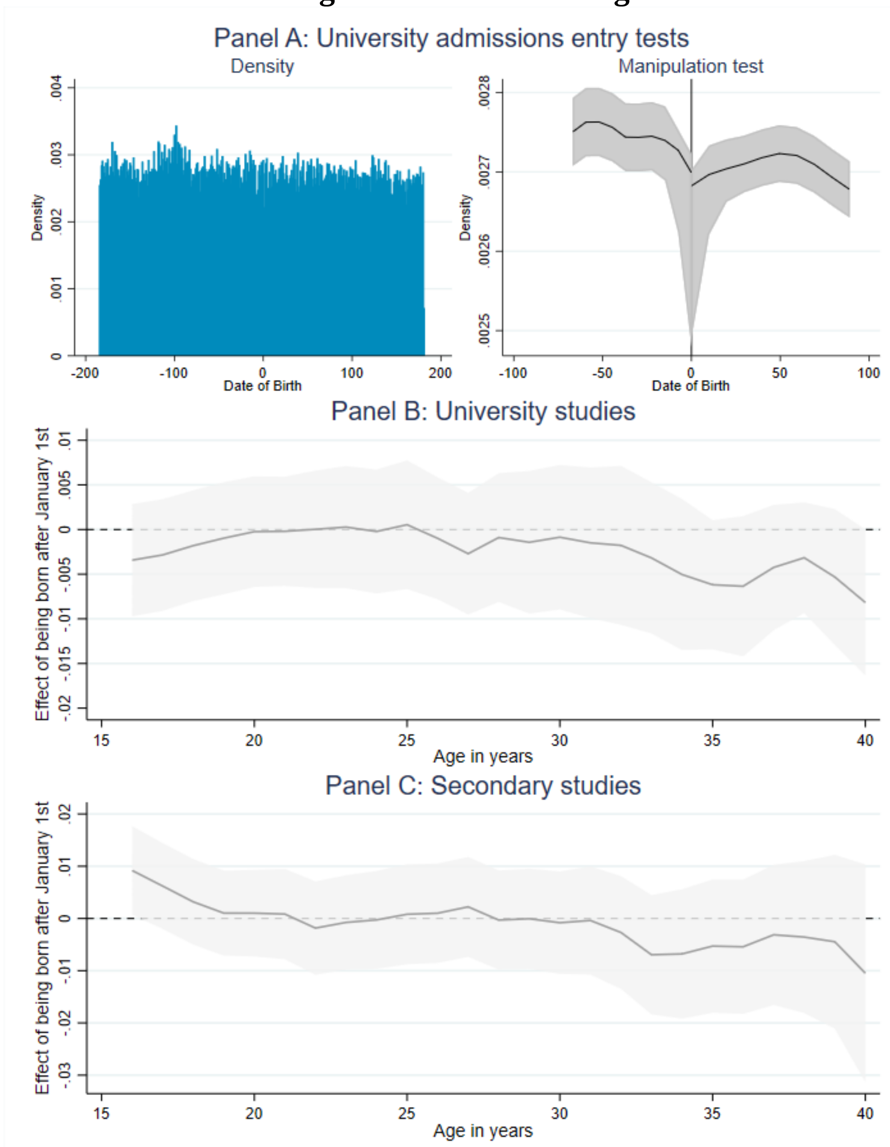
Source: Spanish Vital Statistics. Spanish National Statistical Institute. 1980-1995

Figure 2. Impact of Being Born Early in a Cohort on Offspring Health at Birth.



Notes: Each figure plots the mean outcome variable birth by day of birth, re-scaled from January 1st each year, together with a first order polynomial regression line fitted separately on each side of the cutoff.
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018

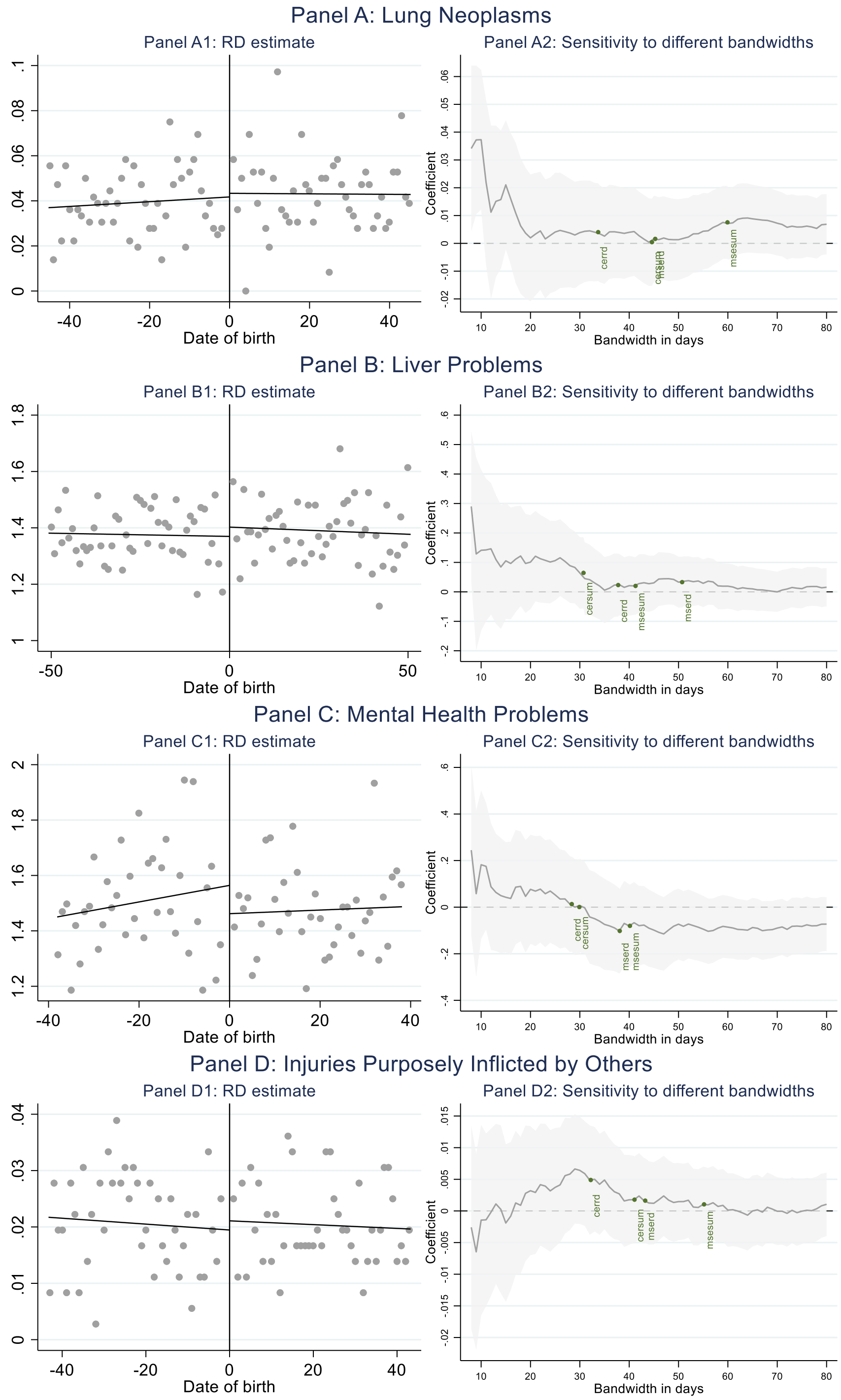
Figure 3. Distribution of birthdates around the cutoff for university entry test takers and impact of being born after the cutoff on the probability of different highest qualifications along the distribution of age



Notes: Panel A plots the distribution of birthdates around the school entry cutoff of female university admission test takers and the corresponding manipulation test. Panels B and C plot the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the on the probability of having university studies (Panel B) or secondary studies (Panel C) as highest qualification for the sample of women older than the specified age in the horizontal axis. The complete sample includes women aged 16 to 50 years old. Each coefficient comes from a different regression. Controls are birth cohorts computed from July to June the following year for July to June pairs from 1949-50 to 1994-95. The window around the cutoff is one month.

Source: 2003-2016 Andalusian University Admissions Data (Panel A) and 2004-2015 LFS microdata, Spanish National Statistical Institute (Panels B and C).

