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Women's education, fertility and children' health during a gender equalization process: Evidence from a child labor reform in Spain[☆]

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ABSTRACT

We study the effect of women's education on fertility and children's health during a period of gender equalization and women's greater access to economic opportunities. In 1980, Spain raised the minimum working age from 14 to 16, while the compulsory education age remained at 14. This reform changed the within-cohort incentives to remain in the educational system. Using a difference-in-differences approach, we find that the reform delayed fertility but did not impact the completed fertility of affected women. We also show that the reform was detrimental to the health of the children of affected mothers at delivery. We document two channels for this adverse effect: the postponement in the entrance of motherhood and the deterioration of mothers' health habits (such as smoking and drinking). However, in the medium run, these more educated mothers reverse the adverse health shocks at birth through maternal vigilance and investment in their children's health habits.

1. Introduction

Women's education is generally considered a key determinant of fertility and children's health. Education may affect fertility and infant health through different channels. More educated women have higher permanent incomes, which increases their opportunity cost of time, prompting them to have fewer children of higher quality (Becker and Lewis, 1973; Willis, 1973). More educated women

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may also have more information about fertility options (contraceptives) and adopt healthier pregnancy behaviors (Grossman, 1972; Currie and Moretti, 2003). Finally, greater maternal education could potentially lead to greater healthcare utilization.

Previous literature has extensively documented the association between women's education, fertility and infant health. However, the causal relationship is still a subject of debate. While many papers find that compulsory schooling has a postponement effect away from teenage years (Black et al., 2008; Silles, 2011; Geruso and Royer, 2018), the effect of education on completed fertility is still unclear. While some papers argue that education does not reduce completed fertility (Monstad et al., 2008; Fort, 2007; McCrary and Royer, 2011), others find otherwise (Cygan-Rehm and Maeder, 2013; Fort et al., 2016; León, 2006). Mothers' education also seems to either improve infant health at the moment of delivery (Behrman and Rosenzweig, 2002; Currie and Moretti, 2003; Chou et al., 2010; Güneş, 2015) or do not have any effect at all (Lindeboom et al., 2009; McCrary and Royer, 2011). The mixed evidence in the literature suggests that the effect of education on fertility and children's health may not be universal and depend on the channels through which the effect works.

In this paper, we contribute to this literature by examining the effect of female education on fertility and children's health during a time of increasing gender equality and women's greater access to economic opportunities. This will allow us to analyze another important channel through which education can affect fertility and children's health: women's empowerment and autonomy.

We take advantage of a quasi-natural experiment. In 1980, a new child labor regulation was enacted, which increased the minimum legal age to work in Spain from 14 to 16 years old, while the compulsory schooling age was maintained at 14. This reform changed the within-cohort incentives to remain in the educational system.

The academic year in Spain starts in September and ends in June. Moreover, all children from the same cohort start school in the same year. Consequently, children born at the beginning of the year start school at an older age (in months) than those born at the end of the year. Before the reform, both the school leaving age and the minimum working age were set at 14 years old. Therefore, individuals born at the beginning of the year reached the minimum legal working age of 14 before finishing their last year of primary education. These individuals would have incentives to leave school to work before completing primary education. On the other hand, individuals born during the last months would have incentives to finish the last year of primary education, as they were not old enough to work legally before that. In 1980, this difference in incentives between those born at the beginning and the end of the year disappeared. We exploit this difference in incentives affecting individuals born at the beginning and the end of the year, before and after the reform, using a difference-in-differences approach. In other words, we examine whether post-reform outcomes of mothers who were born in spring months are from those of mothers born in fall months. This approach will also allow us to isolate the effect of education from any concurrent social or political events or trends, as it should impact equally individuals born in the same year independently of their month of birth.

This reform took place in 1980, just a few years after the end of Franco's dictatorship, which lasted almost 40 years. During the dictatorship, Spain was a male-dominated society where women's rights were greatly ignored or suppressed. For instance, women needed permission to work from their fathers or husbands until 1975, laws against adultery were only abolished in 1978, divorce was legalized in 1981, and the first depanelization of abortion was approved in 1985. In this environment, very few women had access to higher education, and women's labor market participation rates were low (see Fig. 1). The end of the dictatorship increased gender equality and improved women's access to economic opportunities (Philips, 2010). An important unintended effect of this gender equalization process is that smoking or drinking became acceptable and adopted first by the most successful women (those with a higher level of education) as a symbol of independence (Amos and Haglund, 2000).

The paper by Del Rey et al. (2018) analyzes the impact of the same child labor reform on education and labor market outcomes. They show that the reform effectively increased the educational attainment of both men and women. For convenience, we reproduced their results in Table A1 of the Appendix. As we can observe, they find that the reform reduced the number of early school leavers (individuals not finishing primary education) by 1.2 percentage points (or 5.2%) in the case of men and by 1.4 percentage points (or 6.1%) in the case of women. They also find a positive effect on the probability of attaining post-compulsory education. The reform decreased by 1.6 percentage points (or 3%) for men and 1.2 percentage points (or 2.3%) for women, the number of individuals that do not attain any level of post-compulsory education. Affected women did not have a higher probability of attending college, while the reform increased by 1.2 percentage points the probability of having a university degree for men.

This paper focuses on the reform's effect on family behavior outcomes and children's health. The reform prompted a postponement of first births by one month on average. However, we show that a catching-up effect follows this postponement, and the reform did not affect completed fertility. More interestingly, we show that the reform was detrimental to the health of their offspring at the moment of delivery. We find that, for affected mothers, the reform increased the probability of having a first child with less than 37 gestational weeks by 0.209 percentage points (0.23%). Moreover, these mothers had children that weighed, on average, 4.15 fewer grams at birth after the reform.

We propose four different channels through which the reform could potentially negatively impact infant health. The first is the postponement of the age at which women have their first child. The reform increased women's probability of having a first child after the age of 35[1] and the incidence of multiple births.[2] The second channel operates through changes in maternal marital status, increasing the number of children without registered fathers.[3] Yet, the detrimental effect on infant health does not disappear when

[1] Pregnant women over 35 have a higher risk of pregnancy complications and poor infant health outcomes (Ziadeh, 2002; Astolfi and Zonta, 2002). Delayed childbearing is correlated with an increased risk of low birthweight (Tough et al., 1999; Aldous and Edmonson, 1993), stillbirths and unexplained fetal death (Fretts, 2001; Reddy et al., 2006), preterm delivery (Roberts et al., 1994), and multiple births (Tough et al., 2002).

[2] Multiple-birth children usually have worse infant health outcomes at the moment of delivery than single-birth children.

[3] Previous literature has proven that single mothers have a higher probability of having children with worse infant health outcomes at the moment of delivery.

