Maternal Age and Infant Health

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July 2021

Barcelona GSE Working Paper Series

Working Paper nº 1276

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Abstract: We study the effects of maternal age on infant health. Age at birth has been increasing for the past several decades in many countries, and correlations show that health at birth is worse for children born to older mothers. In order to identify causal effects, we exploit school entry cutoffs and the empirical finding that women who are older for their cohort in school tend to give birth later. In Spain, children born in December start school a year earlier than those born the following January, despite being essentially the same age. We show that as a result, January-born women finish school later and are (several months) older when they marry and when they have their first child. We find no effect on educational attainment. We then compare the health at birth of the children of women born in January versus the previous December, using administrative, population-level data, and following a regression discontinuity design. We find small and insignificant effects on average weight at birth, but the children of January-born mothers are more likely to have very low birthweight. We interpret our results as suggestive of a causal effect of maternal age on infant health, concentrated in the left tail of the birthweight distribution, with older mothers more likely to give birth to (very) premature babies.

Keywords: Maternal age, infant health, school cohort.

JEL: I12, J12, J13

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* Borra acknowledges funding from FEDER/Ministerio de Ciencia e Innovación–Agencia Estatal de Investigación (RTI2018-098217-B-I00). González acknowledges funding from the European Research Council (CoG MISSINGMIDDLE-770958) and the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Program for Centers of Excellence in R&D (CEX2019- 000915-S). We thank the General Directorate for Universities of Junta de Andalucía and Alfonso María de Bengoa Díaz for giving us access to Andalusian university admissions anonymized administrative data.

1. Introduction

We provide new empirical evidence on the causal effects of maternal age on infant health, using data on all births in Spain from 1996 to 2018. We use strict school entry cutoff dates to estimate the effect of delaying motherhood (by several months) on newborn health.

Age at first birth has increased steadily over the past forty years in OECD countries, from about 25 to almost 30. There are social and medical reasons behind these trends, including increases in educational attainment (Monstad et al., 2008) and labor market factors (Amuedo-Dorantes and Kimmel, 2005), as well as improved contraceptive methods and fertility treatment options (Buckles, 2013; Goldin and Katz, 2002; Machado and Sanz-de-Galdeano, 2015).

The medical literature has emphasized the increased likelihood of adverse birth outcomes for both younger and older mothers (Fraser et al., 1995; Royer, 2004). For instance, descriptive studies from the medical literature document that teenage mothers have higher risks of eclampsia, puerperal endometritis, systemic infections, low birthweight, and preterm delivery (Fraser et al., 1995; Ganchimeg et al., 2014). Even after adjusting for maternal race, marital status, and prenatal care, teenage pregnancy increases the risk of low birthweight by 20% and the risk of prematurity by 14% (Chen et al., 2007).

Older mothers also display worse birth outcomes. 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 advanced maternal age 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).

Figure 1 shows average birth outcomes in Spain by maternal age. The U-shape indicates that both lower and higher maternal ages are associated with adverse birth outcomes, such as low birthweight and prematurity. However, the extent to which these associations are causal is unclear.

The effect of mothers' age at first birth on infant health is not only interesting in itself, but also because low birth weight and preterm delivery may impact long-term 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).

In this paper, we use the natural experiment generated by school entry cutoff dates to estimate the causal impact of delaying motherhood (by several months) on infant health. Mothers born right after the school entry cutoff date are relatively older in their school cohort. Given that individuals tend to interact with other individuals in their same school cohort, the timing of social behaviors such as motherhood is likely to be influenced by the average age of the school cohort. In addition, those who are older when they start school will also be older when they finish compulsory education. As a result, those born early in the school cohort may be older than average when experiencing demographic events, while those born late in the school cohort may be younger than average (Skirbekk et al., 2004).

While delaying motherhood by a few months may be beneficial for children's health in very young mothers, for the average mother giving birth now at around 31 years of age, postponing childbearing may give rise to complications, such as low birth weight and prematurity.

Our estimation strategy compares birth outcomes of women born shortly before and after the school cohort cutoff date, which in Spain is January 1st, controlling for cohort fixed effects. We use different data sets to, first, set up our estimation strategy, and second, estimate the causal impact of starting school later (and as a result delaying motherhood) 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 administrative data from university admissions (in the region of Andalusia) from 2003 to 2016, together with data from the Spanish Labor Force Survey (LFS) from 2000 to 2018, to show that date of birth around the cutoff does not influence educational attainment in the intensive nor the extensive margins.

We then use survey data from the LFS and population data from Spanish Vital Statistics from 1996 to 2018 to document that being born after the cutoff does not affect the likelihood of becoming a mother, but it increases average maternal age by about 3 months.

Finally, we use Vital Statistics to estimate the causal impact of delaying motherhood (due to the school entry cutoff) on fetal health. We find that January-born mothers are 12% 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. These impacts prove to be quantitatively and economically significant, and basically consistent with the associations reported in the medical literature. However, unlike this literature, we find no impact in average birthweight, the fraction of low birthweight (<2,500g.) babies, or the risk of premature birth (before 37 weeks).

To our knowledge, ours is one of the few studies to date that provide credible causal evidence on the effect of delaying motherhood on infant health outcomes in a large, developed country. Fredriksson et al (2021) follow a similar identification strategy with Finnish data, and find results consistent with ours (small effects on birthweight). Goisis et al. (2017) also study the causal impact of mothers' age at birth on health outcomes in Finland. They use family fixed effects, where their results are subject to bias due to potentially idiosyncratic responses to the previous birth (see

Rosenzweig and Wolpin 1995 and Behrman and Rosenzweig 2004 on the limitations of the fixed effects approach). Like these other studies, we use population-level data.

We also contribute to the literature that uses month or season of birth to capture the causal effect of education on later life outcomes, by pointing out the importance of delays and the potential influence of social age (Angrist and Krueger 1991; McCrary and Royer 2011; Buckles and Hungerman 2013). If season of birth is to be used as an instrument for education, these sorts of timing effects must be carefully considered.

2. Institutional Background

Compulsory education in Spain lasts 10 years (ages 6 to 16). The school entry cutoff is January 1st and, as shown by Berniell and Estrada (2020), compliance with the cutoff rule is very high. 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.

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 Figure A.1 in the Appendix), while labor force participation rates have increased (see Figure A.2).[1] Marriage rates have also decreased and, similarly to other western countries, marriage decisions are increasingly dissociated with childbearing decisions (Bailey et al., 2014; Lesthaeghe, 2014). 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. Date of Birth and School Enrolment as a Natural Experiment

Most school systems have a single-entry cutoff date to access compulsory education. This generates about a year of difference in ages between the youngest (in our case born at the end of December) and the oldest (in our case born at the beginning of January) in the school cohort (Bedard and Dhuey, 2006). This relationship between date of birth and the schooling cutoff date may create additional differences in terms of demographic behaviors later on between individuals born before and after the cutoff.

Individuals tend to interact with other individuals in their same school cohort, that is, individuals 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 fertility. In consequence, those born early in the school cohort may be older than

[1] 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.

the average when experiencing demographic events, and those born late in the school cohort may be younger than average (Røed Larsen and Solli, 2017; Skirbekk et al., 2004).

Figure 2 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, if women born in January 1974 tend to have children with their school cohort, they will tend to “delay” childbirth, relative to women born in December 1973. 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.

In this paper we aim at estimating the causal impact of age at motherhood on infant health outcomes. Women who give birth at older ages are usually more educated and more career-oriented and come from more educated backgrounds, relative to younger mothers. Therefore, simply comparing health outcomes of children born to older and younger mothers would not offer an unbiased estimate of the impact of age at motherhood on infant health outcomes. To solve the endogeneity of the timing of motherhood, we propose using the gap in fertility timing generated by school entry cutoff dates as a natural experiment.

A primary threat to the validity of our research design is that parents may time their births around the cutoff so that birth dates of potential mothers close to the threshold may not be considered quasi-random (sorting across the threshold). For instance, Buckles and Hungerman (2013) find that season of birth may not be random in the United States, given that older, more educated mothers tend to avoid having winter births.