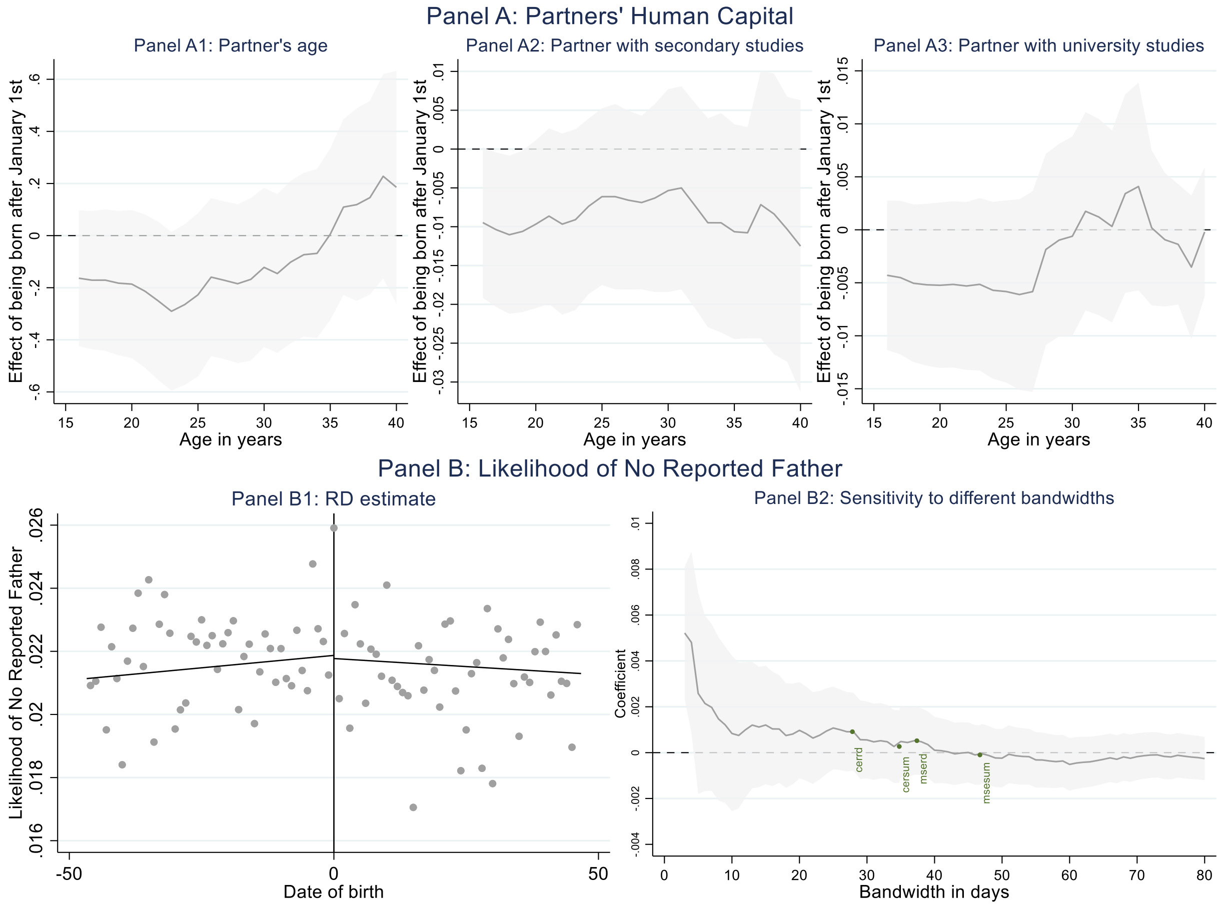
Figure 4. Potential Mothers' Hospitalization Outcomes.



Notes: Panel A.1, B.1, C.1, and D1 plot mean annual hospitalization rates by day of birth, together with a first order polynomial regression line fitted separately on each side of the cutoff. Panels A.2, B.2, C.2, and D.2 plot RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-

optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects. Note that the confidence intervals are not very reliable for very low bandwidths, given the low number of clusters.
Source: Spanish Hospital Discharge Records, Ministry of Healthcare, 2004-2015

Figure 5. Impact of being born after the cutoff on partners' characteristics and the likelihood of reporting a father at motherhood

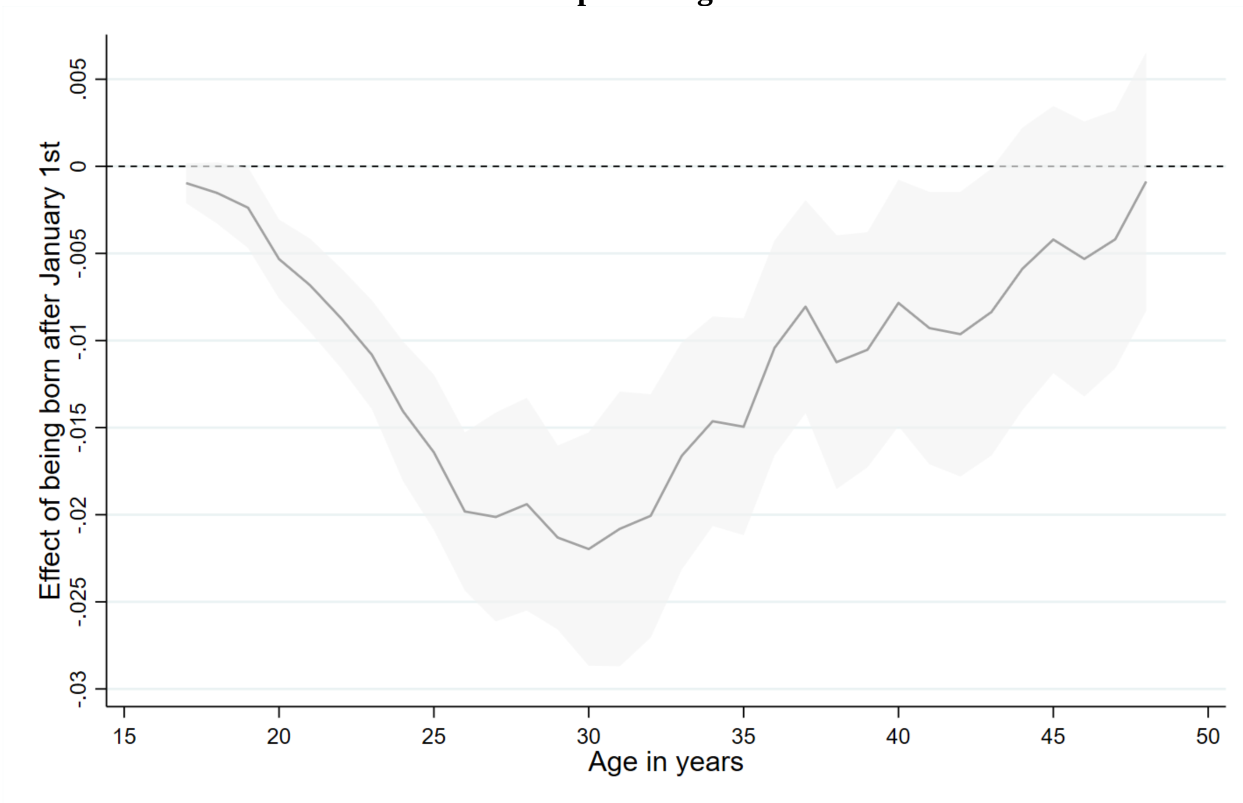


Notes: Panel A in this figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the on the age of their partner (Panel A1) and the probability that their partner has secondary studies (Panel A2) or university studies (Panel A3) as highest qualification for the sample of women older than the specified age in the horizontal axis. The complete sample includes women aged 16 to 50 years old. Each coefficient comes from a different regression. Controls are birth cohorts computed from July to June the following year for July to June pairs from 1949-50 to 1994-95. The window around the cutoff is one month.

Panel B in this figure studies the impact of being a January-born mother on the probability that the child has no reported father. Panel B1 plots the likelihood of having no dad by maternal day of birth, together with a first order polynomial regression line fitted separately on each side of the cutoff. Panel B2 plots RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects.

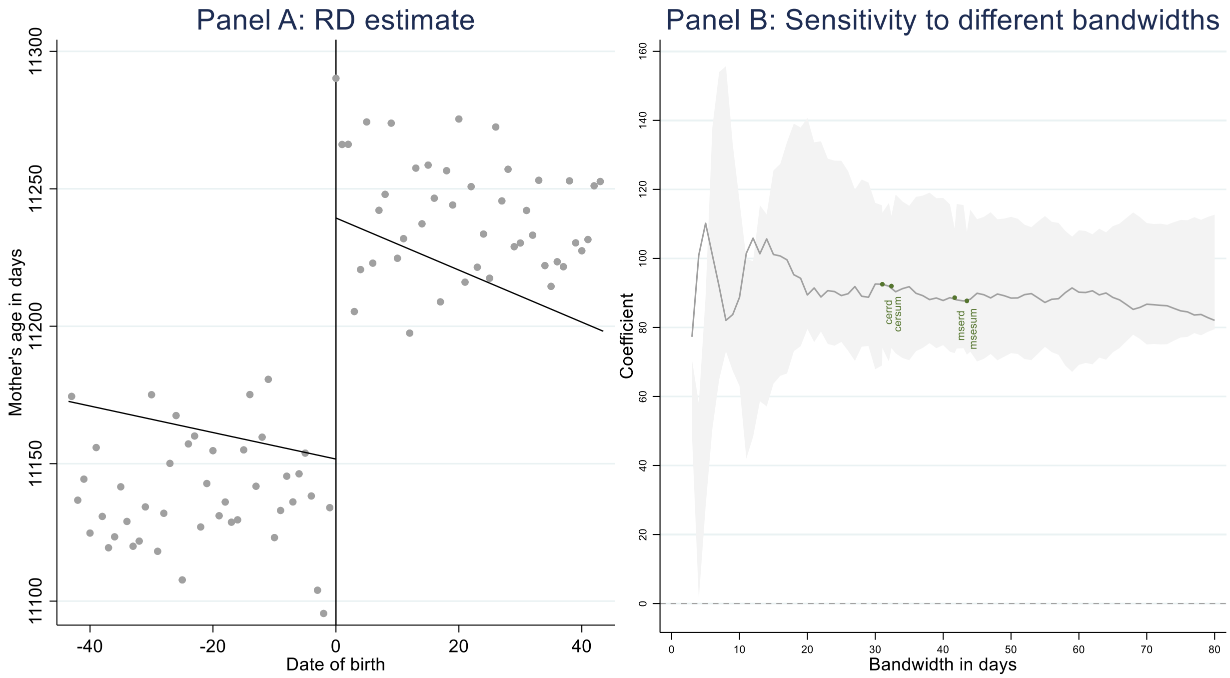
Source: LFS microdata, Spanish National Statistical Institute, 2000-2018 (Panel A) and Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018 (Panel B).