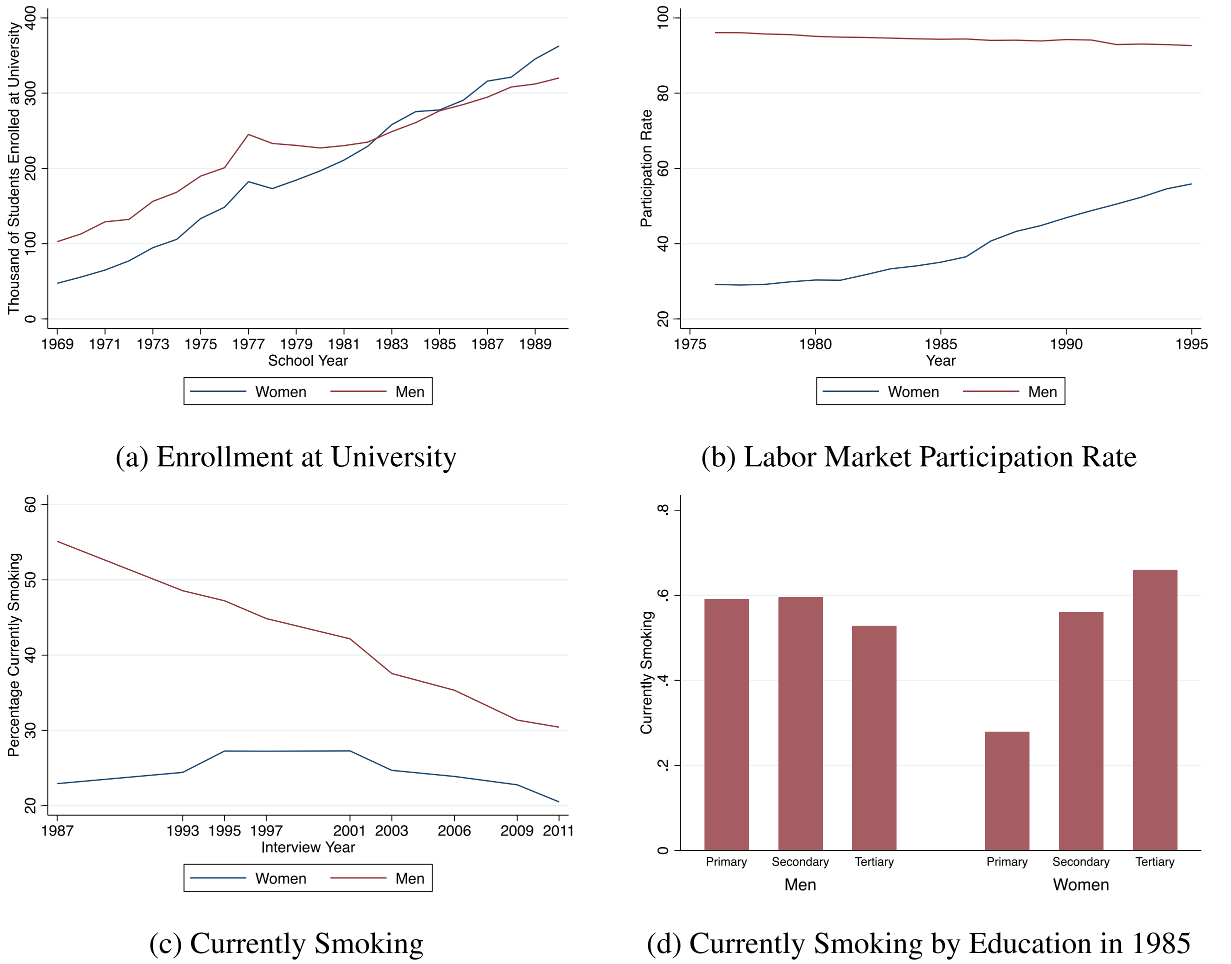


Fig. 1. Gender Differences.
Notes: We plot (a) the number female and male students enrolled at University between 1969 and 1990, (b) the labor market participation rate for men and women aged 25 to 54 between 1976 and 1995, (c) the percentage of women and men currently smoking between 1987 and 2011, and (d) the percentage of women and men currently smoking in 1985.
Source: (a) Statistics on Education in Spain (1969–1990) from the Ministry of Education and Vocational Training, (b) Spanish Labor Force Survey (1976–1995), (c) Spanish National Health Survey (1987, 1993, 1995, 1997, 2001, 2003, 2006 and 2012), and (d) Survey on attitudes and opinions of Spanish regarding Tobacco, Alcohol and Drugs (1985).

we control for the father's presence at the moment of delivery, which indicates that maternal marital status is not a major mechanism. A third potential mechanism is an increase in employment and the change of occupations of affected mothers due to the reform. If affected mothers worked more during pregnancy (especially in high-skilled occupations), they were potentially exposed to more stressful environments, specific occupation conditions, and less leisure time during pregnancy, negatively impacting their children's health at birth. However, this result must be taken with caution as we cannot directly examine the reform's effect on women's employment during pregnancy. The fourth channel we propose is the change in unhealthy habits of affected mothers, which could have contributed to the reported adverse effects if maintained during pregnancy.[4] More precisely, we find that the reform increased the probability of smoking and alcohol consumption for treated mothers. This last channel is the interaction between the acquisition of more education and the gender equalization that women were experiencing at the moment of the reform. More educated women undertook more unhealthy behaviors as a sign of liberalization, despite the health cost for them and their children.

[4] In a previous paper, Bellés-Obrero et al. (2021) examine the effect of this reform on mortality. They found that the reform decreased mortality at young ages (14–29) by 6.3% among men and by 8.9% among women. However, the child labor reform increased mortality for prime-age women (30–45) by 6.3%. This effect is driven by increases in HIV mortality (11.6%) and diseases of the nervous and circulatory systems (8.7%). They explain this last result by showing that women's health habits deteriorated, which increased the incidence of habit-related diseases and ultimately led to higher mortality rates. This paper finds that this deterioration of health habits also applies to the affected women who became mothers.

When we analyze the reform's effect on men's fertility decisions and infant health outcomes of their children, we find a similar postponement effect on fertility to the one observed for women. However, the reform did not affect the infant health outcomes of affected men's children. This important result is consistent with the theory that the mechanism through the child labor reform affects infant health relates to the mother's characteristics or behaviors during pregnancy. This result reinforces our finding that delayed childbearing and bad behaviors during pregnancy (such as smoking) are essential to explain the negative effect of the reform on infant health.

The size of the effects that we find on birth outcomes, as well as the established links between health at birth and long-term health (Figlio et al., 2014; Fletcher et al., 2010; Smith, 2009), would suggest that the deterioration of infant health at birth that we find would persist in the medium and long term and would affect children's health unless there is a compensation mechanism. Yet, in the medium run, we find that the effects of the reform on objective health outcomes are insignificant. Thus, educated mothers can reverse adverse shocks at birth. Our data suggest that long-term reversals are achieved through maternal vigilance and higher investment in children's health habits. Children of treated mothers with higher education are perceived as having worse health, even at older ages. Their objective health status is, however, better. This suggests more concerned mothers. These children are also more likely to have private health insurance. This latter trait is significant. In Spain, private health insurance is purchased in addition to universal public health coverage. This double coverage allows beneficiaries to avoid the system gatekeeper and, hence, to have quicker access to specialists and additional tests and checkups.

This paper contributes to the previous literature in several ways. First, it contributes to the discussion about the link between education, fertility, and infant health outcomes in middle-income countries. Previous evidence on the causality between education, fertility and infant health has largely focused either on fully developed countries or countries with a very low level of development. For instance, previous studies have exploited several reforms in compulsory schooling in the US (Black et al., 2008; León, 2006), Norway (Monstad et al., 2008), Italy (Fort, 2007), the UK (Silles, 2011; Lindeboom et al., 2009; Geruso and Royer, 2018), Germany (Cygan-Rehm and Maeder, 2013), and Europe (Fort et al., 2016). While other papers have studied the link between education and family behavior outcomes in Indonesia (Breierova and Duflo, 2004), Nigeria (Osili and Long, 2008), Taiwan (Chou et al., 2010), Kenya (Duflo et al., 2015), Zimbabwe (Grépin and Bharadwaj, 2015), or Malawi (Makate and Makate, 2016). However, the reform we are exploiting in this paper occurred when Spain was a middle-income country experiencing a gender equalization process. The closest papers to ours examine the effect of female education in Turkey (Dinçer et al., 2014; Güneş, 2015), yet, the cultural norms and social environment of Turkey at the time of the reform exploited in these papers (with strong discrimination against women) were extremely different from the Spanish context during the 1980's reform. Therefore, the main mechanisms behind our results will be very different.

Secondly, our identification strategy allows us to estimate the reform's within-cohort effects, where our treated individuals and their control counterparts differ only in their birth month. Consequently, our identification strategy will be robust to any concurrent social or political events, as these will have the same impact on both our treatment and control groups. Moreover, as we use a difference-in-differences estimator, we do not need to assume that individuals born in different months are equal. Given that in Spain, all children from the same cohort start school the same year, children born at the beginning of the year start school at an older age (in months) than those born at the end of the year. There is vast literature showing that relative school starting age matters. Exploring cut-off entry dates in kindergarten, several papers (Elder and Lubotsky, 2009; Lubotsky and Kaestner, 2016; Cascio and Schanzenbach, 2016) find older kindergarten children perform better in reading and math tests. Even though the effects remain for older children (Attar and Cohen-Zada, 2018; Foureaux Koppensteiner, 2018), this advantage seems to dissipate when children progress through school (Black et al., 2011; Peña, 2017). In our difference-in-differences approach, our results will not capture the effect of relative school starting age, as we assume that these effects remain constant for the cohorts before and after the reform.