Similarly, Shigeoka (2015) in Japan and Huang et al (2020) in China show that births are shifted from before to after the cutoff in Japan and from after to before the cutoff in China. The alleged reasons are that in Japan parents want their children to excel in school to maximize their chances of attending a good university; conversely in China, parents maximize future labor market experience instead.

However, in other countries available evidence does not support birth timing with respect to school entry dates. Fredriksson and Ockert (2013) in Sweden and Black et al (2011) in Norway show that there is no evidence of parents in these countries systematically timing births around the cutoff. In Section 6 we show that there is no evidence of parents systematically timing births with respect the cutoff date for affected cohorts in Spain, in terms of number of births nor observed family background characteristics.

School entry policies may affect child outcomes at birth not only through maternal age, but also through other channels such as maternal education. Older students in the cohort tend to perform better during primary education (Bedard and Dhuey, 2006; Calsamiglia and Loviglio, 2020). 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 (Fredriksson, Huttunen, and Öckert 2021; Meulen 2019; Black, Devereux, and Salvanes 2011).

School starting age legislation can also influence marital status. Women may tend to 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). In Section 6 we show that birth-date around the cutoff does not affect educational attainment or partnership status in Spain.

Finally, not all women become mothers. Our identification strategy would be threatened by differences in the fertility between women born around the cutoff. That is, our identification strategy requires that women born after the cutoff have children later, but end up with the same completed fertility than women born before the cutoff (Fredriksson et al., 2021; McCrary and Royer, 2011). Section 6 also shows that there is no evidence of selection into motherhood for affected cohorts in Spain.

4. Empirical Strategy

We want to estimate the impact of maternal age at first birth on infant health outcomes. As in McCrary and Roger (2011), we follow a Regression Discontinuity (RD) design using the fact that women born after the school cutoff date tend to have children later than those born before the cutoff. We estimate the following reduced-form equation:

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

where Y denotes our outcomes of interest (maternal age and fetal health). More specifically, we use maternal age in days, and the following measures of health at birth: 24-hour mortality, birthweight, low birthweight, very low birthweight, gestation weeks, pre-term birth, and early pre-term birth. The variable *Treat* is an indicator for births on or after January 1st, each year; *Date* is the running variable, defined as the difference between the date of birth of the potential mother and the January 1st cutoff; *f(.)* is a kernel or polynomial of the running variable; and τ_t are cohort 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 test the validity of our identification strategy, we test for balance in covariates in other maternal characteristics (at the time of mothers' birth), as well as for potential effects of the school cutoff date on other outcomes (fertility, partnership, and educational attainment). Absent exact date of birth for some of these outcomes, we estimate the following equation, using the local randomization framework for RD designs (Calonico et al., 2019):[2]

$$Y_{imt} = \alpha + \beta Treat + \tau_t + \varepsilon_{imt} \qquad (2)$$

where *Y* denotes the outcome, 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 as health and family outcomes at birth, and whether educational attainment is primary, secondary or university education, and, importantly, whether she has had her first child before specific ages (18, 23, 28, 33, 38, 43, and 48 years of age) as demographic outcomes in adulthood. The variable *Treat* is an indicator for women born on or after January 1st

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

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.

5. 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 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 maternal age at birth on 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. 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 8). 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.[3] 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 8 we show that our results hold for an unrestricted sample of all first births to Spanish mothers.

Our main analysis variable is mother's age at first birth. 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. Other outcomes are birth weight and indicators for low birth weight (below 2500 grams) and very low birth weight (below 1500 grams) and 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 (11196/365), the baby's weight at birth is over 3000 grams, and gestation lasts about 39 weeks.

To show the validity of the RD design we use three additional datasets: administrative data from Spanish Vital Statistics from 1980 to 1995, survey data from the Spanish Labour Force Survey, and administrative data from Andalusian University Entry exams from 2003 to 2016.

We use Vital Statistics from 1980, first year that birth records include health data, to 1995 to study women's health and family background at birth. 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.

[3] 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.

We use the Spanish Labour Force Survey from 2000 to 2018 to study fertility, education, and partnership outcomes in adulthood for all women born around the January 1st cutoff. We first examine the probability of giving birth before 18, 23, 28, 33, 38, 43, and 48 years of age. We then look at educational attainment by computing indicators for primary education or less, secondary education, and university education. We finally also compute indicators for being married and for living in a partnership. Panel B in Table A.1 in the Appendix provides summary statistics.

We complement the above information by looking at the intensive margin of educational attainment using administrative data of all University Entry exams from Andalusia, the largest region in Spain, from 2003 to 2016. We study the total number of students taking the test, together with indicators for passing the test and grading for those who passed. 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.[4] Panel C in Table A.1 also shows descriptive statistics for the variables in this dataset.

6. 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 are comparable with respect to other characteristics. In particular, we show that, first, potential mothers' birthdates can be considered quasi-random around the cutoff, and second, school-entry policies did not impact potential mothers' educational attainment nor selection into marriage nor motherhood.

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

Figure 3 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 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.

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). If this initial advantage translates into different levels of educational attainment at either side of the cutoff, our methodology would not be able to isolate the impact of maternal age on health outcomes at birth. We provide evidence defending that date of birth does not impact neither the extensive nor the intensive margins of potential mothers' educational outcomes. Columns 3 and 4 in Table 2 show that there is in fact a mechanical effect of age on educational attainment during the early adulthood -16 to 25 year-olds, that comes from the fact that younger women born in January belong to a cohort that has not had time to finish their studies, compared to those born in December the previous year. For the younger cohorts, women born after the school entry cutoff are 0.6 pp (26%) more likely to be primary educated, 3.4 pp (4%) more likely to be secondary educated and 4.9 pp (28%) less likely to be university educated. However, from ages 25 and over, this mechanical impact disappears, and women born before and after the cutoff are equally likely to be primary, secondary, or university educated (Columns 5 to 8).

Regarding the intensive margin, we also find that being born after the cutoff does not grant any advantages with respect to university admissions tests. To start with, date of birth does not predict the probability of taking the admission test (See Figure A.3 in the Appendix). We 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 (t-statistic -1.0464, p-value 0.295). Table 3 (and Figure A.3 in the Appendix) further shows that older students in the cohort, born after January 1st obtain university entry scores that are indistinguishable from scores from younger students born before the cutoff. Consistent with the findings by Calsamiglia and Loviglio (2020) for younger children, panels A and B show that older students are about 8 pp (36%) less likely to have repeated a previous school year and about 0.4pp (100%) more likely to be advanced for their age. However, Panels C to G show that there are no 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. 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.

Finally, even if long-term educational attainment is 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 and delaying motherhood may impact their chances of becoming mother. If only very healthy women born after the cutoff succeed in becoming 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 had if all mothers had had the same chances of success in becoming pregnant independently of their date of birth.

Figure 4 (and Panel A in Table A.3 in the Appendix) shows the impact of being born after the cutoff on the probability of being mother for the first time before specific ages. It clearly shows that there are no differences in the probability of ever becoming mother at either end of the age distribution. In particular, being born after the cutoff does not affect the chances of having had a child after the age of 40. We therefore find no evidence of selection into motherhood as a result of being born early in a cohort. Panel B in Table A.3 in the Appendix further shows 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 (2021), this evidence supports the assumption that being born early in the cohort does not influence selection into motherhood or completed fertility.[5]

Figures 5 and 6 (and Panels C and D in Table A.3 in the Appendix) also document 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 marriage or partnership. The fact that school entry policies influence motherhood

[5] Figure A.4 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.

timing but not so much partnership or marriage timing is consistent with recent evidence documenting the decoupling of marriage and motherhood in Spain and other Southern European countries (Lesthaeghe, 2014).