Figure 6. Impact of Being Born Early in a Cohort on the Probability of Becoming Mother for the First Time before a Specific Age



Data source: LFS microdata, Spanish National Statistical Institute, 2000-2018.
Notes: This figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the probability to give birth before specific ages (age plotted on the horizontal axis). Each coefficient comes from a different regression. Controls are birth cohorts computed from July to June the following year for July to June pairs from 1942-43 to 1994-95. The window around the cutoff is one month.

Figure 7. Impact of Being Born Early in a Cohort on Maternal Age at First Birth.



Notes: Panel A plots mean age at first birth by day of birth, together with a first order polynomial regression line fitted separately on each side of the cutoff. Panel B plots RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects. Note that the confidence intervals are not very reliable for very low bandwidths, given the low number of clusters.

Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018

Table A.1. Descriptive Statistics for Supplemental Datasets

	Obs.	Average	St. dev.	Mean
Panel A. Vital Statistics 1980-1995. Potential Mothers' Health and Family Characteristics at Birth				
Treat	518813	0.499	0.500	0
Mortality	518813	0.007	0.085	0
First birth	515073	0.476	0.499	0
Twin	515073	0.009	0.095	0
Birthweight	425264	3248.8	480.26	3250
Premature birth	515073	0.038	0.191	0
Mother's age at birth	518813	339.65	65.61	336
Married mother	515073	0.919	0.272	1
No registered dad	515073	0.020	0.139	0
Employed mother	515073	0.321	0.467	0
High-skilled mother	515073	0.104	0.305	0
Panel B. University Admissions Data 2000-2016. Women Sitting the Test				
Treat	160022	0.485	0.500	0
Test-year	160022	2008.9	2.912	2009
Ordinary call	160022	0.820	0.384	1
Repeater	154446	0.222	0.415	0
Advanced student	154446	0.004	0.059	0
Passed 2000-09	54124	0.956	0.206	1
Grade 2000-09 (if passed)	55362	6.249	1.738	6.26
Passed in ordinary call 2000-09	54124	0.777	0.416	1
Grade in ordinary call 2000-09	55362	5.232	2.985	6.19
Grade 2010-19	36330	6.168	1.601	6.20
Grade in ordinary call 2010-19	36330	5.464	2.653	6.11
Panel C. Hospital Records Sample 2004-2015. Women Aged 16-44				
Treat	133920	0.4872	0.5	0
Lung Cancer Hospitalizations	133920	0.0393	0.263	0
Liver Problems Hospitalizations	133920	1.4004	1.648	1
Mental Problems Hospitalizations	133920	1.5139	1.905	1
Aggressions Hospitalizations	133920	0.0209	0.15	0
Abortions Hospitalizations	133920	1.5596	1.752	1
Panel D. LFS Sample 2000-2018. Women Aged 16-44				
Treat	550929	0.5007	0.5	1
Primary or Less	176088	0.0718	0.258	0
Secondary	176088	0.5439	0.498	1
University	176088	0.3843	0.486	0
Has a Child	550929	0.4679	0.499	0
Age at First Birth (months)	257762	327.1742	61.986	329
Married	550929	0.4412	0.497	0
Partnered	550929	0.5082	0.5	1
Age of Partner (years)	176088	39.403	6.633	40
Partner Primary or Less	176088	0.0978	0.297	0
Partner Secondary	176088	0.5851	0.493	1
Partner University	176088	0.3171	0.465	0

Data sources: Vital Statistics, 1980-1995; Andalusian University Admissions, 2003-2016, Spanish Hospital Discharge Registry, 2004-2015; and LFS microdata, Spanish National Statistical Institute, 2000-2018.

Table A.2. Potential Mothers' Health and Family Characteristics at Birth. Local Randomization Impact of Being Born After the Cutoff. 1980-1995

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Potential Mothers' Health at Birth						
	Mortality	First Birth	Weight	Low Birth Weight	Very Low Birth Weight	Premature Birth
	0.000163	-0.000728	-2.589*	0.00110*	-0.000275	0.000346
	(0.000235)	(0.00139)	(1.468)	(0.000653)	(0.000178)	(0.000533)
Observations	518,813	515,073	425,264	425,264	425,264	515,073
Mean/Std. dev.	0.0072/ 0.085	0.476/ 0.499	3248.7967/ 480.258	0.0475/0.213	0.0034/0.058	0.0381/ 0.191
Panel B: Potential Mothers' Family Background						
	Mother's age	Married	No father	Employed mother	High-skill Mother	High-skill Father
	0.0916	-0.00166**	0.000171	-0.000438	0.000860	-0.000427
	(0.182)	(0.000756)	(0.000387)	(0.00128)	(0.000845)	(0.00102)
Observations	515,073	515,073	515,073	515,073	515,073	515,073
Mean/Std.dev.	339.7/65.611	0.9193/0.272	0.0196/0.139	0.3212/ 0.467	0.1038/ 0.305	
Controls	Pair	Pair	Pair	Pair	Pair	Pair
Bandwidth	1month	1month	1month	1month	1month	1month

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1980-1995.

Notes: The coefficients reported are for the binary indicator taking value 1 for January to June. Each coefficient is from a different regression. The dependent variable is indicated in each row header. The sample includes all births in December one year and January the following year for pairs from 1980-81 to 1994-95. Robust standard errors are shown in parentheses.

Table A.3. Power Calculations. Vital Statistics. 1996-2018

	(1)	(2)	(3)	(4)	(5)
Panel A. Mortality (tau=0.00022)					
Power	0.117	0.106	0.105	0.094	0.093
Panel B. Birthweight (tau=159.14)					
Power	1.000	1.000	1.000	1.000	1.000
Panel C. Low Birthweight (tau=0.0037)					
Power	0.793	0.67	0.676	0.492	0.501
Panel D. Very Low Birthweight (tau= 0.00044)					
Power	0.244	0.254	0.305	0.168	0.222
Panel E. High Birthweight (tau= 0.00044)					
Power	0.745	0.797	0.829	0.666	0.73
Panel F. Gestation weeks (tau=1.957)					
Power	1.000	1.000	1.000	1.000	1.000
Panel G. Pre-term birth (tau=0 .0035)					
Power	0.870	0.830	0.78	0.619	0.602
Panel H. Early pre-term birth (tau= 0.00086)					
Power	0.241	0.221	0.320	0.197	0.188
Bw selection method	msecomb2	cercomb2	mserd	mserd	mserd
Kernel	Uni	Uni	Tri	Uni	Tri
Polynomial order	1	1	1	2	2

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.
Notes: Table presents the estimated statistical power of the robust bias-corrected inference methods implemented in Table 2 for hypothesized RD treatment effects (tau) of 5% of the corresponding dependent variable mean. The sample includes all first mothers born in December and January of the following year.

Table A.4. Potential Mothers' University Admissions Outcomes

VARIABLES	(1) Specific. 1	(2) Specific. 2	(3) Specific. 3	(4) Specific. 4	(5) Specific. 5
Panel A					
Repeater (mean 0.222)	-0.0801***	-0.0764***	-0.0795***	-0.0781***	-0.0777***
	(0.00727)	(0.00849)	(0.00652)	(0.00862)	(0.00818)
Robust 95% CI	[-.075 ; -.043]	[-.077 ; -.043]	[-.074 ; -.045]	[-.072 ; -.034]	[-.073 ; -.037]
Obs.	193,100	193,100	193,100	193,100	193,100
Panel B					
Advanced (mean 0.040)	0.00420***	0.00463***	0.00408***	0.00481***	0.00479***
	(0.00105)	(0.00121)	(0.000981)	(0.00128)	(0.00133)
Robust 95% CI	[.002 ; .008]	[.002 ; .009]	[.002 ; .008]	[.003 ; .01]	[.003 ; .01]
Obs.	193,100	193,100	193,100	193,100	193,100
Panel C					
Passed 2000-09 (mean 0.956)	0.0104	0.0154	0.0138	0.0155	0.0169
	(0.00869)	(0.0105)	(0.00858)	(0.0121)	(0.0115)
Robust 95% CI	[-.008 ; .032]	[-.006 ; .038]	[-.005 ; .036]	[-.009 ; .045]	[-.008 ; .044]
Obs.	54,124	54,124	54,124	54,124	54,124
Panel D					
Std score among passes 2000-09	0.0596	0.0465	0.0548	0.0248	0.0387
	(0.0380)	(0.0439)	(0.0414)	(0.0463)	(0.0459)
Robust 95% CI	[-.033 ; .135]	[-.049 ; .133]	[-.05 ; .135]	[-.093 ; .111]	[-.068 ; .132]
Obs.	51,732	51,732	51,732	51,732	51,732
Panel G					
Std score 2010-16	0.0461	0.0540	0.0459	0.0386	0.0501
	(0.0436)	(0.0490)	(0.0441)	(0.0542)	(0.0510)
Robust 95% CI	[-.041 ; .158]	[-.043 ; .165]	[-.054 ; .15]	[-.089 ; .153]	[-.063 ; .162]
Obs.	36,330	36,330	36,330	36,330	36,330
Panel H					
Std score in ordinary call 2010-16	0.0417	0.00616	0.0264	0.0271	0.0249
	(0.0412)	(0.0477)	(0.0431)	(0.0513)	(0.0493)
Robust 95% CI	[-.059 ; .129]	[-.1 ; .105]	[-.079 ; .121]	[-.083 ; .141]	[-.087 ; .131]
Obs.	31,111	31,111	31,111	31,111	31,111
Bw selection method	msecomb2	cercomb2	mserd	Mserd	Mserd
Kernel	Uni	Uni	Tri	Uni	Tri
Polynomial order	1	1	1	2	2

Data source: Andalusian University Admissions Data. 2003-2019.