Thirdly, as far as we are aware, this is the first paper to investigate the effect of education on fertility and children's health using a child labor regulation. A large part of the literature has used changes in the state's compulsory schooling laws as an instrument for years of education. On the other hand, other studies have examined the effect of both child labor laws and compulsory schooling laws on short-term outcomes such as educational attainment and child labor (Goldin and Katz, 2011; Lleras-Muney, 2002; Edmonds and Shrestha, 2012).[5] Child labor reforms differ from compulsory schooling reforms in many aspects. For one, the individuals affected by both reforms could be similar but might not completely overlap. Compulsory schooling reforms will force children to stay in the educational system, increasing educational attainment across the board (if correctly applied). The compliers of this reform will be all children who would have dropped out earlier if they would have had the opportunity either because of their low ability, lack of interest in studying, or/and the need/preference to work, among others. Probably the majority of these compliers have low socioeconomic background. On the other hand, child labor reforms will only act as a subtle incentive to continue studying. This type of reform does not allow individuals to start working but does not require them to stay in the educational system. Therefore, after a child labor reform, students dropping out because of their low ability or lack of interest in education could still leave school early. Only those dropping out because they have a preference/need to work will be really incentivized to continue studying. These

[5] Lleras-Muney (2002) and Goldin and Katz (2011) examine the effects that compulsory schooling and child labor laws from 1910 to 1939 have on educational attainment in the US. While Lleras-Muney (2002) finds that legislation increased the educational attainment of individuals at the lowest percentile in the distribution of education, Goldin and Katz (2011) reports that the reform has only a positive but modest impact on secondary schooling rates. Edmonds and Shrestha (2012) analyze the effect of a statutory minimum school-leaving age on child labor and schooling in 59 mostly low-income countries. However, they find that minimum age regulations are barely enforced in such countries. It is important to note that child labor in low-income countries might be vital for family subsistence. If this is the case, child labor regulations might simply divert children from formal jobs to informal jobs without reducing their employment rate.

students, though, will also have been compliers under the compulsory schooling law and probably share the same low socioeconomic background. Therefore, the possibility that the compliers of these two types of reforms do not entirely overlap should be considered when interpreting the results.

Fourthly, unlike most extant literature, we use registered data of all births in Spain, which allows us to observe the universe of all births for more than 30 years. These data will allow us to examine completed fertility (instead of focusing on teenage fertility as in most of the previous literature) and infant health outcomes at the moment of delivery for women having children at all age ranges. Moreover, administrative data have some advantages over census data, which only identify a woman's child as those living in the same household at the time of the interview. Divorce, the death of the mother, or the emancipation of older children can impact this number. If the level of education affects the probability that some of these situations occur, then census data could bias the results.

The remainder of the paper is organized as follows. Section 2 presents the institutional context. In Section 3, we discuss the identification strategy. After describing the data we use in Section 4, we present the main results of our estimation in Section 5. Section 6 presents the robustness checks. Section 7 shows the main explanatory mechanisms behind our main results. In Section 8, we present evidence on the medium-term effects of the reform on children. Section 9 concludes.

2. Institutional context of Spain before 1980

In 1980, just a few years after the end of the dictatorial regime in Spain, a new child labor regulation (Law 8/1980) was enacted, which changed the minimum legal age to work from 14 to 16 years old. Before the reform, Spain was characterized by having a considerable percentage of its population participating in the labor market at an early age. In the late 1970s, formal and informal labor market participation was quite high among 15 and 14-year-old boys and girls. We observe in the Labor Force Survey that 40% and 30% of 15-year-old boys and girls were participating in the labor market, and 30% and 20% of them had a formal job. At 14, 10% and 8% of boys and girls were employed. Using Social Security data, we see a radical elimination of formal work for children before the age of 16 after the reform. The cohorts of individuals born between 1961 and 1965 (that turned 14 before the reform) have a probability of contributing to the Social Security system before the age of 16 of 9.22% for boys and 7.57% for girls. On the other hand, for individuals born between 1967 and 1971 (that turned 14 after the reform) these numbers dropped to 0.17% for both boys and girls.

It is empirically more difficult to determine how the reform affected the informal employment of children, as the Spanish Labor Force Survey (which usually contains information about formal and informal employment) stopped reporting the labor force status of children below 16 from the second quarter of 1980, as it was illegal to work at these ages. However, we use the Spanish Budget Survey of 1980–1981, which collects informal and formal labor market participation of children from 14 to 16 years old right after the reform; from April 1980 to March 1981 (see Alonso-Colmenares et al., 1999, for a description). We can see that, in this survey, 9.83% of 15-year-old boys and 5.1% of 15-year-old girls were participating in the labor market. These numbers are much lower than those reported by the Labor Force Survey just before the reform (31% of 15-year-old boys and 19% of 15-year-old girls in the first quarter of 1980). This gives suggestive evidence that the informal market was strongly also reduced (although not entirely eliminated) right after the reform.

The Spanish educational system was regulated at that time by the General Law of Education of 1970 (*Ley General de Educación*) that was in force until 1990. There were four levels of education: three years of preschool, eight years of primary education, four years of secondary education, and tertiary education. This law established compulsory education until 14, which remained constant before and after the child labor reform. There was no requirement to complete a specific level of education before individuals could abandon the educational system. As soon as they turn 14, normally during the last year of primary education, they could legally drop out of the educational system. In Spain, all children from the same cohort start school the calendar year they turn six years old. Consequently, some children were five years old when they started primary school, while others started at six years old and are thus a bit older (in months). At the same time, some children finish the last year of primary education at 13, while others finish it at 14 years old. Spain had very low levels of educational attainment before 1980, with 28% of the women dropping out of school before or at the age of 14 (9% before they were 14 years old), almost 12.7% not finishing primary education, and 43.8% of them not finishing secondary education.[6]

Finally, Spain was experiencing a gender equalization process at that time. The level of social development for individuals born between 1940 and 1960 was different according to gender. During the dictatorship regime, Spain was a male-dominated society where women were granted very few rights. This meant that very few women had access to higher education. Fig. 1(a) shows the number of male and female students enrolled at University from 1969 to 1990, and we observe how before 1978, the number of men enrolled at University was much larger than the number of women. Women's participation in the labor market was also very low. In Fig. 1(b), we can observe that in 1976 only 29% of working-age women (25–54 years old) were participating in the labor market in Spain. This percentage increased slowly but was still very low during the 80 s (34.5% in 1985). The end of the dictatorship increased gender equality and improved women's access to economic opportunities (Philips, 2010). We observe in Fig. 1 how we already have more women enrolled at University than men in the late 80 s and early 90 s. The labor market participation

[6] These percentages are calculated from the Spanish Labor Market Survey of 1995 to 2016 for the cohorts of women born in 1965 (the last cohort not affected by the reform). Note that only 21% of women that dropped out from school before the age of 14 did it without finishing primary education. The rest of these women were probably 13 during the last year of primary education and were allowed to drop out of the system after finishing that education level. Therefore, there was not a huge lack of enforcement on the minimum school leaving age at that time in Spain.

rate also significantly increased for women in the early 90 s. However, this gender equalization process led to a convergence of health risk factors between men and more independent women (e.g., smoking, drinking, taking drugs, and sexual promiscuity). Fig. 1(c) shows how smoking rates among men decreased over time between 1987 and 2011, while women's even increased during the 90 s. More importantly, before 1980, more educated women had a larger smoking prevalence than women with fewer years of education (Bilal et al., 2015). Fig. 1(d) shows the percentage of men and women currently smoking in 1985 by the level of education. We observe that while, for men, the smoking incidence is slightly decreasing with education, more educated women smoke more than less educated ones. As women had the opportunity of entering the labor force and had access to better economic opportunities, smoking or drinking became acceptable and adopted first by the most successful women (those more educated) as a symbol of independence (Amos and Haglund, 2000). Therefore, it is not surprising to see that during the gender equalization process, more educated women were undertaking more unhealthy behaviors, despite their health costs. This positive correlation between unhealthy behaviors and education for Spanish women is gradually reversed until the cohorts of women born after 1980, which mirrors that of developed countries, with less-educated women showing the highest smoking and drinking prevalence rates.