7. School Entry Rules, Age at First Birth and Infant Health

We now turn to our main results on the impact of being born after the school entry cutoff on birth outcomes. We begin by reporting estimates of the impact of school starting rules on age at first birth, our first stage equation. Figure 7 shows that being born after the school entry cohort in Spain delays mothers' age at first birth by approximately 3 months (about 90 days). The point estimates in Panel A of Table 4 are significantly positive and range from 87 to 91 days of delay. This first result is thus highly robust to changes in bandwidth selection methods, kernel functions, and polynomial orders. 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.

Figure 8 and Panels B to H in Table 4 report our main reduced form results of the impact of being born after the cutoff on infant health. 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, delaying childbearing by about 3 months 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, delaying motherhood by about 3 months increases the likelihood of having a very low birthweight newborn by 18 percent. 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. 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 12 percent. Most specifications also find a corresponding reduction in gestation weeks of about 2.5 percentage points (0.06 percent).

7.1. 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 delaying motherhood by about three months 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 labor legislation that may be impacting both children outcomes and mothers' age at first birth.

Table 5 displays the results for these exercises. Column 1 in Table 5 reproduces the estimates of our main analysis in Column 1 in Table 4. 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 delays motherhood by about three months, 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, age at birth increases about 2.7 months, the likelihood of having a very-low-birthweight newborn increases by 19 percent, the likelihood of having an early pre-term birth increases by 12 percent, and gestation weeks drop by

0.05 percent.

Column 4 in Table 5 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. Given that advanced maternal age is also associated to naturally occurring multiple births, this exercise can be considered to estimate lower bounds for the impact of motherhood delays (Adashi and Gutman, 2018; Buckles, 2013). 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 4, age at birth increases almost three months, 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 10 percent, though is only marginally significant.

Lastly, columns 5 and 6 in Table 5 show that neither the 1980 change in the minimum working age nor the 1990 increase in compulsory education are behind the estimated impacts of age at first birth 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 age at first birth increases significantly by almost 3 months, 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 4. Age at first birth continues to increase almost 3 months and 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.

7.2. Interpreting the Magnitudes

We have seen that mothers being born after the school entry cutoff tend to delay motherhood by about 3 months compared to women born before the cutoff. This delay increases the likelihood of very low birthweight by about 18 percent and of early preterm by about 12 percent. In this section, we compare our findings to previous estimates both from quasi-experimental and within-family studies such as Goisis et al. (2017) and Fredriksson, Huttunen, and Öckert (2021), and from recent metanalyses of the association of advanced maternal age and birth outcomes from the medical literature (Lean et al., 2017). To defend the external validity of our data, specifically, that the negative associations reported in the medical literature are also present in our data, we additionally offer the estimated correlation of an indicator for maternal age over 35 years and our different health-at-birth outcomes.

The estimated impact of school entry policies on maternal age at first birth shown in Panel A of Table 4 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 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 Labour 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 6 compares our RD-estimates from Table 4 to previous results in the literature and shows that our small and statistically insignificant results for infant health at birth are consistent with previous quasi-experimental and siblings fixed-effects evidence, though less so with previous findings from the medical literature. 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 C of Table 6). Even if advanced age is associated with approximately 37 percent increase in the chances of newborn mortality in our data (Panel B), we fail to find a significant causal impact of delayed motherhood on infant mortality (Panel A).[6]

Our small and in general statistically insignificant results for birthweight, low birthweight, gestation weeks, and premature birth are basically in line with previous quasi-experimental and within-family estimates. For instance, Goisis et al. (2017) find statistically insignificant associations between maternal ages over 35 years and the risk of low birth weight or preterm delivery (see Panel D in Table 6). 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 E in Table 6). We fail to find any statistically significant impact of motherhood delay on birthweight

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

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).

One instance in which our results tend to agree with the medical literature is the impact of maternal age on the risks of very low birthweight (column 4 in Table 6). 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 E in Table 4). As reported in Panels C and D in Table 6, the medical literature -and our own data- finds on average a 60-percent increase in the risk of low-birthweight for mothers aged at least 35, compared to mothers aged 29 to 34 (Lean et al., 2017). We can conclude that, with respect to very low birthweight, our estimates are not only statistically and economically significant, but also consistent with the associations in the medical literature.

Thus far, we have shown that our estimates are coincident with the significant associations in the medical literature for very-low birthweight, but not for other birth outcomes. 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.4 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 but we do have power to detect effect sizes larger than 5% of the sample mean for birthweight, low birthweight, very low birthweight, gestation week, and pre-term birth.

9. Conclusions

We exploit strict school entry cutoffs in Spain to study the effects of maternal age on infant health. 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. In order to get at causal effects, we exploit that fact that 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 show that as a result, January-born women finish school later and are (several months) older when they marry and when they have their first child.

We then 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 birth-weight. January-born mothers are 12% more likely to have an early pre-term birth, and 18% more likely to give birth to a very low birthweight child. 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 in average birthweight, the fraction of low birthweight (<2,500g.) babies, or the risk of premature birth (before 37 weeks).

We interpret our results as suggestive of a causal effect of maternal age on infant health, concentrated in the left tail of the birthweight distribution, with older mothers more likely to give birth to (very) premature babies.

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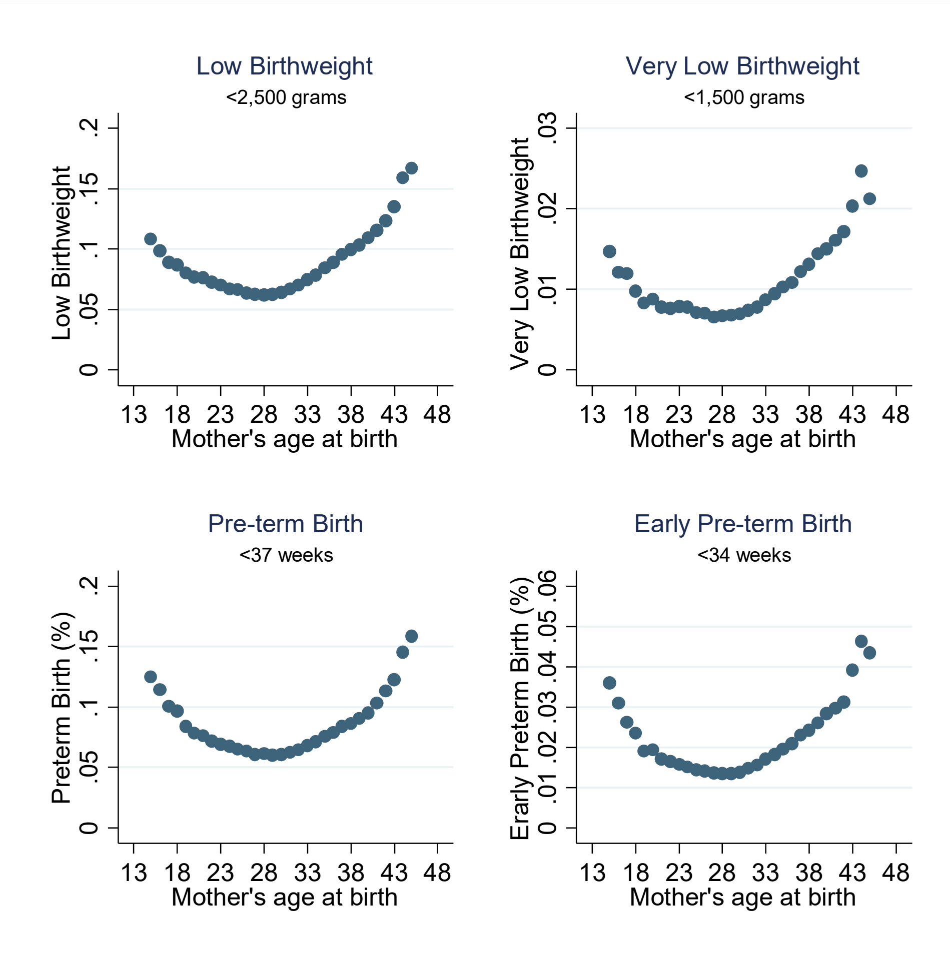
Figure 1. Descriptive Associations. Mothers aged 15 to 44.