Notes: The coefficients reported are for the binary indicator taking value 1 for women born after the school cutoff of January 1st. Each coefficient comes from a different regression. The outcome of interest is indicated in each row header. The sample includes all women taking part in University Admission tests with their school cohort and up to two years behind, and one year in advance. Controls are birth cohort computed from July to June the following year and dummies for changes in the examination system in 2010 and 2017. The bandwidth selection procedure msecomb2 computes the median bandwidth for each side of the cutoff of the, msetwo (two different Mean Square Error (MSE)-optimal bandwidth selectors, below and above the cutoff), mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator) and msesum (MSE-optimal bandwidth selector for the sum of regression estimates) methods. Robust standard errors in parentheses (clustered by date of birth).

Table A.5. Impact of Being Born After the Cutoff on Selection into Motherhood and Fertility. LFS Sample 2000-2018.

	(1) Age 18	(3) Age20	(5) Age25	(7) Age 30	(9) Age 35	(11) Age 40	(13) Age 45
Panel A. Dep var: First Child Before Specific Ages							
Treat	-0.0012	-0.0046***	-0.018***	-0.019***	-0.012***	-0.0056*	0.00067
	(0.00073)	(0.00100)	(0.0022)	(0.0028)	(0.0028)	(0.0030)	(0.0032)
Dep var Mean/S.d.	0.590 / 0.492	0.619 / 0.486	0.697 / 0.460	0.761 / 0.427	0.789 / 0.408	0.788 / 0.409	0.763 / 0.425
Panel B. Dep. var: Number of Children When Observed After Specific Ages							
Treat	-0.020***	-0.022***	-0.023***	-0.019***	-0.011*	-0.0038	0.0063
	(0.0048)	(0.0052)	(0.0057)	(0.0061)	(0.0067)	(0.0078)	(0.0100)
Dep var Mean/S.d.	1.033 / 1.030	1.084 / 1.029	1.226 / 1.016	1.353 / 0.995	1.422 / 0.990	1.414 / 0.995	1.339 / 1.007
Panel C. Dep. Var: Partnership							
Treat	-0.0020	-0.0032	-0.0019	0.0020	0.0064*	0.0048	0.0058
	(0.0032)	(0.0034)	(0.0033)	(0.0032)	(0.0035)	(0.0038)	(0.0039)
Dep var Mean/S.d.	0.626 / 0.484	0.657 / 0.476	0.733 / 0.442	0.777 / 0.416	0.791 / 0.407	0.792 / 0.406	0.790 / 0.407
Panel D. Dep var: Marriage							
Treat	-0.0067**	-0.0076**	-0.0058	-0.00049	0.0059	0.0039	0.0046
	(0.0032)	(0.0034)	(0.0035)	(0.0033)	(0.0036)	(0.0039)	(0.0042)
Dep var Mean/S.d.	0.569 / 0.495	0.598 / 0.490	0.675 / 0.469	0.730 / 0.444	0.755 / 0.430	0.766 / 0.423	0.772 / 0.419
Obs.	120,237	114,298	99,773	86,022	70,519	52,721	33,687
Controls	Pair	Pair	Pair	Pair	Pair	Pair	Pair
Bandwidth	1month	1month	1month	1month	1month	1month	1month

Data source: LFS microdata, Spanish National Statistical Institute, 2000-2018.
Notes: The coefficients reported are for the binary indicator taking value 1 for women born after the school cutoff of January 1st. Each coefficient comes from a different regression. In Panel A the dependent variable is having the first child before the age specified in each column header. In Panel B the dependent variable is the number of children. Each column samples women cohorts aged at least the number of years indicated in the column header. Controls are birth cohort computed from July to June the following year for July to June pairs from 1942-43 to 1994-95. The window around the cutoff is one month as indicated in the bandwidth row. Sample includes all women born in the last 12 to 7 months of the year and the first 1 to 6 months of the following year (depending on the column). Robust standard errors are shown in parentheses.

Table A.6. School Starting Age and Maternal Age at First Birth. RD estimates.1996-2018.

	(1)	(2)	(3)	(4)	(5)
Panel A. Sensitivity to different functional forms and bandwidth selection methods					
RD_Estimate	88.45***	91.22***	90.00***	92.66***	92.30***
	(7.759)	(8.923)	(7.856)	(8.957)	(8.791)
Robust CI	[72.637 ; 108.228]	[72.993 ; 111.697]	[73.289 ; 109.369]	[74.357 ; 114.117]	[73.721 ; 112.127]
Bw selection method	msecomb2	cercomb2	mserd	mserd	Mserd
Kernel	Uni	Uni	Tri	Uni	Tri
Polynomial order	1	1	1	2	2
N. Obs.	4,467,784	4,467,784	4,467,784	4,467,784	4,467,784
Panel B. Sensitivity to different selected samples					
	Unrestricted sample	Controlling for covariates	Dropping multiple births	Dropping cohorts affected by the Workers Law Reform	Dropping cohorts affected by the LOGSE Reform
RD_Estimate	91.01***	83.41***	89.76***	89.83***	86.61***
	(7.883)	(7.463)	(7.940)	(8.242)	(8.010)
Bw selection method	msecomb2	msecomb2	msecomb2	msecomb2	msecomb2
Kernel	Uni	Uni	Uni	Uni	Uni
Polynomial order	1	1	1	1	1
N. Obs.	4,483,942	4,483,942	4,384,210	3,882,468	3,331,784

Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.
Notes: Each cell comes from a different RD regression. The dependent variable is Maternal age in days (mean: 11,194.2). In Panel A, controls are birth cohort computed from July to June following year. The bandwidth selection procedure msecomb2 computes the median bandwidth for each side of the cutoff of the msetwo (two different Mean Square Error (MSE)-optimal bandwidth selectors, below and above the cutoff), mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator) and msesum (MSE-optimal bandwidth selector for the sum of regression estimates) methods. Robust standard errors in parentheses (clustered by date of birth). Robust confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1.

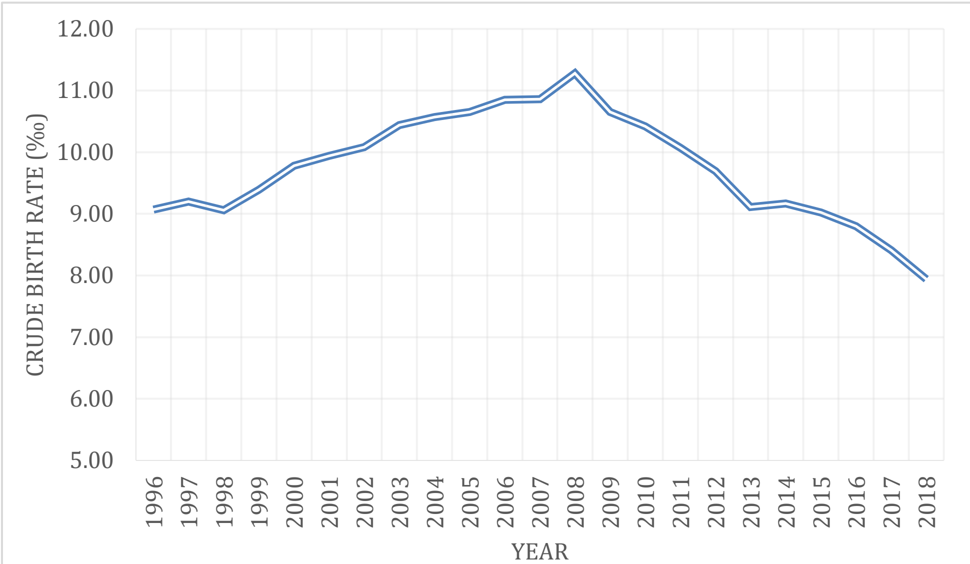
Table A.7. Descriptive Statistics for Comparison Datasets