3. Identification strategy

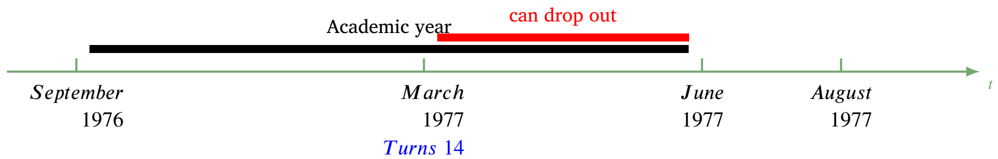
The Law 8/1980 "Estatuto de los Trabajadores" (ET), introduced in March 1980, increased the minimum legal working age from 14 to 16. This child labor reform introduced an exogenous variation in the incentive to stay in the educational system depending on the individual's year and month of birth.

For the cohorts of individuals born before 1966 (that turned 14 before 1980), the age of compulsory schooling and the legal age of working was the same: 14 years old. In Spain, all children from the same cohort start school in the same year. Consequently, children born at the beginning of the year start and end school at an older age (in months) than those born at the end of the year. Therefore, individuals born before 1966 during the first months of the year could start working (at age 14) during their last year of primary school without completing this level. However, individuals from these cohorts born during the last months of the year were still 13 during their last year of primary school, so they could not start working before finishing the last year of primary education. Therefore, we expect that those born during the first months of the year have less educational attainment than individuals born in the same year but at the end of the year.

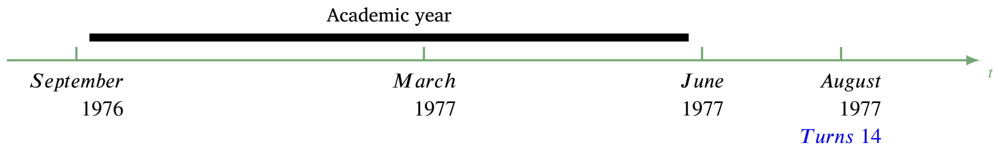
In 1980, with the Child Labor Reform, the age of compulsory schooling (14) did not coincide anymore with the legal age of working (now 16). All individuals born after 1966 (that turn 14 after 1980), independently of their birth month, have the same incentives to finish primary education, as they cannot start working until 16.

The following chart illustrates the incentives for staying in the education system during the last year of primary school for all individuals born before 1966, depending on their birth month. We take as an example two individuals born in 1963 but only six months apart:

(1) An individual that was born in March of 1963:



(2) For an individual that was born in September of 1963:



The individual born in March of 1963 would have turned 14 years old in March of 1977 and could have dropped out of school before completing the last year of primary education, which finished in June of 1977. While the individual born in September 1963 was still 13 in June of 1977 when the last year of primary school finished.

All individuals born after 1966, independent of their month of birth, will finish primary education before they can start working (now they are only allowed at age 16). Therefore, all individuals will have the same incentive to finish primary education.

We perform a within-cohort difference-in-difference strategy[7] to identify the reform's effect on women's fertility decisions and the (short-term and medium-term) health of the affected women's offspring. In our identification, we compare women of the same cohort that only differ in the month of birth. It is important to note that the month of birth threshold used for the identification (being born before or after the summer) does not coincide with the cutoff for school enrollment in Spain (being born before or after

[7] Note that the designs proposed by Sun and Abraham (2021), Callaway and Sant'Anna (2021), or Borusyak et al. (2021) are not appropriate in our setting. These new methodologies are have been designed for differences-in-difference approaches where the treatment varies across units. Our paper uses a difference-in-differences strategy without variation in our treatment timing.

January; individuals born in the same year start school together). Therefore, we will capture only the reform's within-cohort effects, and our results will be robust to any concurrent social or political events or trends. Any other concurrent event should impact equally individuals born in the same year independently of their month of birth.[8]This is important as this reform was approved during a period of significant social change in Spain.

In our difference-in-differences strategy, all individuals born between August and October are in the control group. Regardless of whether they were born before or after 1966, these individuals always had incentives to finish primary education, as they were always below the legal working age during their last academic year of primary education. On the other hand, individuals born between March and May are in our treatment group. For this group, the incentives to finish primary education depend on whether they were born before or after 1966. If they were born before 1966, they could start working before finishing the last year of primary education, while if they were born after 1966, they could no longer do so.[9]

First, we consider the following econometric model for the different fertility and health behavior outcomes of woman i born in month m and year y observed in year t:

$$Outcome^{i}_{myt} = \alpha + \beta_1 Treated^{i} + \beta_2 Treated^{i} * Post\ Reform^{i} + \delta_m + \mu_y + \theta_t + \epsilon^{i}_{myt}$$

where $Treated^{i}$ is a dummy variable that equals one if the woman was born between March and May and zero if she was born between August and October.[10] $Post\ Reform^{i}$ is also a dummy variable that takes a value of one if the woman turned 14 after the reform and zero otherwise. We define pre-Reform cohorts as those born from 1961 to 1965 and post-Reform cohorts as those born between 1967 and 1971. We control for the woman's month and year of birth dummies and calendar year fixed effects (δ_m, μ_y, and θ_t, respectively). We cluster the standard errors at the cohort level and report the wild bootstrap p-values in brackets. The reform's effect can be identified by the coefficient of the interaction between the post-reform and the treatment dummy variables, β_2.

When examining the effects of the reform on their children's health outcomes at the moment of delivery, we use the same econometric model but set at the level of their first child j, born in year t and month n:

$$Outcome^{ji}_{mytn} = \alpha + \beta_1 Treated^{i} + \beta_2 Treated^{i} * Post\ Reform^{i} + \delta_m + \mu_y + \theta_t + \gamma_n + \epsilon^{i}_{mytn}$$

where we also add to the specification the child's month and year of birth fixed effects (θ_t and γ_n respectively). When examining the medium-term effects of the reform on the children, instead of adding the child's month and year of birth, we control for the children's age at the moment of the interview, adding both a linear and a quadratic terms.

Note that we assume that the reform did not affect the cohort of individuals between 14 and 16 years old when the reform passed (individuals born in 1964, 1965, and 1966). In other words, we assume that when the reform was enacted, these individuals who could have been working before the reform had to quit their jobs and return to the educational system. We are aware that this is a strong assumption, so we relax it in Section 6.

Given the significant social change that Spain was experiencing in the 80 s, we cannot exploit the before–after effect of the reform (which could potentially be stronger than our within-cohort comparison) using a regression discontinuity design. The Spanish democratization process started in 1979, and many reforms passed quickly after that. For instance, divorce was legalized in 1981. In 1978 the commercialization and use of contraceptives was decriminalized for the first time in Spain. Moreover, in 1985 abortion was decriminalized under one of three circumstances: rape, risk to the mother's physical or mental health, and fetus's malformation. Finally, in 1984 temporary contracts were liberalized to promote employment and its use dramatically increased very rapidly. Consequently, the cohorts of women that turned 14 years old before and after the reform were exposed to different environments. There is also a second important reason why we cannot implement this alternative strategy. The reform did not sharply affect a specific cohort of individuals. The reform was introduced in March of 1980. Therefore, it fully affected all women born after March of 1966. They were below the age of 14 when the reform was passed and, with the reform in place, they could not start working until they turned 16 years old. At the same time, all women born before April 1963 were already 16 when the reform took place and were completely unaffected by the reform. However, women born between March 1964 and February 1966 were between 14 and 16 years old when the reform was enacted and could have been partially affected by it. Hence, our strategy is much more conservative as we only exploit the within-cohort variation but, in this setting, the identification strategy is much more reliable than a before–after modeling approach.