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 2. How Social Age Creates a Gap in Fertility Timing

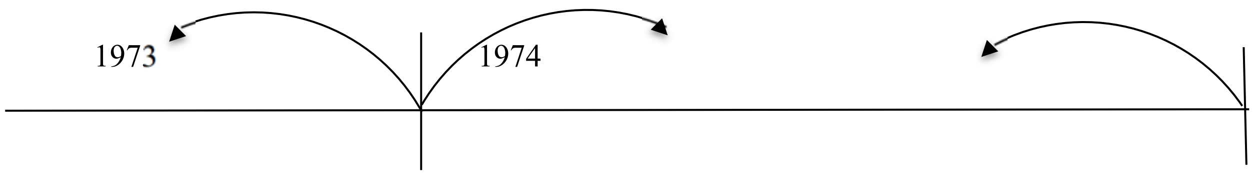
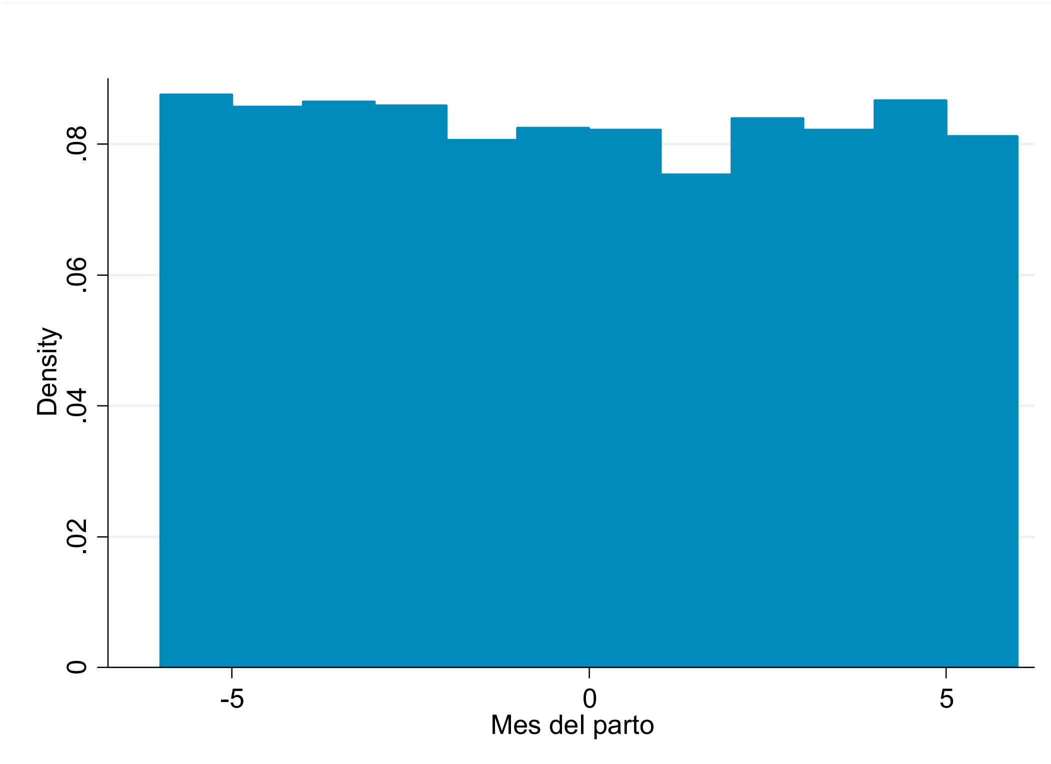
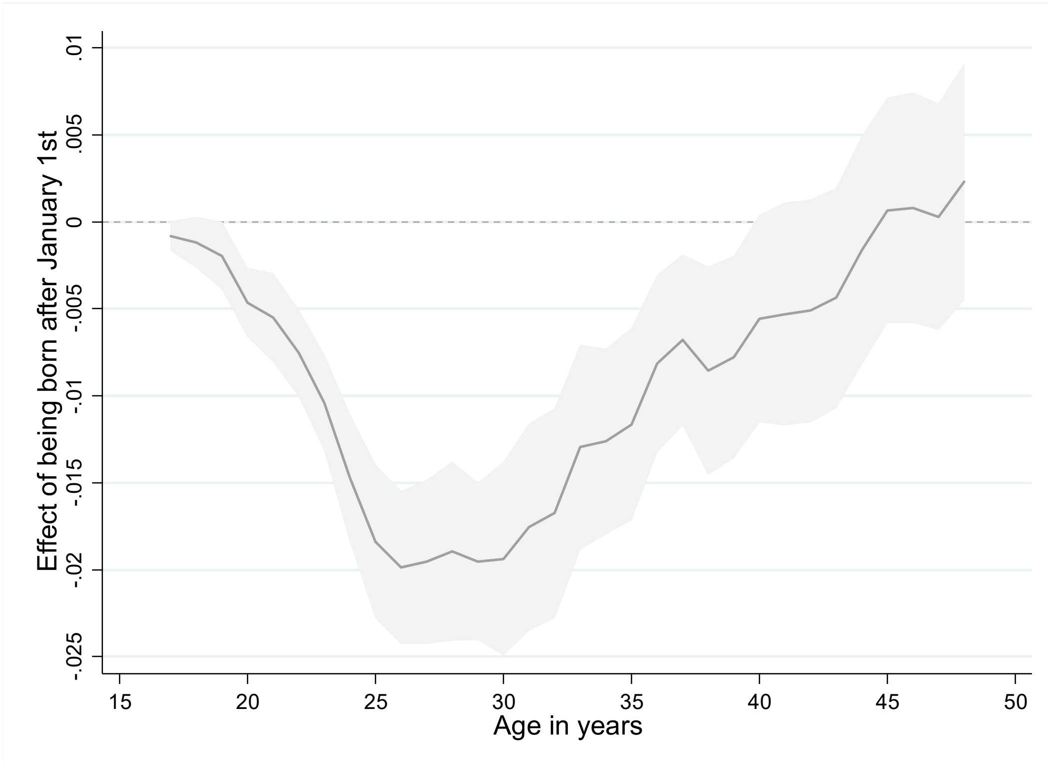


Figure 3. Distribution of months of birth dates of potential mothers



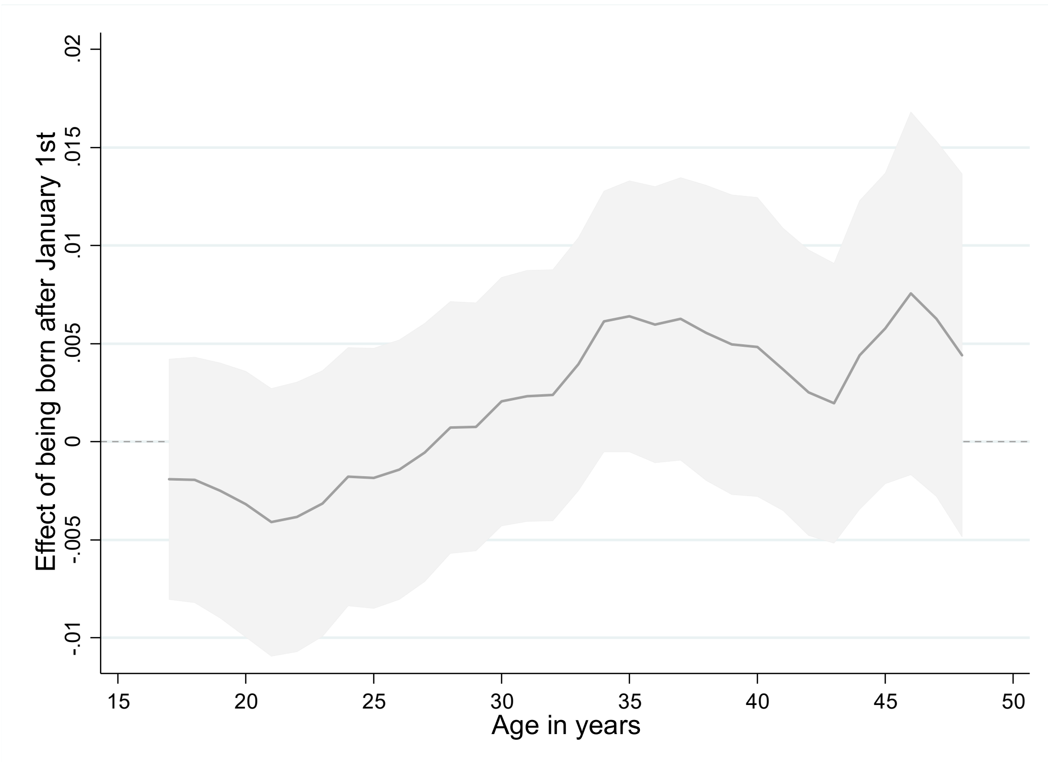
Source: Spanish Vital Statistics. Spanish National Statistical Institute. 1980-1995.