	Obs.	Average	Stdev.	Mean
Panel A. Vital Statistics Data. OLS Sample				
Maternal age in 87.21 days bins	8289016	133.7318	21.613	135.1336
Mortality	8289016	0.0037	0.061	0
Birthweight	7945605	3218.4137	514.264	3240
Low birth weight	7945605	0.0683	0.252	0
Very low birth weight	7945605	0.0076	0.087	0
Gestation weeks	7175052	39.0695	1.891	39
Pre-term birth	7175052	0.0701	0.255	0
Early Pre-term birth	7175052	0.0161	0.126	0
Panel B. Vital Statistics Data. Within-Family Sample				
Maternal age in 87.21 days bins	3254748	133.6214	20.048	134.7552
Mortality	3254748	0.0027	0.051	0
Birthweight	3132558	3244.6598	495.623	3250
Low birth weight	3132558	0.0571	0.232	0
Very low birth weight	3132558	0.0057	0.075	0
Gestation weeks	2830912	39.1341	1.779	39
Pre-term birth	2830912	0.0605	0.238	0
Early Pre-term birth	2830912	0.0124	0.111	0

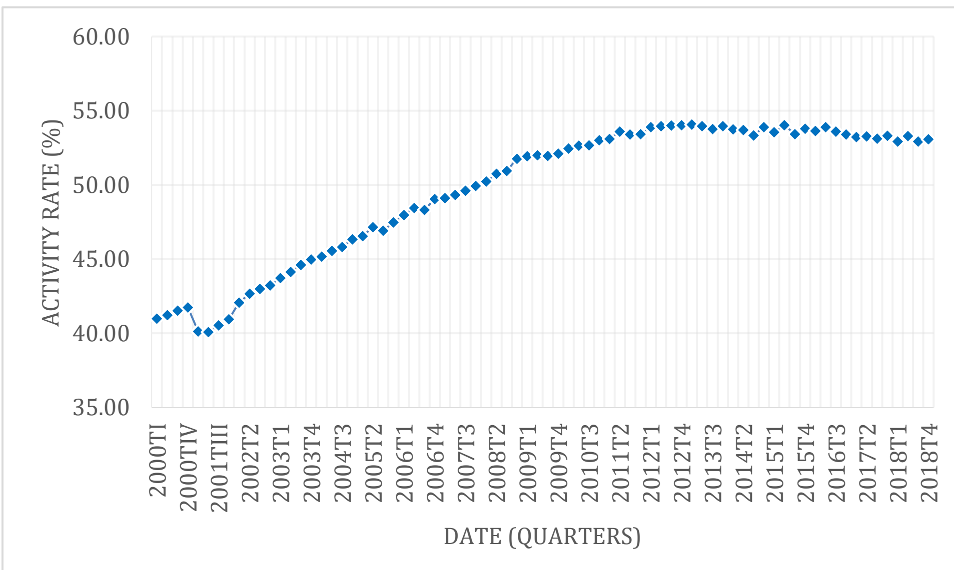
Data source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

Figure A.1. Recent Trends in Spain

Panel A Crude birth rate

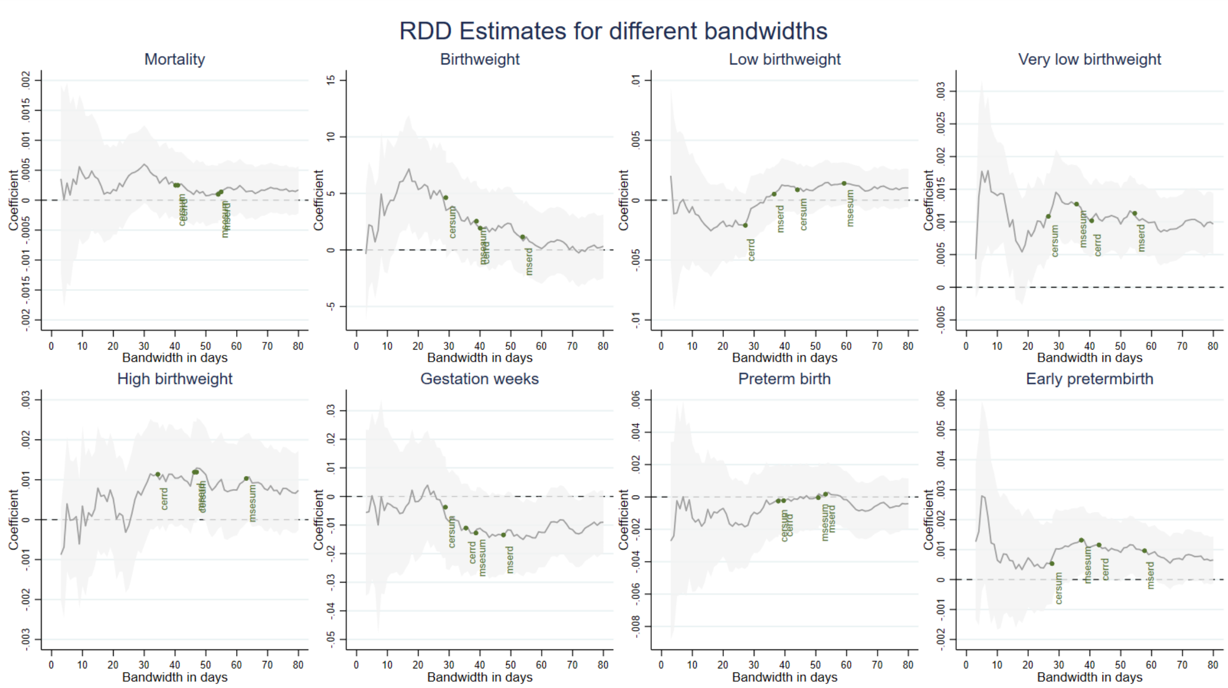


Panel B: Female activity rate



Source: Panel A, Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018. Panel B, LFS data, Spanish National Statistical Institute, 2000-2018.

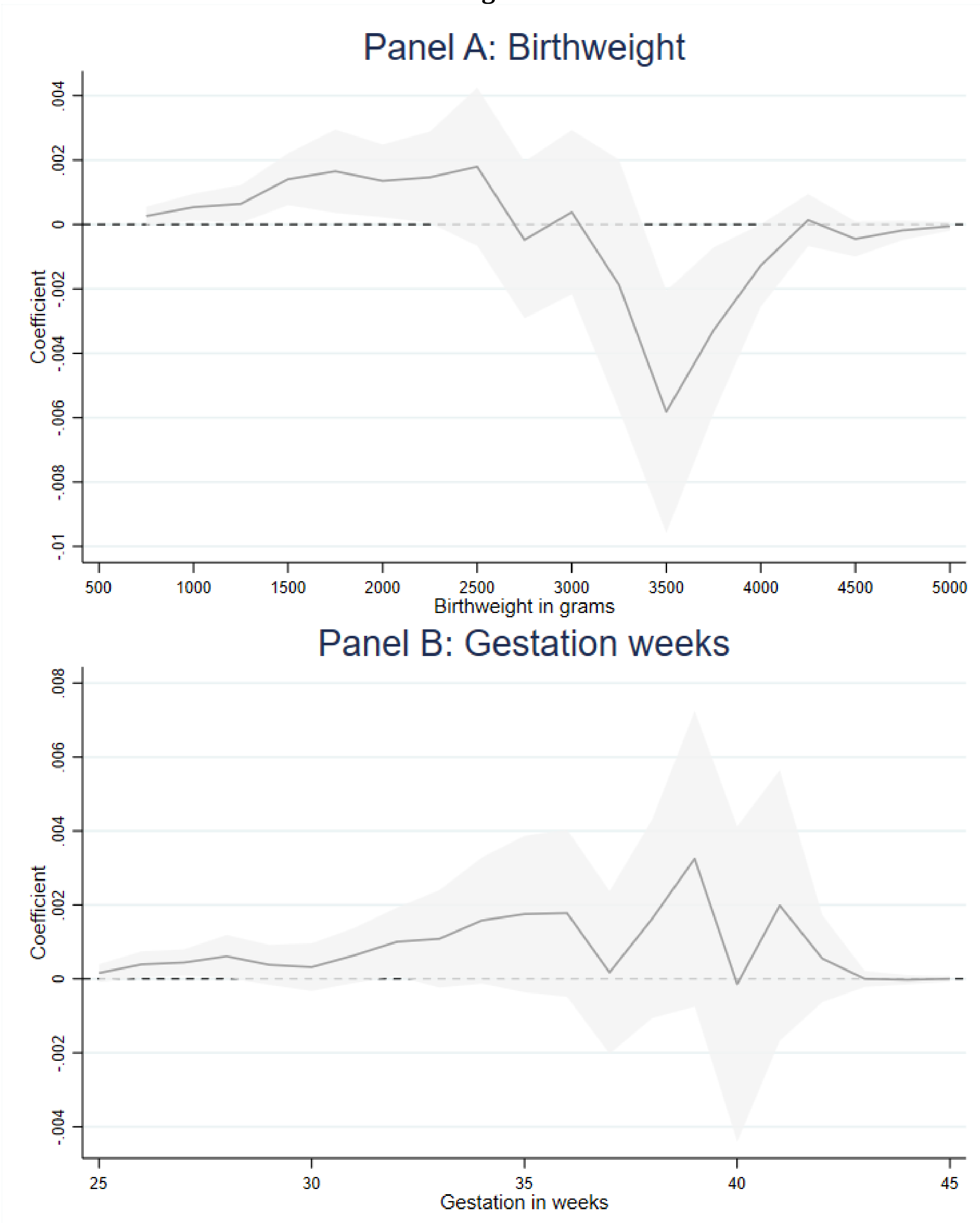
Figure A.2. Impacts of Being Born Early in a Cohort on Children's Health Outcomes. Different Bandwidths



Notes: RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects. Note that the confidence intervals are not very reliable for very low bandwidths, given the low number of clusters.