4. Data and descriptive statistics

In order to examine the effect of the reform on the affected women's fertility and their offspring's health outcomes at the moment of delivery, we use administrative data from the birth certificate records. Thus, we have the universe of children born in Spain

[8] Our identification will not be robust, though, to any shock that differently affects individuals born before and after summer in the reform year. However, it is difficult to think what type of shock will impact individuals born in the same year differently.

[9] A similar strategy is used by Schulkind and Sandler (2019) that compares the outcomes of women who had a child near the end of their senior year of high school with those who have a child just after the (expected) end of high school.

[10] Note that we are excluding women born in the first two months and the last two months of the year, as they are potentially the most different ones. In Section 6, we explore our main results comparing women born between January and May with those born between July and December. We also leave out from the analysis individuals born in June and July because final exams usually take place during these months in Spain, although the exact date of these examinations varies at the school level (within these two months). However, our results are robust to the inclusion of women born in June in the treatment group and women born in July in the control group. The results can be obtained upon request.

between 1975 and 2018. The data is available from the Spanish National Statistics Institute and contains information about the parents and the newborn self-reported by parents or relatives compelled by law to declare the childbirth. The raw microdata contains 20,199,495 births. We restrict our sample to births of Spanish women born between 1961 and 1971 that were 14 to 47 years old at the moment of delivery.[11] We also drop births of women born in 1966 who turned 14 the year the reform took place (1980) and those born in January, February, June, July, November, and December. Thus, finally, we observe 2,527,415 births or 1,393,937 first births in our sample. As fertility outcomes, we first look at the number of first births delivered in a particular year from women born in a specific month and year by every 1000 women born in that same month and year. Similarly, we also show the total number of births delivered in a certain year from women born in a specific month and year by every 1000 women born in that same month and year. We also look at the age at which women had their first child. We report the descriptive statistics of the fertility variables used in Table A2. We observe that there are, on average, 844 first births and 1530 total births per 1000 women in our sample. Moreover, Spanish women born between 1961 and 1971 had their first child at almost 28 years old, on average.

We measure children's health at the moment of delivery using four measures: the probability that the child survives the first 24 h, the probability that the child is born after more than 37 gestational weeks,[12] birth weight (in grams) at the moment of delivery, and the probability of being born weighing less than 2500 g.[13] Data on birth weight and survival within the first 24 h are only available from 1980 to 2018. Thus, when analyzing these outcomes, we drop the 1961 cohort from the pre-reform group and restrict the sample to all births that took place when the mother was between the ages of 18 and 47.[14] It should also be noted that the birth weight is missing from 11 percent of all registered first births. However, as it can be observed from Table A3, the reform does not impact the probability of not having information on birth weight. Moreover, we analyze if the reform had any effect on the probability that women had first multiple births and the sex ratio (the probability of having a male first birth).[15] We only examine the health of the woman's first child. We include this restriction because a poor health outcome for the first birth can influence the decision to have a second child, as pointed out by Wolpin (1993).

In Table A2, we can observe that 51% of the first births in our sample are male. Around 3.3% of the reported first births in the sample are multiple births, and 99.8% of the observed first births survive the first 24 h after delivery. Moreover, around 90% of children are born with 37 or more weeks of gestation, children are born on average with 3192 g, and 7.4% of them are born with low birth weight.

We also examine the reform's effect on women's (and mothers') employment prospects between 1999 and 2019 using the Spanish Labor Force Survey (LFS). The LFS is a continuous quarterly survey that contains information related to the labor market, active unemployment, and inactivity of the population living in a family dwelling in Spain. This database has been available since 1964; however, in this paper, we use this database from 1999 to 2019, as the month of birth was not specified before. We again only consider Spanish women born in March–May or August–October of 1961–1965 or 1967–1971. In our final sample, we observe 524,034 women (424,044 with at least one child). We use this database to assess the reform's effect on women's or mothers' labor outcomes. In Table A4, we observe that, on average, 60% of women in our sample are employed, 12% are unemployed, and 27% are inactive. Women with children have 2 percentage points more probability of being inactive and not employed. Regarding occupations, 15% of women have a high-skill job, 33% a semi-skilled job, and 11% a low-skill job. Mothers have similar characteristics, with a slightly higher probability of having low-skilled jobs.

To examine women's risky behaviors, we use three waves (2003, 2006, and 2012) of the Spanish National Health Survey. This is a nationwide cross-sectional survey that collects health-related information and the socio-economic status and habits of adults and children (up to 15 years old). The raw microdata contains 59,296 individuals. We again restrict the sample to Spanish women born in March–May or August–October of 1961–1965 or 1967–1971.[16] In our final sample, we observe a total of 3102 women. We use this database to assess the reform's effect on some health behavior outcomes: smoking and drinking alcohol. In Table A5, we observe that, on average, 77% of women drink alcohol, although only 5.5% drink alcohol daily. Moreover, 40% have never smoked while, among those that have ever smoked, 62% smoke at the moment of the interview. Finally, 21% of the women that quit smoking did it because of pregnancy.

We use two different databases for the medium-term effects on the health of the women's offspring. We first use three waves (2003, 2006, and 2012) of the Spanish National Health Survey. We restrict our sample to mothers whose first child[17] is between 2 and 15 years old at the moment of the interview that were born in March–May or August–October of 1961–1965 or 1967–1971. We focus on the health and habits information of the first child aged 2 to 15, which are self-reported by the mother. We also use the

[11] This age restriction allows us to include the same ages for all the cohorts considered, as women of the first cohort (1961) were 14 in the first year of the register, and women of the last cohort (1971) were 47 in the last year of the register.

[12] We select 37 gestational weeks as a threshold because babies born earlier than that are medically considered premature.

[13] Babies born with less than 2500 g are considered low birth weight by medical standards.

[14] The reform did not have an effect on the probability of women having the first child at early ages, so we are confident that we do not have a selected sample.

[15] This outcome can be considered a proxy for miscarriage, as male births are known to miscarry more often. The medical literature argues that hormones induced by stress increase the probability of spontaneous abortions at an early stage of pregnancy, and these hormones have a larger effect on male than on female fetuses (Hobel et al., 1999; Byrne et al., 1987).

[16] In the last wave (2012), the month of birth is reported in brackets. For this specific interview year, women born from January to May are considered treated women, and those born from August to December control women.

[17] We assume that the oldest child in the household is the woman's first child. Some children may not live anymore in the household or are over 18 at the moment of the interview. If this is the case, we will not be able to observe them. As a robustness check, we have also used for our sample mothers where any of their observed children are between 2 and 15 years old. We report these results in Appendix A.

Table 1
Fertility outcomes.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Number first births per 1000 women	Total number births per 1000 women	Age first birth
Treated	17.749**	37.716***	−0.060*
	(6.272)	(12.120)	(0.030)
	[0.016]	[0.007]	[0.100]
Treated* Post Reform	0.096	−6.787	0.088***
	(4.450)	(9.393)	(0.029)
	[0.988]	[0.406]	[0.010]
Mother Birth-Year FE	✓	✓	✓
Mother Birth-Month FE	✓	✓	✓
Observations	60	60	1,393,937
R^2	0.586	0.862	0.036
Dependent Variable Mean (Pre-Reform)	853.838	1579.643	26.920

Notes: The dependent variables are (1) the number of first births per 1000 women born in each year–month, (2) the total number of births per 1000 women born in each year–month, and (3) the age (in years) of the woman when she had her first child. Regressions include mother's year of birth and month of birth fixed effects. Treated individuals are those women born from March to May, and the control are those born from August to October. Women born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

extended 2000 wave of the European Community Household Panel (ECHP, see Peracchi (2002) for a description) database to study the effect of the reform on the probability of having a private (complementary to the public) insurance.[18] This is a cross-sectional database that contains detailed information on income, financial situation, working life, social relations, and health of the household members of those individuals that are being interviewed. The descriptive statistics of the main variables can be observed in Table A6. We use two main measures of children's health: the probability of having self-reported (subjective) good health and the probability that the child has good objective health (defined as not having diabetes, asthma, chronic allergies, or mental disorders). 82% of the children in our sample do not have diabetes, asthma, chronic allergies, or mental disorders, while only 35% percentage of mothers self-report that their children have good subjective health. Moreover, 4% of the children visited the hospital at least once in the last 12 months. We also report some habits of the children, such as the probability of exercising more than once a month, the number of sleeping hours, the probability of watching TV less than 3 h a day, or the probability that they follow a Mediterranean diet (eating fruit, vegetables and milk every day and legumes and fish at least three times a week). Finally, around 10% of the households with children have private insurance.