Figure 4. Impact of Being Born Early in a Cohort on the Probability of Becoming Mother for the First Time before a Specific Age



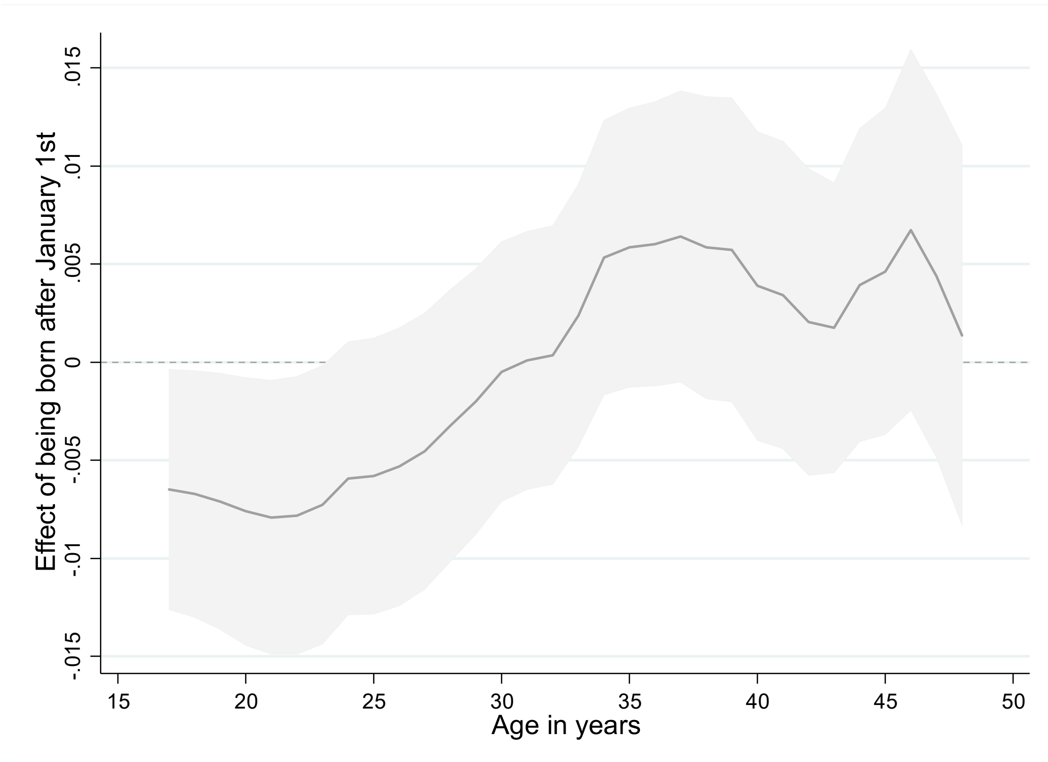
Data source: EPA microdata, Spanish National Statistical Institute, 2000-2018.
Notes: This figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the probability to give birth before specific ages (age plotted on the horizontal 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 5. Impact of Being Born Early in a Cohort on the Probability of Being in a Partnership by Age



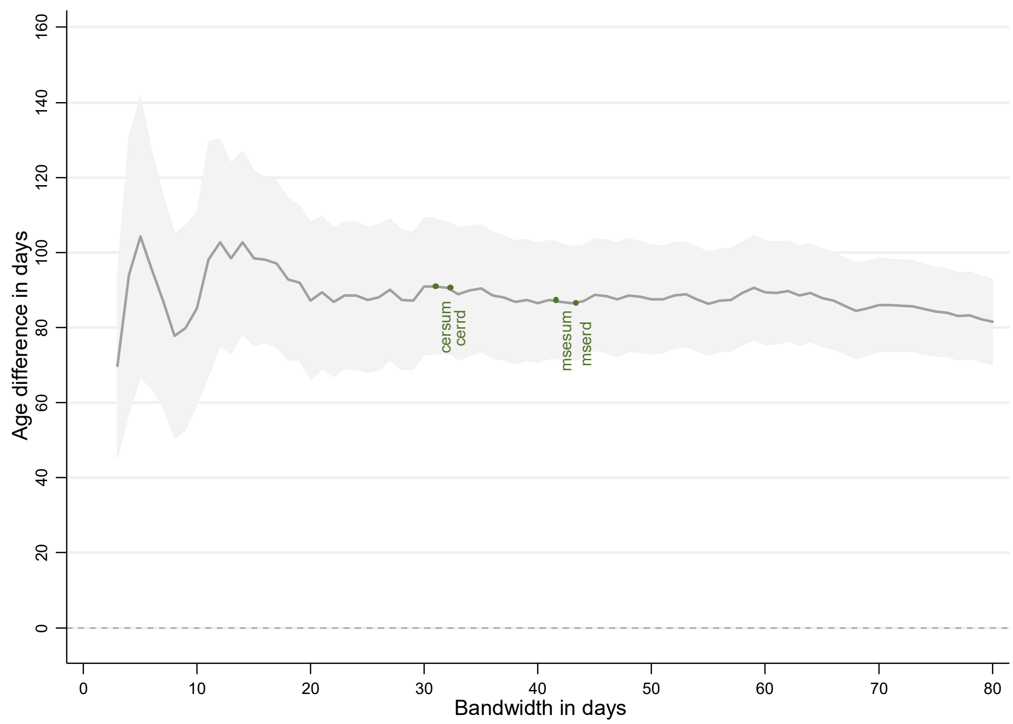
Data source: EPA microdata, Spanish National Statistical Institute, 2000-2018.
Notes: This figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the probability of being in a partnership before specific ages (age plotted 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 6. Impact of Being Born Early in a Cohort on the Probability of Being Married by Age