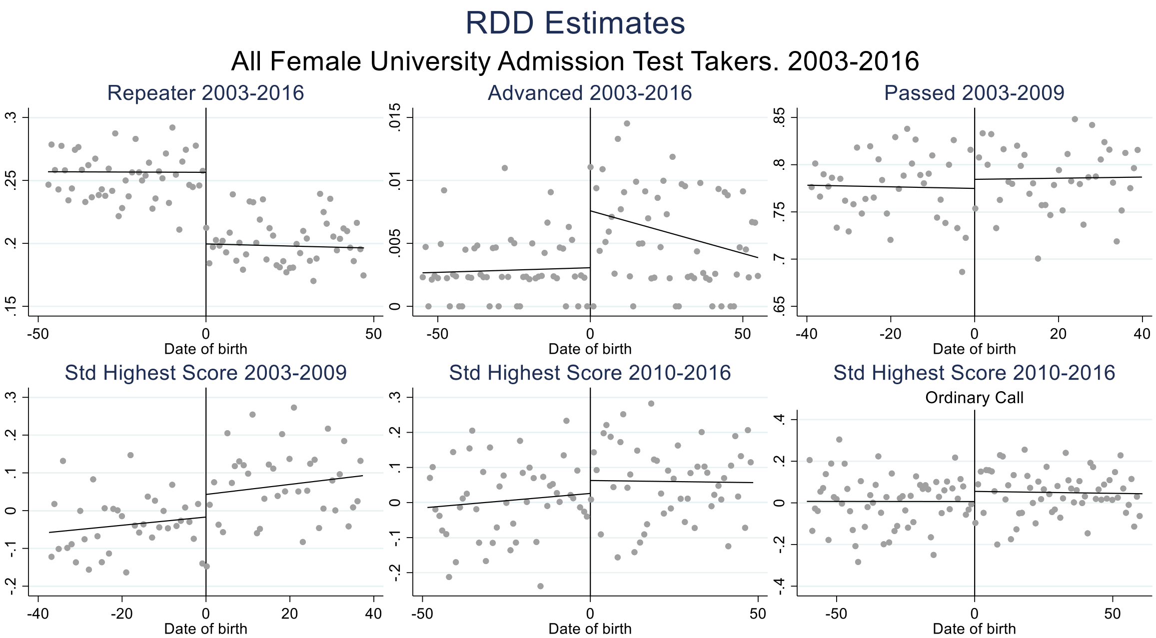
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

Figure A.3. Impact of being born after the cutoff along the distribution of birthweight and gestation weeks



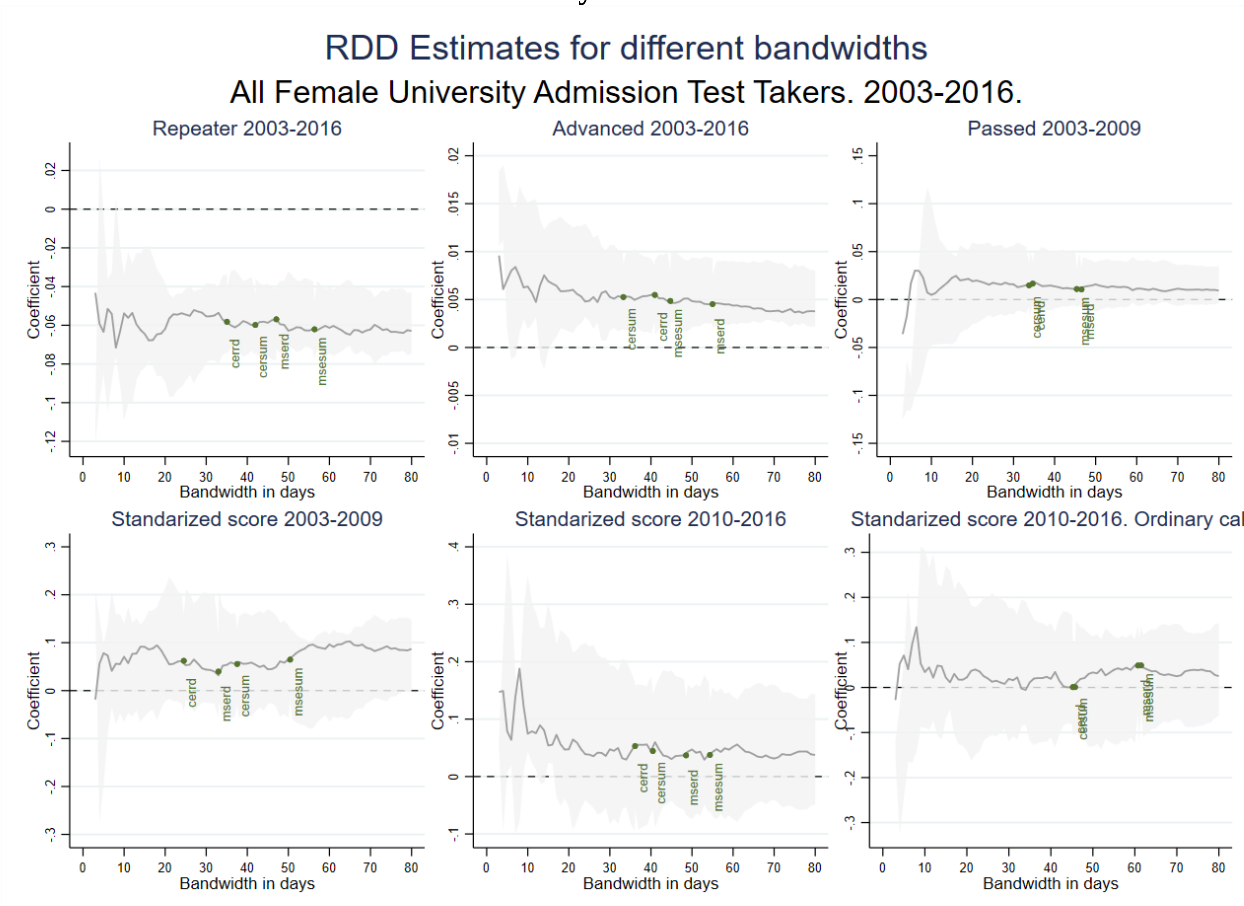
Notes: The figures plot the RD estimates from different regressions that estimate the probability to have birth weight of at most the amount on the x axis (Panel A) or gestation weeks of at most the amount on the x axis (Panel B). All coefficients were computed using a uniform kernel function, a first order polynomial, MSE-optimal bandwidths, and cohort fixed effects. Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

Figure A.4. Potential Mothers' University Admissions Test Results. Testing for Long-Term Human Capital Impacts of Being Older in a Cohort. All Female Test-Takers. Ordinary and Extraordinary Calls unless Stated



Source: Andalusian University Admissions Data. 2003-2016.
Notes: Each graph plots the conditional mean of the outcome of interest (as indicated in each figure title) by day of birth, together with a first order polynomial regression line fitted separately on each side of the cutoff. The sample includes all women taking part in University Admission tests with their school cohort and up to two years behind, and one year in advance.

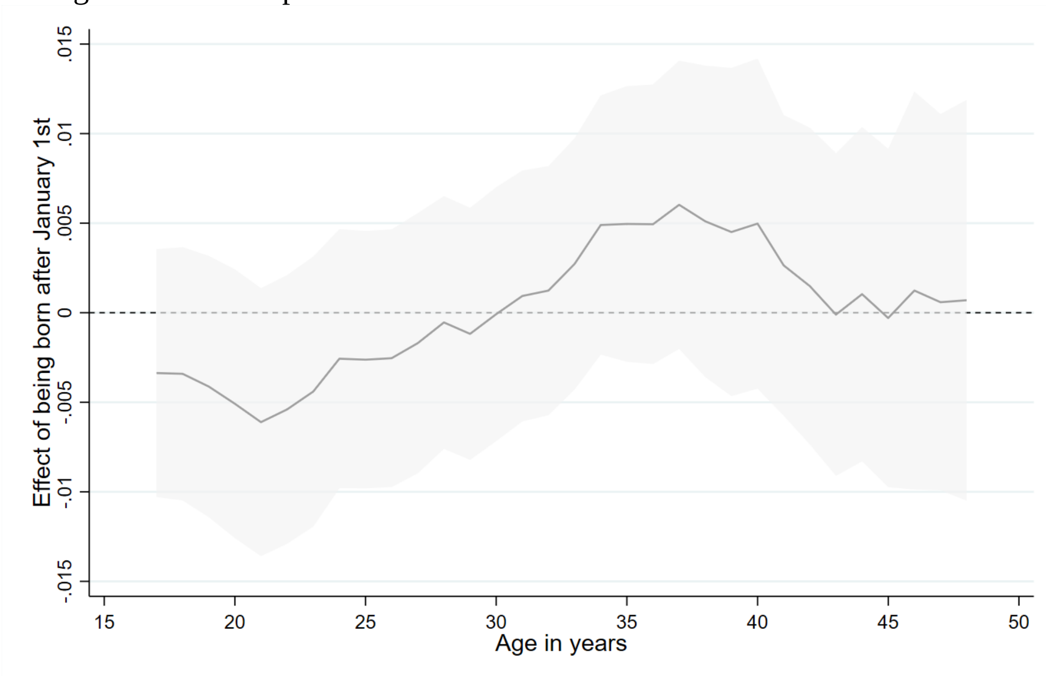
Figure A.5. Potential Mothers' University Admissions Test Results. Testing for Long-Term Human Capital Impacts of Being Older in a Cohort. All Female Test-Takers. Ordinary and Extraordinary Calls unless Stated