5. Effect of the reform on fertility and infant health

We first study the impact of the reform on several fertility outcomes. Table 1 shows that the reform postponed, on average, the entrance into motherhood of the affected women by one month. However, this postponement was compensated later, as the reform did not impact women's completed fertility and the total number of children each woman had. This null effect over fertility is consistent with previous studies by Black et al. (2008), Fort (2007), Monstad et al. (2008), Silles (2011), or Geruso and Royer (2018).

We next focus on the potential long-term impacts of the reform. More precisely, we study whether the health of the first children born from women affected by the reform changed after the new policy was implemented. First of all, Table 2 shows that the reform did not have any effect on the sex ratio, providing suggestive evidence that differential miscarriage is not a problem in our setting. Furthermore, we find that the reform harms the health of children born to affected women.[19]

After the reform, the first child of a woman born at the beginning of the year has a 0.209 percentage-point (0.23% with respect to the pre-reform mean) higher probability of being premature. The reform also caused women born at the beginning of the year to have children that weighed 4.15 g less, on average, compared to children of women born at the end of the year. While 4.15 g may not seem like a lot, it has to be taken into account that this is the estimated average impact of the reform. In fact, this result is of similar magnitude as the change in birth weight brought on by several US federal nutrition programs. For instance, Hoynes et al. (2011) determined that the Supplemental Program for Women, Infants, and Children in the United States led to an increase in average birth weight of around 2 g. Similarly, Almond et al. (2011) estimates that the US Food Stamp program increased the average birth weight between 2 and 5 g. We also observe that women born at the beginning of the year have a 0.18 percentage point (2.76%) higher probability of having a first child with a low birth weight (less than 2500 g). We also find that the reform increased the probability of having multiple births by 0.217 percentage points (8.2%); however, this result should be taken with caution as the parallel trend assumption for this specific outcome is not entirely fulfilled (see Section 6).

[18] The information on private health insurance is asked for the entire household, not for each individual. Therefore, we will take as our sample families affected by the reform with at least one child at the moment of the interview.

[19] Results are robust in sign and significance to the substitution of cohort time dummies by linear, quadratic, and quartic pre-and post-reform trends.

Table 2
Infant health outcomes.
Source: Birth registries (1975–2018), first children of women from cohorts 1961–1965 and 1967–1971. For birth-weight and survival, we only consider the birth registries from 1980–2018 and cohorts of women 1962–1965 and 1967–1971.

	Infant health of the woman's first child					
	Male	Multiple birth	Survives first 24 h	Weeks of gestation ≥ 37	Birth weight	Weight < 2500g
Treated	0.047	0.023	0.029	0.011	2.856	−0.215
	(0.140)	(0.098)	(0.026)	(0.067)	(2.016)	(0.125)
	[0.764]	[0.845]	[0.313]	[0.881]	[0.199]	[0.141]
Treated* Post Reform	0.157	0.217**	−0.022	−0.209***	−4.154**	0.187*
	(0.127)	(0.091)	(0.026)	(0.053)	(1.676)	(0.091)
	[0.294]	[0.044]	[0.456]	[0.001]	[0.029]	[0.095]
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓
Mother Birth-Month	✓	✓	✓	✓	✓	✓
Child Birth-Year FE	✓	✓	✓	✓	✓	✓
Child Month-Year FE	✓	✓	✓	✓	✓	✓
Observations	1,393,937	1,393,937	1,217,395	1,393,937	1,085,865	1,085,865
R^2	0.000	0.027	0.000	0.009	0.010	0.011
Dependent Variable Mean (Pre-Reform)	51.770	2.633	99.768	90.362	3217.151	6.516

Notes: The dependent variables are (1) the probability that the first birth is a boy (multiplied by 100), (2) the probability of having multiple births (multiplied by 100), (3) the probability of having a first child that survives the first 24 h after delivery (multiplied by 100), (4) the probability of having a first child with more than 37 weeks of gestation (multiplied by 100), (5) the weight at birth (in grams) of the woman's first child and, (6) the probability that the first child is born with less than 2500 g (multiplied by 100). Regressions include mother's year and month of birth fixed effects and the child's year and month of birth fixed effects. Treated children are those whose mother was born from March to May, and the control are those whose mother was born from August to October. Children whose mother were born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at mother's cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

In the Appendix, Table A7 reports the effects of the reform on all children (not restricting to the first child of the affected mothers). The effects are very similar.

Our results conflict with the scarce evidence presented in the extant literature, which finds either a positive impact of maternal education on child health (Currie and Moretti, 2003), or no causal effect (McCrary and Royer, 2011). Thus, in Section 7, we propose four potential channels through which the reform could have a negative impact on infant health.

6. Robustness checks

Before analyzing the potential mechanisms behind our main results, in this section, we perform several robustness checks of our key results.

Cohorts born in 1964, 1965, and 1966

The reform was enacted in March of 1980. All individuals born after 1966 were fully affected by the reform and could not start working until they turned 16 years old. At the same time, all women born in 1963 or before were already 16, the year the reform took place, and were completely unaffected by the reform. However, women born between March 1964 and February 1966 were between 14 and 16 years old when the reform was enacted and could have been partially affected by it.

In our main specification, we dropped women born in 1966 (who were 14 when the reform took place) and assumed that women born between 1964 and 1965 were not affected by the reform. We now examine the robustness of our results to the relaxation of this assumption. We construct a post-reform variable that reflects the possibility of some women being partially affected by the reform. All women born after March of 1966 are fully affected by the reform, and the post-reform variable takes a value of 1 for them. For women born before February 1964, the variable takes a value of 0. For women born between March 1964 and February 1966, the post-reform variable will take a value between 0 and 1 depending on how many months they had the opportunity of working before the law passed. For instance, a woman born in March of 1964 had to wait one month before they could start working, as they were one month away from turning 16 when the reform was passed. Thus for these women, the post-reform variable will take a value of 1/24 (as those fully affected had to wait 2 years or 24 months to start working when the reform was passed). In the same way, the post-reform variable will take the value of 2/24 for all women born in April 1964, and so on. We followed this rule until women that were born in February 1966 which were affected by the reform for 23 months (the variable takes the value of 23/24).

We can observe in the first regression of Tables 3–6, that the results are robust in sign and significance when this alternative specification is used. The estimated delay in age at which women affected by the reform have their first child is 39 days instead of 29. The impact on the probability of having a premature child is also very similar in magnitude. However, the reform's effect on the probability of having multiple births is now a bit smaller and no longer significant. Finally, the effect on birth weight is stronger. Now we find that the reform decreased the average birth weight by 6.17 g (instead of 4.15).

An alternative assumption is to consider women born in 1964, 1965, and 1966 as potential non-compliers of the law. Then, we can check the sensitivity of our results if we drop these cohorts sequentially from the analysis. The results in the second and third regressions of Tables 3–6, indicate that the effects of the reform on fertility and infant health outcomes are unchanged when we exclude these two additional cohorts.