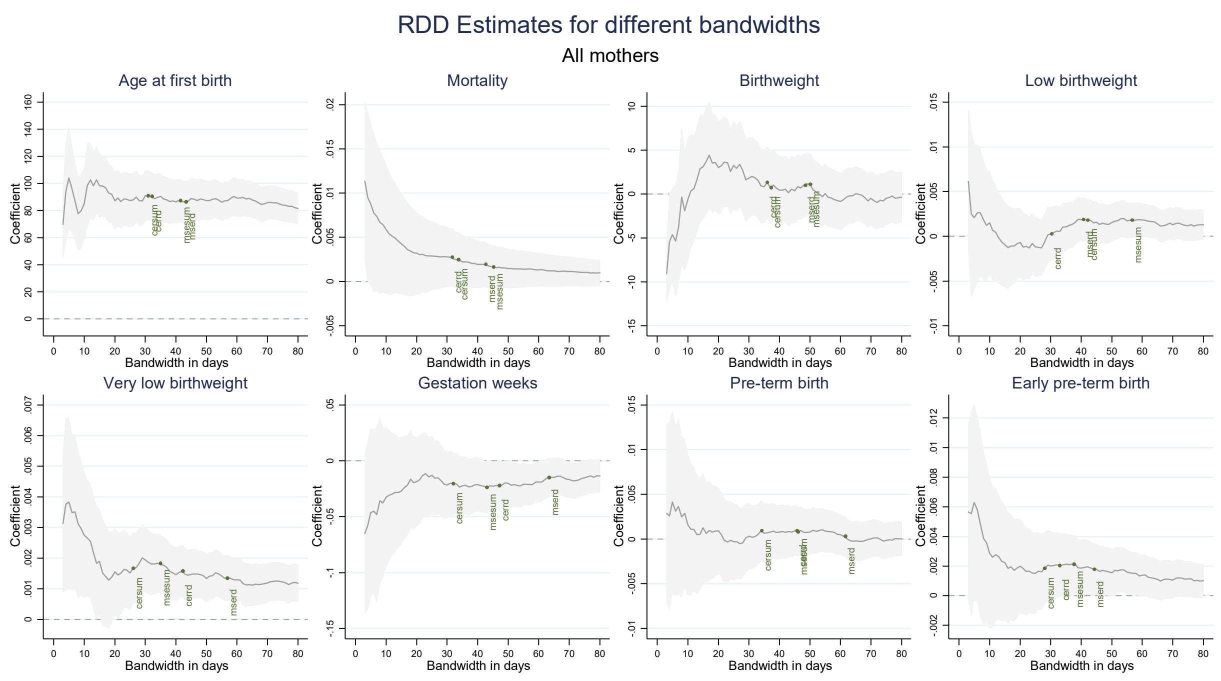
Data source: EPA microdata, Spanish National Statistical Institute, 2000-2018.
Notes: This figure plots the estimated coefficients for the binary indicator taking value 1 for women born after the school cutoff of January 1st on the probability of being married before specific ages (age plotted on the 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 7. Impact of Being Born Early in a Cohort on Maternal Age (in Days). All Mothers 15-44 Years Old. Optimum 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, msesum, cerrd, and cersum. The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects.
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018

Figure 8. Impacts of Being Born Early in a Cohort on Children's Health Outcomes. All Mothers 15-44 Years Old. Optimum 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, msesum, cerrd, and cersum. The coefficients were computed using a uniform kernel function, a first order polynomial, and cohort fixed effects.

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

Tables

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

	Obs.	Average	Stdev.	Median
Panel A. Vital Statistics Data. Outcome Variables				
Treatment	4471412	0.4979	0.5	0
Maternal age in days	4471412	11196.4	1895.9	11287
Mortality	4471412	0.0051	0.071	0
Weight	4266386	3182.8	511.8	3200
Low birth weight	4266386	0.0742	0.262	0
Very low birth weight	4266386	0.0088	0.093	0
Gestation weeks	3883391	39.1357	1.94	40
Pre-term birth	3883391	0.0701	0.255	0
Early Pre-term birth	3883391	0.0173	0.13	0
Normal birth	4471412	0.8358	0.37	1
C-section birth	2278973	0.2788	0.448	0
Pair	4471412	33.6614	7.212	33
Year the mother is born	4471412	1976.16	7.204	1976
Month mother is born	4471412	6.4979	3.418	7
Day mother is born	4471412	15.6714	8.799	16
Panel B. Vital Statistics Data. Background variables				
Baby is a girl	4471412	0.4845	0.500	0
Multiple birth	4471412	0.0148	0.121	0
Married mother	4471412	0.6485	0.477	1
Registered dad	4471412	0.0211	0.144	0
Mother employed	4471412	0.6531	0.476	1
Mother high skilled	4471412	0.2227	0.416	0
Mother homemaker	4471412	0.1601	0.367	0
Primary or less	2100595	0.1737	0.379	0
Secondary	2100595	0.469	0.499	0
University	2100595	0.3573	0.479	0
Father’s age	4471412	31.7477	7.239	32
Father employed	4471412	0.8059	0.395	1
Father high skilled	4471412	0.2199	0.414	0
Rural	4471412	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. Potential Mothers' Educational Attainment. EPA Sample 2000-2018. Aged 16-45. Different Age Brackets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var:	16_45	16_45	16_25	16_25	25_35	25_35	35_45	35_45
Primary-less	0.0020	0.0014	0.016***	0.013***	0.0011	-0.0032	-0.0060*	-0.0029
	(0.0019)	(0.0015)	(0.0034)	(0.0026)	(0.0022)	(0.0023)	(0.0031)	(0.0026)
Mean / Std. dev	0.0821/0.275	0.0821/0.275	0.0604	0.0604/0.238	0.0604/0.238	0.0547/0.227	0.1142/0.318	0.1142/0.318
Secondary	0.0069**	0.0068**	0.034***	0.036***	0.0011	-0.0020	0.00082	-0.0025
	(0.0027)	(0.0028)	(0.0052)	(0.0045)	(0.0058)	(0.0048)	(0.0033)	(0.0040)
Mean / Std. dev	0.5626/0.496	0.5626/0.496	0.766	0.766/0.423	0.4639/0.499	0.4639/0.499	0.517/0.5	0.517/0.5
University	-0.0089***	-0.0081***	-0.049***	-0.049***	-0.0022	0.0052	0.0052*	0.0055
	(0.0029)	(0.0027)	(0.0033)	(0.0034)	(0.0054)	(0.0048)	(0.0031)	(0.0037)
Mean / Std. dev	0.3553/0.479	0.3553/0.479	0.1735	0.1735/0.379	0.4814/0.5	0.4814/0.5	0.3688/0.482	0.3688/0.482
Observations	91,045	176,568	23,027	44,482	28,423	54,782	35,753	69,779
Controls	Pair	Pair	Pair	Pair	Pair	Pair	Pair	Pair
Bandwidth	1month	2month	1month	2month	1month	2month	1month	2month

Data source: EPA 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. The outcome of interest is indicated in each row header. The sample includes all women born in December one year and January the following year (1month) or born from November one year to February the following year (2months) (depending on the column), for July to June pairs from 1954-55 to 2000-01. Robust standard errors are shown in parentheses.

Table 3. Potential Mothers' University Admissions Outcomes

VARIABLES		(1) Specification 1	(2) Specification 2	(3) Specification 3	(4) Specification 4	(5) Specification 5
Repeater		-0.0801***	-0.0764***	-0.0795***	-0.0781***	-0.0777***
(proportion 22.2%)		(0.00727)	(0.00849)	(0.00652)	(0.00862)	(0.00818)
	Obs.	193,100	193,100	193,100	193,100	193,100
Advanced		0.00420***	0.00463***	0.00408***	0.00481***	0.00479***
(proportion 4%)		(0.00105)	(0.00121)	(0.000981)	(0.00128)	(0.00133)
	Obs.	193,100	193,100	193,100	193,100	193,100
Passed 2000-09		0.0104	0.0154	0.0138	0.0155	0.0169
(proportion 95.6%)		(0.00869)	(0.0105)	(0.00858)	(0.0121)	(0.0115)
	Obs.	54,124	54,124	54,124	54,124	54,124
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)
	Obs.	51,732	51,732	51,732	51,732	51,732
Passed in ordinary call 2000-09		0.00966	0.0176	0.0113	0.0193	0.0182
(proportion 77.7%)		(0.0172)	(0.0208)	(0.0149)	(0.0198)	(0.0198)
	Obs.	54,124	54,124	54,124	54,124	54,124
Std score among ordinary passes 2000-09		0.0429	0.0417	0.0433	0.00993	0.0266
		(0.0452)	(0.0517)	(0.0477)	(0.0512)	(0.0517)
	Obs.	42,077	42,077	42,077	42,077	42,077
Std score 2010-16		0.0461	0.0540	0.0459	0.0386	0.0501
		(0.0436)	(0.0490)	(0.0441)	(0.0542)	(0.0510)
	Obs.	36,330	36,330	36,330	36,330	36,330
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)
	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, mserd and msesum methods. Robust standard errors in parentheses (clustered by date of birth).