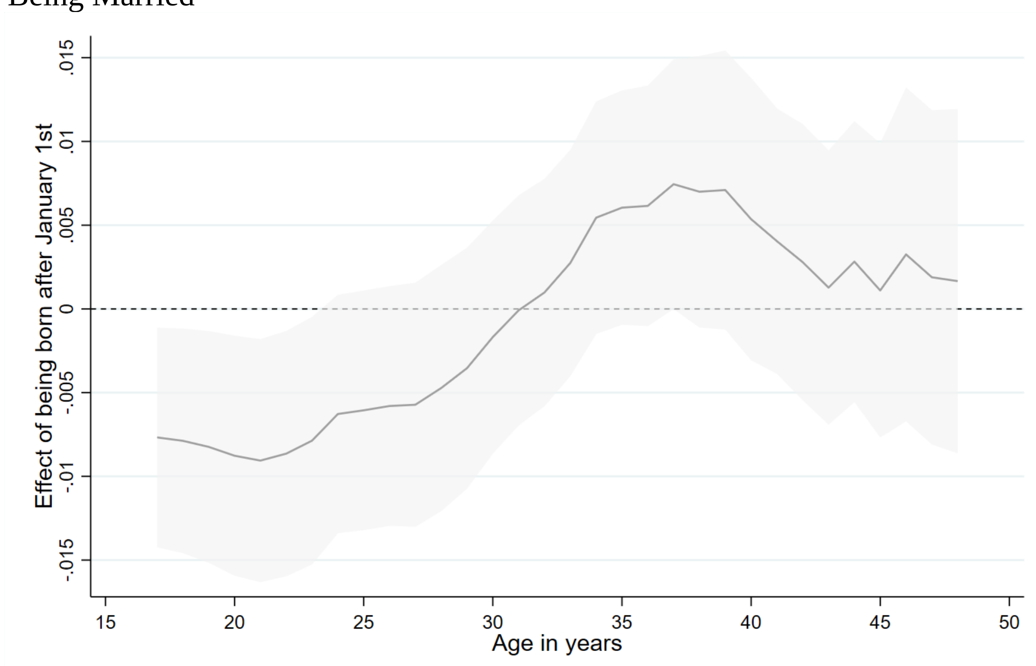
Notes: RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects.
Source: Andalusian University Admissions Data. 2003-2016.

Figure A.6. Impact of Being Born Early in a Cohort on the Probability of Different Marital Statuses by Age

Panel A Being in a Partnership

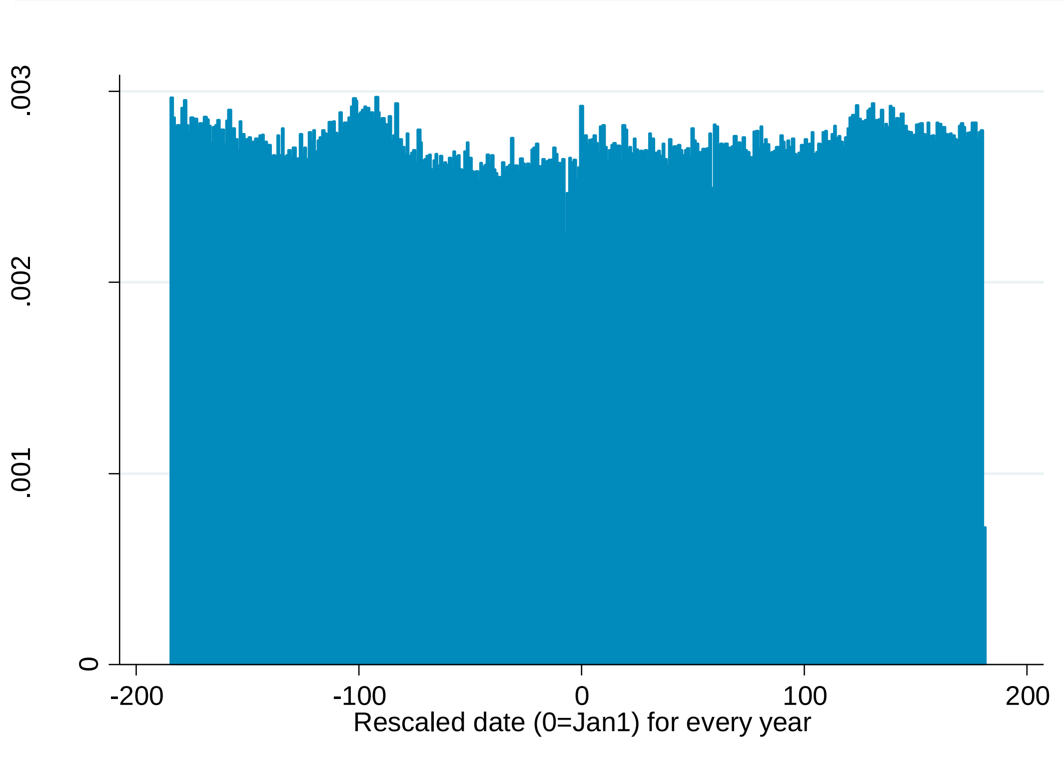


Panel B Being Married



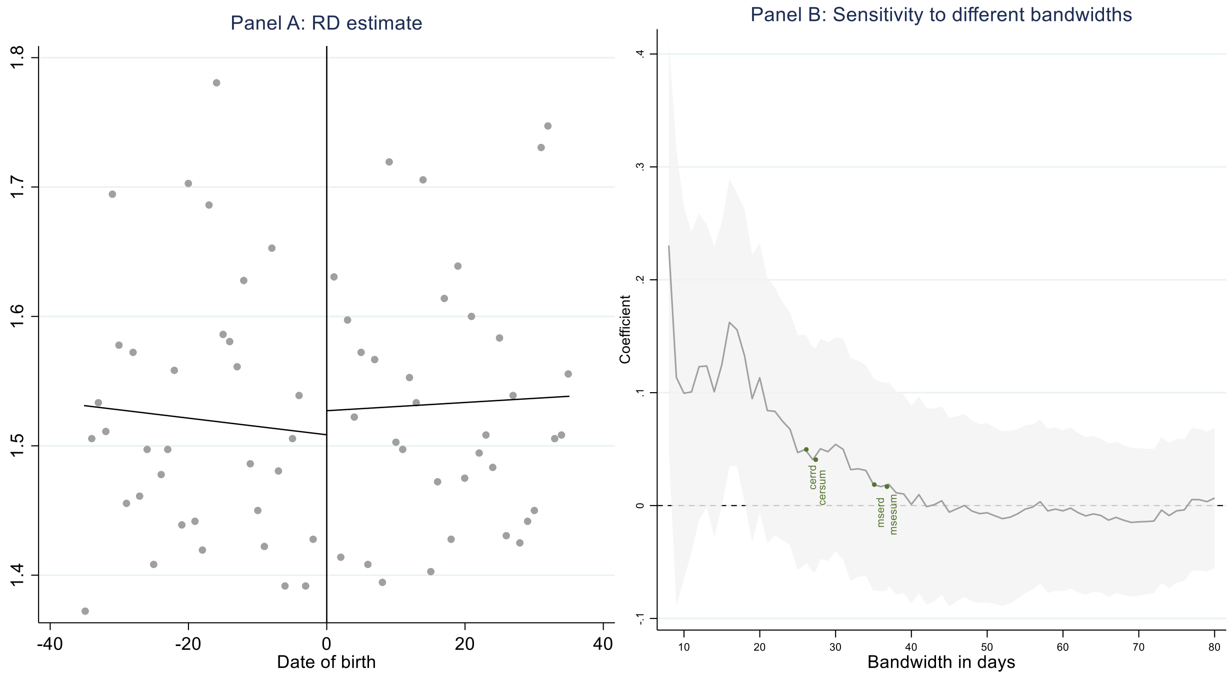
Data source: LFS microdata, Spanish National Statistical Institute, 2000-2018.
Notes: This figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the probability of being married before specific ages (age plotted on the horizontal axis). Each coefficient comes from a different regression. Controls are birth cohorts computed from July to June the following year for July to June pairs from 1942-43 to 1994-95. The window around the cutoff is one month.