Table 3
Robustness check: Age of the mother at first child.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Baseline results	1964, 1965 and 1966 partially affected	Drop 1966 and 1965	Drop 1966, 1965 and 1964	Treated months 1–5 Control 7–12	Treated months 1–5 Control 7–12 Partial affected	Region FE
Treated	−0.060*	−0.098***	−0.094***	−0.130***	−0.070**	−0.081***	−0.084**
	(0.030)	(0.022)	(0.020)	(0.031)	(0.026)	(0.015)	(0.029)
	[0.100]	[0.007]	[0.008]	[0.000]	[0.028]	[0.000]	[0.032]
Treated* Post Reform	0.088**	0.109***	0.103***	0.112***	0.074***	0.091***	0.072**
	(0.029)	(0.024)	(0.027)	(0.029)	(0.018)	(0.010)	(0.027)
	[0.010]	[0.002]	[0.006]	[0.008]	[0.001]	[0.000]	[0.017]
Post Reform		−0.097				−0.074*	
		(0.062)				(0.029)	
		[0.308]				[0.092]	
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓
Mother Birth-Month	✓	✓	✓	✓	✓	✓	✓
Region FE							✓
Observations	1,393,937	1,535,301	1,250,301	1,103,481	2,511,592	2,764,859	1,393,937
R^2	0.036	0.033	0.039	0.042	0.036	0.033	0.067
Dep. Var. Mean (Pre-Reform)	26.920	26.605	26.770	26.613	26.915	26.642	26.920

Notes: The dependent variable is the age (in years) of the woman when she had her first child. Regressions (1) shows the baseline estimation from Table 1, (2) assume the 1964 to 1966 cohorts to be partially affected by the reform, (3–4) eliminate the cohorts 1965–66 and 1964–66 from the analysis, (5) assumes treated women are those born from January to May and control women those born from July to December, (6) assumes treated women are those born from January to May and control women those born from July to December and treats 1964 to 1966 cohorts to be partially affected by the reform, and (7) include regional FE. All regressions include mother's year and month fixed effects. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Broader sample

Previous literature has pointed out that individuals born at the beginning of the year are typically quite different in several dimensions from individuals born at the end of the year (Bound and Jaeger, 2000; Buckles and Hungerman, 2013). However, our identification strategy does not require that individuals born at the beginning and the end of the same year are similar. Our identification is based on a weaker assumption: if there are any differences between women born at the beginning and the end of the same year, these differences stay constant for all cohorts. In any case, in our baseline results, we exclude individuals born in January, February, November, and December to delete the potentially "more" different individuals from the sample.

We re-estimate our results using a broader definition of our treatment and control groups, including women born in January, February, November, and December. The fourth regression of Tables 3, 4, 5 and 6 shows that our main findings are robust to using this broad sample, as well as if we use this broad sample and also considering the cohort of women born in 1964, 1965 and 1966 as partially treated (fifth regression of Tables 3, 4, 5 and 6). This suggests that any differences in women born at the beginning and end of the year stay constant for the cohorts affected and not affected by the reform.

Region fixed effects

We also explore the sensitivity of our results to the inclusion of the regional fixed effects. In particular, we control for Autonomous Communities' fixed effects ("comunidades autónomas" in Spanish). We have 18 Autonomous Communities in Spain, and there is a lot of decentralization at that level in terms of education and health policies. The decentralization began after the promulgation of the 1978 Constitution, and the process was carried out gradually until 2003. In terms of education, the central government regulates the basic rules of the educational system and its general organization. It also establishes the basic aspects of the curriculum. However, Autonomous Communities have complete discretion over schools, teachers, and students and enjoy financial autonomy. As far as the curriculum is concerned, the central government establishes the basic aspects, and the Autonomous Communities are responsible for establishing the complete curriculum, incorporating the basic elements set out by the central government. Regarding health almost all competences are transferred to the Autonomous Communities and the central government only carries out general and basic coordination tasks between the different regions. We believe that controlling for Autonomous Communities' fixed effects is necessary as it accounts for the different characteristics of the health and educational system at the regional level that are fixed over time.

The fifth regression of Tables 3–6, shows that our findings are extremely robust to this inclusion of regional dummies.

Event Studies and Placebos

Our main identifying assumption is that in the absence of the reform, outcomes for women born at the beginning of the year would have evolved similarly to those of women born at the end of the year. This requires that all unobservable differences between

Table 4
Robustness Check: Multiple Birth.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Baseline results	1964, 1965 and 1966 partially affected	Drop 1966 and 1965	Drop 1966, 1965 and 1964	Treated months 1–5 Control 7–12	Treated months 1–5 Control 7–12 Partial affected	Region FE
Treated	0.023	0.105	0.095	−0.058	0.100	0.131	0.026
	(0.098)	(0.078)	(0.071)	(0.128)	(0.100)	(0.106)	(0.098)
	[0.845]	[0.257]	[0.242]	[0.672]	[0.302]	[0.269]	[0.804]
Treated* Post Reform	0.217**	0.099	0.165*	0.178	0.254***	0.232*	0.218**
	(0.091)	(0.093)	(0.084)	(0.094)	(0.073)	(0.087)	(0.092)
	[0.044]	[0.348]	[0.088]	[0.137]	[0.009]	[0.076]	[0.038]
Post Reform		0.591				0.017	
		(0.694)				(0.230)	
		[0.701]				[0.946]	
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓
Mother Birth-Month	✓	✓	✓	✓	✓	✓	✓
Region FE							✓
Observations	1,393,937	1,535,301	1,250,301	1,103,481	2,511,592	2,764,859	1,393,937
R^2	0.027	0.027	0.028	0.028	0.028	0.027	0.028
Dep. Var. Mean (Pre-Reform)	2.633	2.435	2.547	2.438	2.623	2.451	2.633

Notes: The dependent variable is the probability that the woman has a first multiple birth (multiplied by 100). Regressions (1) shows the baseline estimation from Table 2, (2) assume the 1964 to 1966 cohorts to be partially affected by the reform, (3–4) eliminate the cohorts 1965–66 and 1964–66 from the analysis, (5) assumes treated women are those born from January to May and control women those born from July to December, (6) assumes treated women are those born from January to May and control women those born from July to December and treats 1964 to 1966 cohorts to be partially affected by the reform, and (7) include regional FE. All regressions include mother's year and month fixed effects. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5
Robustness check: Mature first birth.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Baseline results	1964, 1965 and 1966 partially affected	Drop 1966 and 1965	Drop 1966, 1965 and 1964	Treated months 1–5 Control 7–12	Treated months 1–5 Control 7–12 Partial affected	Region FE	Control for age mother	Control for multiple birth	Control for registered father
Treated	0.011	0.111	0.105	0.031	−0.029	0.066	−0.029	−0.031	0.021	0.010
	(0.067)	(0.069)	(0.082)	(0.109)	(0.073)	(0.090)	(0.070)	(0.066)	(0.072)	(0.068)
	[0.881]	[0.115]	[0.232]	[0.789]	[0.697]	[0.513]	[0.745]	[0.718]	[0.779]	[0.887]
Treated* Post Reform	−0.209***	−0.237***	−0.227***	−0.188**	−0.234***	−0.246***	−0.216***	−0.135**	−0.120*	−0.207***
	(0.053)	(0.051)	(0.055)	(0.050)	(0.047)	(0.047)	(0.049)	(0.049)	(0.063)	(0.053)
	[0.001]	[0.004]	[0.004]	[0.016]	[0.000]	[0.003]	[0.000]	[0.022]	[0.099]	[0.001]
Post Reform		−0.159				−0.067				
		(0.121)				(0.114)				
		[0.238]				[0.613]				
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mother Birth-Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region FE							✓			
Observations	1,393,937	1,535,301	1,250,301	1,103,481	2,511,592	2,764,859	1,393,937	1,393,937	1,393,937	1,393,937
R^2	0.009	0.009	0.009	0.009	0.009	0.009	0.014	0.010	0.072	0.009
Dep. Var. Mean (Pre-Reform)	90.362	90.107	90.223	90.099	90.370	90.127	90.362	90.362	90.362	90.362

Notes: The dependent variable is the probability that the woman has a first child with more than 37 weeks of gestation (multiplied by 100). Regressions (1) shows the baseline estimation from Table 2, (2) assume the 1964 to 1966 cohorts to be partially affected by the reform, (3–4) eliminate the cohorts 1965–66 and 1964–66 from the analysis, (5) assumes treated women are those born from January to May and control women those born from July to December, (6) assumes treated women are those born from January to May and control women those born from July to December and treats 1964 to 1966 cohorts to be partially affected by the reform, (7) include regional FE, (8) controls for if the mother gave birth between 14 to 19, 20 to 34 or more with more than 35 years old, (9) controls if the birth is single or multiple, and (10) controls if the child has a registered father or not. All regressions include mother's year and month of birth and the children's year and month of birth fixed effects. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

women born at the beginning and the end of the year are fixed over the different cohorts. While this assumption is untestable, we explore its plausibility by analyzing whether women in the treatment and control groups were on parallel trends before the introduction of the reform. We test the parallel trend assumption by adding the interaction terms between the treatment group and all cohort dummies to our baseline specification.