Table 4. RDD Birth and Infant Health Outcomes.1996-2018. All Mothers

	(1)	(2)	(3)	(4)	(5)
Panel A. Maternal age in days (mean: 11,194.7)					
RD_Estimate	87.21***	89.70***	88.48***	90.78***	91.11***
	(7.811)	(8.978)	(7.927)	(8.913)	(8.748)
Panel B. Mortality (mean: 0.0051)					
RD_Estimate	0.00173	0.00254	0.00246	0.00279	0.00341
	(0.00127)	(0.00161)	(0.00167)	(0.00193)	(0.00225)
Panel C. Birthweight (mean: 3,182.8)					
RD_Estimate	0.946	1.179	0.690	1.314	2.580
	(1.795)	(2.038)	(1.759)	(2.659)	(2.516)
Panel D. Low Birthweight (mean: 0.0742)					
RD_Estimate	0.00220*	0.000332	0.000902	0.000110	-0.000354
	(0.00116)	(0.00138)	(0.00130)	(0.00196)	(0.00190)
Panel E. Very Low Birthweight (mean: 0.0088)					
RD_Estimate	0.00170***	0.00170***	0.00151***	0.00156***	0.00178***
	(0.000468)	(0.000565)	(0.000411)	(0.000494)	(0.000559)
Panel F. Gestation weeks (mean: 39.1357)					
RD_Estimate	-0.0266**	-0.0201	-0.0215*	-0.0251*	-0.0241*
	(0.0103)	(0.0127)	(0.0118)	(0.0134)	(0.0140)
Panel G. Pre-term birth (mean: 0.0681)					
RD_Estimate	0.000755	0.00101	0.000397	0.000761	0.000572
	(0.00107)	(0.00132)	(0.00118)	(0.00157)	(0.00175)
Panel H. Early pre-term birth (mean: 0.0173)					
RD_Estimate	0.00173*	0.00199*	0.00187*	0.00205**	0.00217*
	(0.000944)	(0.00117)	(0.00101)	(0.00104)	(0.00124)
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, mserd and msesum methods. Robust standard errors in parentheses (clustered by date of birth). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5. 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. Maternal age in days (mean: 11,194.7)						
RD_Estimate	87.21***	89.75***	80.23***	88.88***	88.55***	85.51***
	(7.811)	(7.894)	(7.316)	(8.066)	(8.506)	(7.991)
Panel B. Mortality (mean: 0.0051)						
RD_Estimate	0.00173	0.00169	0.00136	0.00140	0.00146	0.00190
	(0.00127)	(0.00125)	(0.00110)	(0.00116)	(0.00116)	(0.00149)
Panel C. Birthweight (mean: 3,182.8)						
RD_Estimate	0.946	0.579	0.205	1.119	-0.0942	-1.486
	(1.795)	(1.773)	(1.689)	(1.768)	(1.839)	(2.095)
Panel D. Low Birthweight (mean: 0.0742)						
RD_Estimate	0.00220*	0.00220*	0.00114	0.000611	0.00189	0.00242*
	(0.00116)	(0.00116)	(0.00120)	(0.00118)	(0.00119)	(0.00130)
Panel E. Very Low Birthweight (mean: 0.0088)						
RD_Estimate	0.00170***	0.00171***	0.00164***	0.00123***	0.00171***	0.00142***
	(0.000468)	(0.000462)	(0.000445)	(0.000471)	(0.000493)	(0.000451)
Panel F. Gestation weeks (mean: 39.1357)						
RD_Estimate	-0.0266**	-0.0262**	-0.0205**	-0.0183*	-0.0273**	-0.00794
	(0.0103)	(0.0107)	(0.0104)	(0.0104)	(0.0109)	(0.0111)
Panel G. Pre-term birth (mean: 0.0681)						
RD_Estimate	0.000755	0.000939	0.000322	0.000536	0.000557	0.000721
	(0.00107)	(0.00130)	(0.00127)	(0.00122)	(0.00149)	(0.00144)
Panel H. Early pre-term birth (mean: 0.0173)						
RD_Estimate	0.00173*	0.00209**	0.00171*	0.00155*	0.00220**	0.00146
	(0.000944)	(0.000966)	(0.000917)	(0.000866)	(0.000997)	(0.00102)
N. Obs.	4,467,970	4,467,970	4,484,139	4,402,165	3,900,429	3,389,645
Bw selection method	msecomb2	msecomb2	msecomb2	msecomb2	msecomb2	msecomb2
Kernel	Uni	Uni	Uni	Uni	Uni	Uni
Polynomial order	1	1	1	1	1	1

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. Robust standard errors in parentheses (clustered by date of birth). *** p<0.01, ** p<0.05, * p<0.1.

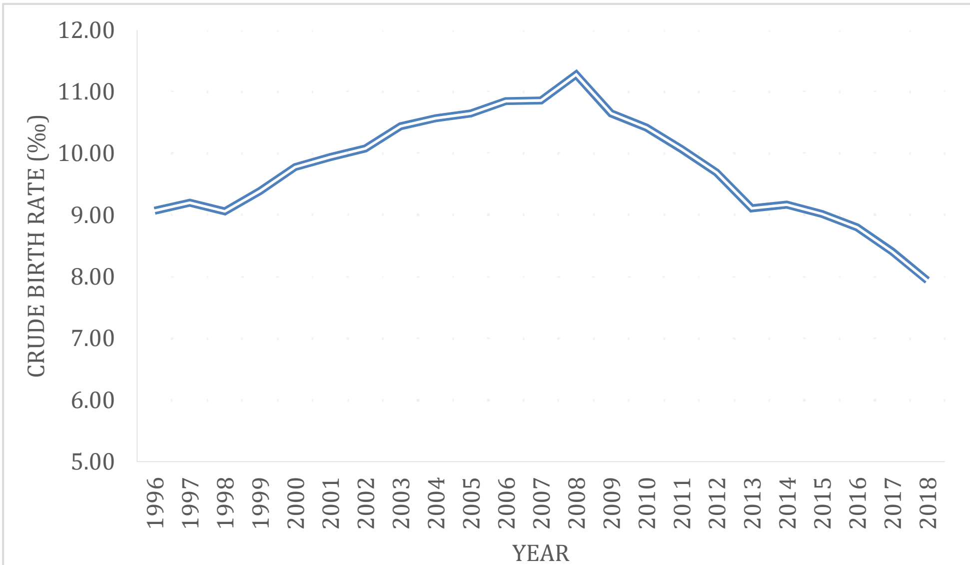
Table 6. Comparison to Previous Birth and Infant Health Outcomes in the Literature.

	(1) Mortality	(2) Birthweight	(3) Low Birthweight	(4) Very low Birthweight	(5) Gestation Weeks	(6) Pre-term Birth	(7) Early Pre-term Birth
Panel A: Our estimates from Column 1 in Table 4							
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: Over 35 indicator in our data							
Over 35 indicator	0.00165***	-44.05***	0.0258***	0.00463***	-0.211***	0.0191***	0.00779***
	(9.28e-05)	(0.695)	(0.000371)	(0.000138)	(0.00279)	(0.000372)	(0.000199)
Estimated percent change	37.76%	-1.38%	34.91%	54.15%	-0.54%	27.44%	46.09%
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 E: 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.