Figure A.7. Distribution of mothers' birth dates



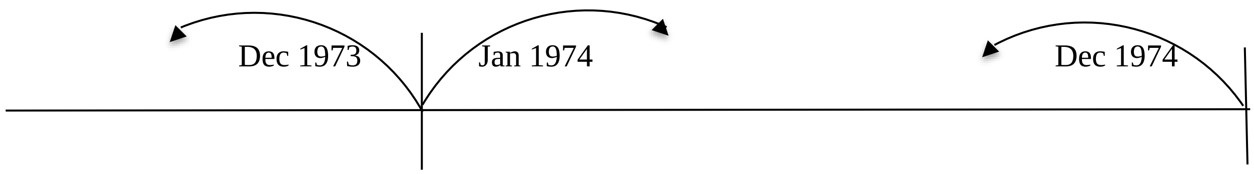
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

Figure A.8. Potential Mothers' Abortion Outcomes.



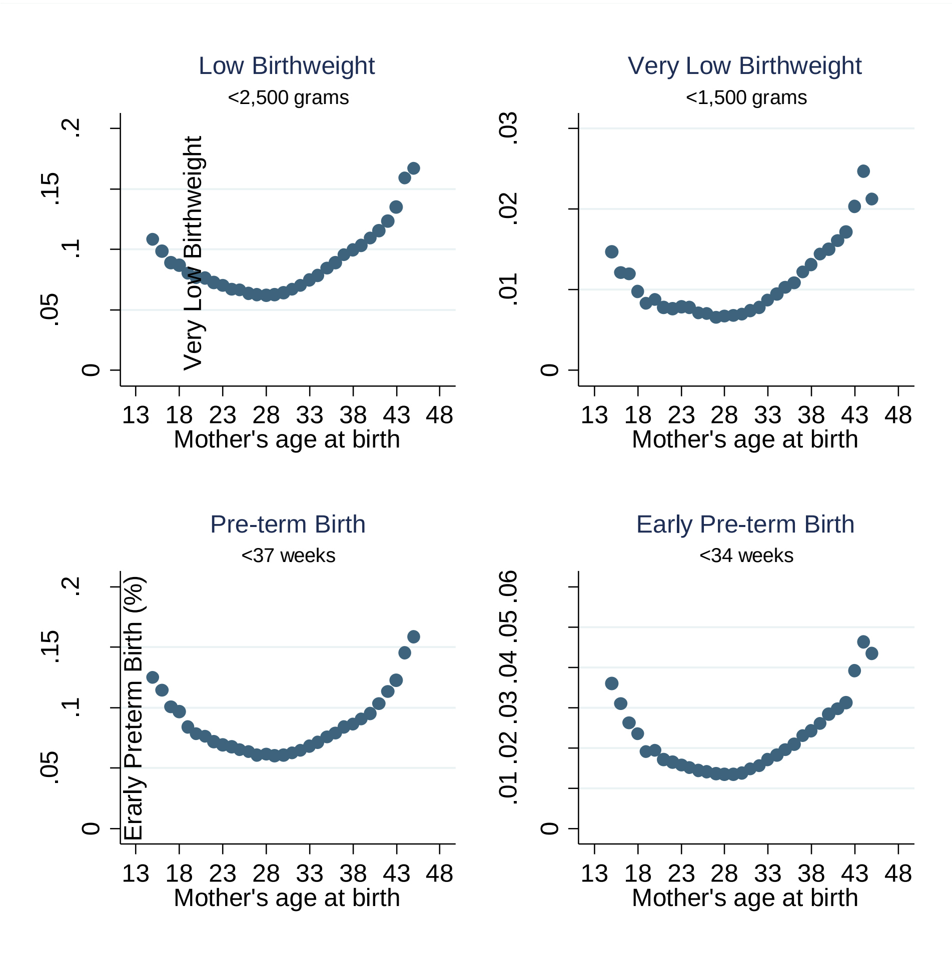
Notes: Panel A plots mean annual hospitalization rates by day of birth, together with a first order polynomial regression line fitted separately on each side of the cutoff. Panel B plots RDD estimates using different bandwidth selection methods. Shaded area represents the 95% confidence intervals. The highlighted points correspond to the optimal bandwidth selection methods mserd (MSE-optimal bandwidth selector for the RD treatment effect estimator), msesum (MSE-optimal bandwidth selector for the sum of regression estimates), cerrd (Coverage Error Rate (CER)-optimal bandwidth selector for the RD treatment effect estimator), and cersum (CER-optimal bandwidth selector for the sum of regression estimates). The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects.
Source: Spanish Hospital Discharge Records, Ministry of Healthcare, 2004-2015

Figure A.9. How Social Age Creates a Gap in Fertility Timing



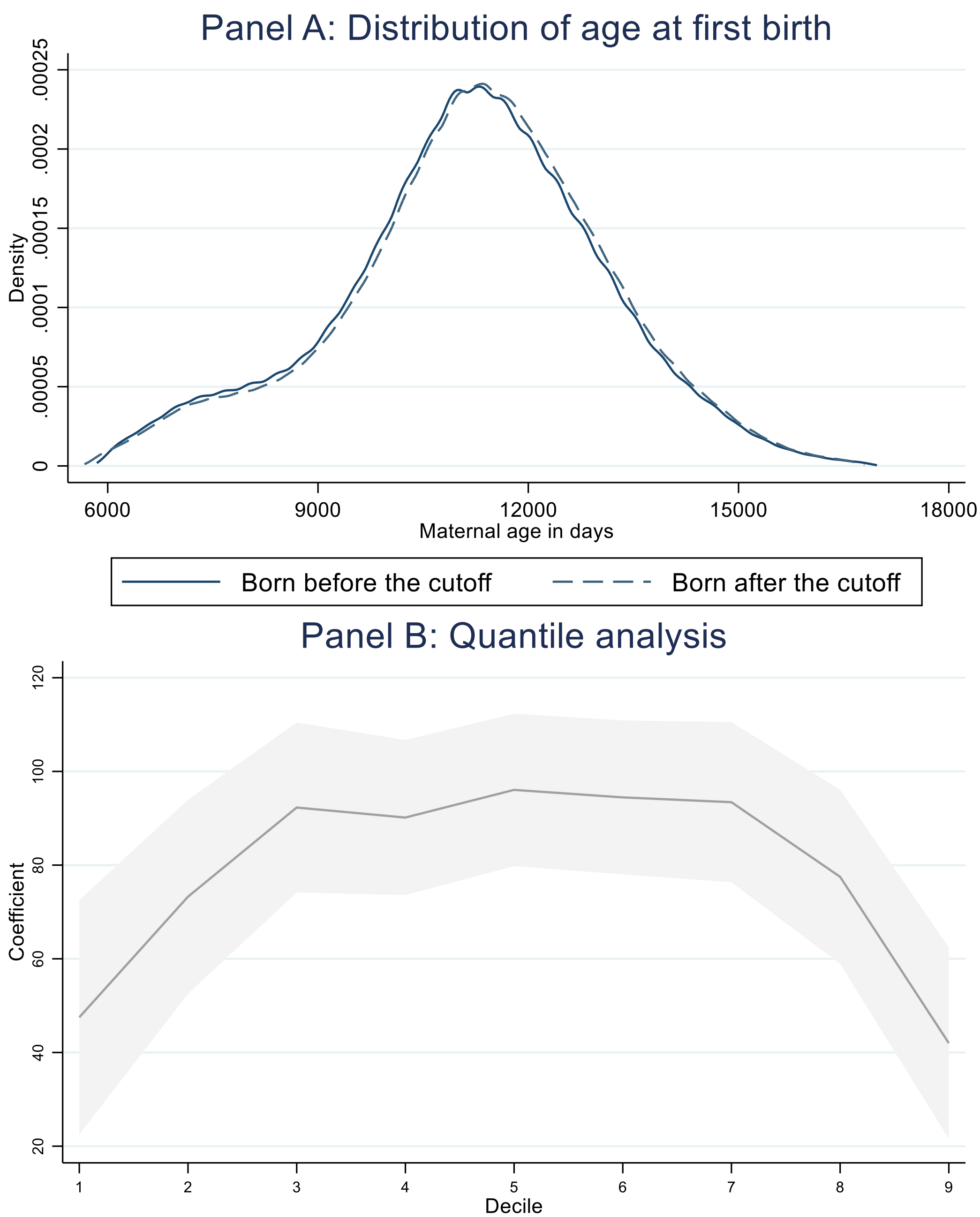
Notes: The figure shows two contiguous school cohorts. We compare women of the same biological age born around the school cutoff of January 1st in 1974. Women born in January 1974 will tend to have children alongside their peers at a later age, when they are older, and women born in December 1973 will tend to have children alongside their peers at an earlier age. This creates a jump in the biological age at which women have their first child around the school entry cutoff of January 1st.

Figure A.10. Descriptive Associations. Mothers aged 15 to 44.



Notes: The sample includes all first births to Spanish mothers. Raw data with no controls.
Source: Vital Statistics Data. Spanish National Statistical Institute. 1996-2018.

Figure A.11. Impact of being born after the cutoff along the distribution of age at first birth



Notes: Panel A plots the distribution of maternal age at first birth separately for mothers born before the cutoff and mothers born after the cutoff. Panel B plots the estimated RD coefficient for maternal age at first birth along different deciles of age at first birth using quantile regression techniques.
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.