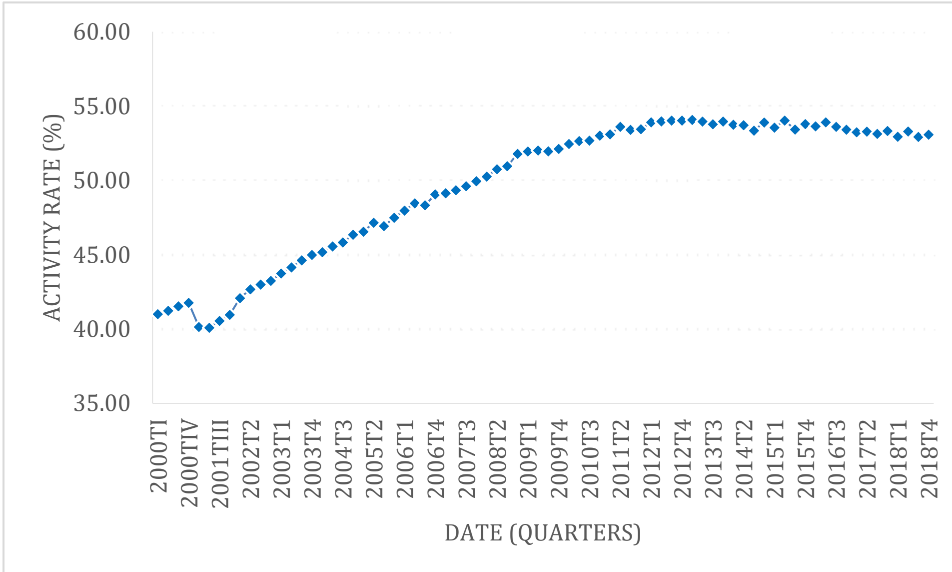
Appendix

Figure A.1 Crude birth rate, Spain, 1996-2018



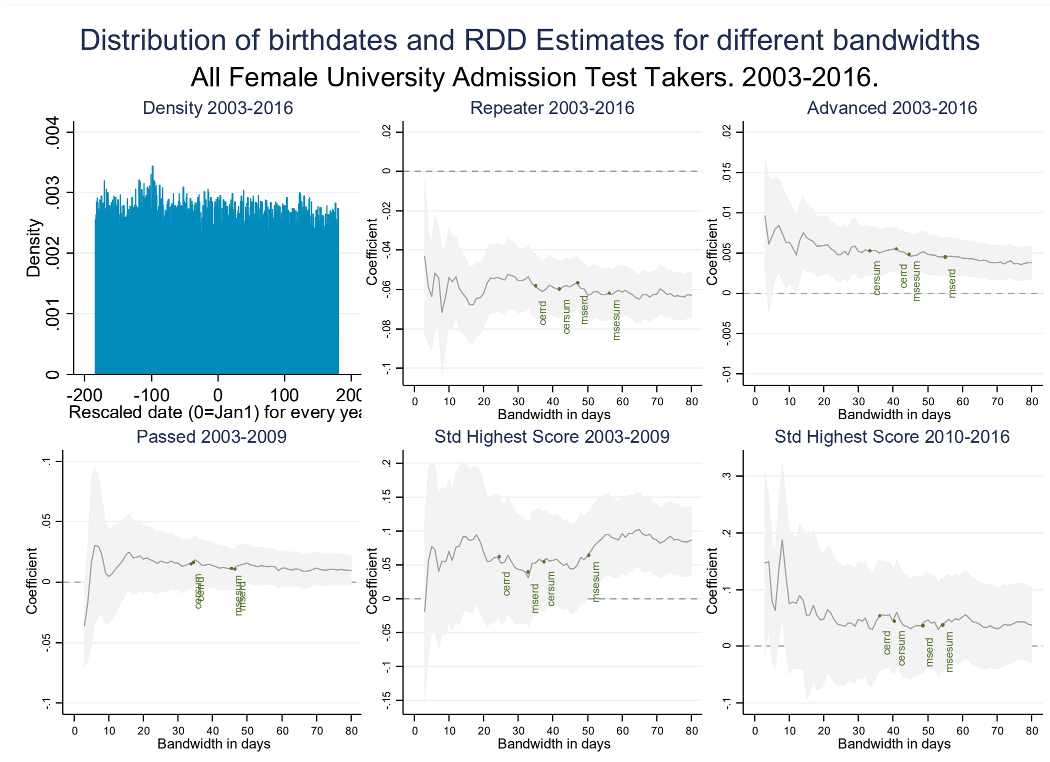
Source: Spanish Vital Statistics, Spanish National Statistical Institute, 1996-2018.

Figure A.2 Female activity rate, Spain, 2000-2018.



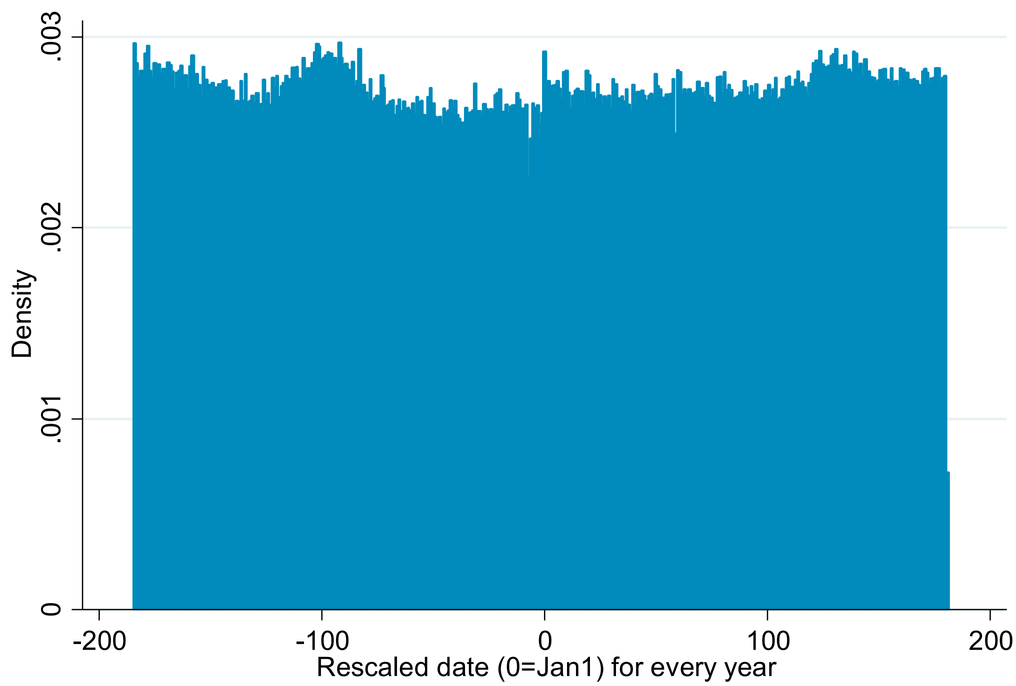
Data source: EPA data, Spanish National Statistical Institute, 2000-2018.

Figure A.3. 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



Source: Andalusian University Admissions Data. 2003-2016.

Figure A.4. Distribution of mothers' birth dates



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

Table A.1. Descriptive Statistics for Supplemental Datasets

	Obs.	Average	Stdev.	Mean
Panel A. Vital Statistics. 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 (grams)	425264	3248.8	480.26	3250
Premature birth	515073	0.038	0.191	0
Mother's age at birth (months)	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. EPA Sample 2000-2018. Women Aged 16-45				
Treat	550929	0.501	0.500	1
Primary or Less	550929	0.082	0.275	0
Secondary	550929	0.563	0.496	1
University	550929	0.355	0.479	0
Age at First Birth (months)	257762	327.17	61.99	329
Married	550929	0.441	0.497	0
Partnered	550929	0.508	0.500	1
Panel C. University Admissions Data 2000-2019. 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

Data source: EPA microdata, Spanish National Statistical Institute, 2000-2018.
Notes: Sample includes all Spanish women 25-44 years old.

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. Impact of Being Born After the Cutoff on Selection into Motherhood and Fertility. EPA 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: EPA 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.4. Power Calculations. Vital Statistics. 1996-2018

	(1)	(2)	(3)	(4)	(5)
Panel A. Mortality (tau=0.00022)					
Power	0.050	0.050	0.050	0.050	0.050
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	0.780	0.688	0.643	0.408	0.408
Panel D. Very Low Birthweight (tau= 0.00044)					
Power	0.128	0.110	0.154	0.126	0.118
Panel E. Gestation weeks (tau=1.957)					
Power	1.000	1.000	1.000	1.000	1.000
Panel F. Pre-term birth (tau=0 .0035)					
Power	0.647	0.564	0.600	0.482	0.401
Panel G. Early pre-term birth (tau= 0.00086)					
Power	0.121	0.104	0.117	0.116	0.099
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 4 for hypothesized RD treatment effects (tau) of 2% of the corresponding dependent variable mean. The sample includes all first mothers born in December and January of the following year.