Table 6
Robustness check: Birth weight.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Baseline results	1964, 1965 and 1966 partially affected	Drop 1966 and 1965	Drop 1966, 1965 and 1964	Treated months 1–5 Control 7–12	Treated months 1–5 Control 7–12 Partial affected	Region FE	Control for age mother	Control for multiple birth	Control for registered father
Treated	2.856	4.931*	4.042	2.743*	1.204	5.344**	3.489*	2.316	3.270	2.751
	(2.016)	(2.201)	(2.421)	(1.571)	(2.628)	(2.238)	(1.925)	(1.897)	(2.360)	(1.988)
	[0.195]	[0.087]	[0.145]	[0.094]	[0.680]	[0.013]	[0.094]	[0.266]	[0.256]	[0.211]
Treated* Post Reform	−4.154**	−6.175***	−5.144**	−6.966**	−4.783	−7.759**	−4.004**	−2.727	−2.246	−4.059**
	(1.676)	(1.376)	(1.868)	(1.394)	(2.158)	(1.300)	(1.548)	(1.598)	(2.214)	(1.650)
	[0.025]	[0.004]	[0.023]	[0.016]	[0.105]	[0.015]	[0.021]	[0.160]	[0.426]	[0.023]
Post Reform		21.284**				16.883*				
		(6.949)				(2.310)				
		[0.062]				[0.082]				
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mother Birth-Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region FE							✓			
Observations	1,085,865	1,207,357	963,975	841,292	1,953,792	2,171,568	1,085,865	1,085,865	1,085,865	1,085,865
R^2	0.010	0.010	0.011	0.011	0.011	0.011	0.012	0.011	0.120	0.011
Dep. Var. Mean (Pre-Reform)	3217.151	3226.377	3221.523	3227.268	3217.073	3226.298	3217.151	3217.151	3217.151	3217.151

Notes: The dependent variable is the weight at birth (in grams) of the woman's first child. Regressions (1) shows the baseline estimation from Table 2, (2) assume the 1964 to 1966 cohorts to be partially affected by the reform, (3–4) eliminate the cohorts 1965–66 and 1964–66 from the analysis, (5) assumes treated women are those born from January to May and control women those born from July to December, (6) assumes treated women are those born from January to May and control women those born from July to December and treats 1964 to 1966 cohorts to be partially affected by the reform, (7) include regional FE, (8) controls for if the mother gave birth between 14 to 19, 20 to 34 or more with more than 35 years old, (9) controls if the birth is single or multiple, and (10) controls if the child has a registered father or not. All regressions include mother's year and month of birth and the children's year and month of birth fixed effects. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Given that the cohorts of women born between 1964 and 1966 are partially affected by the reform, we decided to add a couple more pre-reform cohorts for the event study approach. In particular, we consider women born between 1958 and 1971. We estimate the following regression:

$$Outcome^{i}_{myt} = \alpha + \beta_1 Treated_i + \lambda_y Treated^i * \sum_{y=1958, y\neq 1963}^{1971} \mu_y + \delta_m + \mu_y + \theta_t + \epsilon^{i}_{myt}$$

where $\sum_{y=1958,y\neq 1963}^{1971} \mu_y$ is a set of dummy variables that equal one for women born in year y and zero otherwise. We consider the first cohort, 1963, as the baseline. Finally, we report robust standard errors as the wild bootstrap procedure does not run correctly when there is a large number of coefficients to be estimated vis-a-vis the number of clusters (see Roodman et al., 2019 for a discussion on boottest, the Stata procedure to evaluate wild bootstrap). In Fig. 2 we plot the estimates of the interaction terms (λ_y) and the 95 percent confidence interval.

Reassuringly, most of the coefficients of the cohorts not affected by the reform are estimated zeros for all the main outcomes of the paper, providing reliable evidence of the fulfillment of the parallel trend assumption.[20] There is one exception. We observe that, for the probability of having multiple births, most of the estimates for the pre-treatment cohorts are negative and statistically significant, which suggests that the parallel trends assumption for this specific outcome is not entirely fulfilled. Consequently, we should view the estimated reform effect on this particular outcome with caution.

We also perform several placebo tests in which we use "fake" reform years. We examine the effect of eight "fake" reforms affecting the cohorts of 1956 to 1963, and we use for our data sample all births of women born between 1954 and 1964. We do not perform placebos for the cohorts of 1964 and 1965 because, as explained above, women born in those years could be potentially influenced by the reform. We use the exact econometric specification and treatment status definition as before. We expect a nonsignificant effect of the interaction term between the post-reform dummy and the treatment dummy.

In Fig. 3, we plot the estimates of the interaction term and the 95 percent confidence interval for our main findings. We observe that the effect is not significant for most of our placebos. This provides clear evidence that any seasonal differences between women born at the beginning or end of the year remained constant across the cohorts before the reform, and our estimates capture the reform's change of incentives to continue in the educational system.

7. Explanatory mechanisms

The Postponement of First Births and Multiple Births

The first channel through which the reform could potentially affect infant health is the postponement of the entrance into motherhood. We have shown in Table 1 that women affected by the reform postponed fertility by approximately one month. In

[20] In Figure A2, we also see that the parallel trend assumption holds for the women's educational outcomes.

(a) Age at which women had their first birth

(b) Probability of having a multiple births

(c) Probability of mature first birth

(d) Birth weight of first child

Fig. 2. Event Studies
Notes: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the cohort dummies of the event studies estimation. We consider cohorts not affected by the reform: 1958–1963. Cohorts of women born between 1964 and 1965 are partially affected, and all women born after 1966 are fully affected by the reform.
Source: Birth registries (1975–2018), all women from cohorts 1959–1971.

Table 7, we further study this postponement by looking at the effect of the reform on the probability of having a first birth during the teenage years (14–19), between 20 and 34 years old and at what the medical literature considers advanced maternal age (35–48). We observe that the reform increased the probability that women have their first child after 35 by 0.36 percentage points (or a 4% with respect to the pre-reform mean). Previous medical literature has indicated that having a first birth after that age could adversely affect infant health as the risk during pregnancy increases (Ziadeh, 2002; Astolfi and Zonta, 2002). For instance, Jolly et al. (2000) finds that advanced maternal age is correlated with an increased likelihood of delivering a small (for gestational age) baby, which may be related to poorer placental perfusion or transplacental flux of nutrients. Likewise, delayed childbearing is correlated with an increased risk of low birthweight (Tough et al., 1999; Aldous and Edmonson, 1993), stillbirths, unexplained fetal death (Fretts, 2001; Reddy et al., 2006), and preterm delivery (Roberts et al., 1994). We believe that the reform produced a shift in the overall distribution of age (at first birth) for treated mothers, which increased the probability of having their first child after the age of 35. In Figure A1 , in the Appendix, we report the estimated impact of the reform on the probability of women's having their first birth at each age. As can be seen, the reform reduced the number of pregnancies for mothers below 21 years old and increased the number of pregnancies for mothers above 32 years old. While the estimates for pregnancies of women aged between 22 and 31 seem pretty mixed. This suggests a progressive shift in the age at first birth distribution, making the decrease in the left tail and increase in the right tail significant. We also report the reform's effect using these alternative age brackets (below 21, 22–27, 28–32, 33–38, and over 39) in Table A8 in the Appendix.

Delayed childbearing is also correlated with a higher incidence of multiple births (Tough et al., 2002). Multiple-birth children normally have worse infant health outcomes at the time of delivery than single-birth children. We find that the reform affected

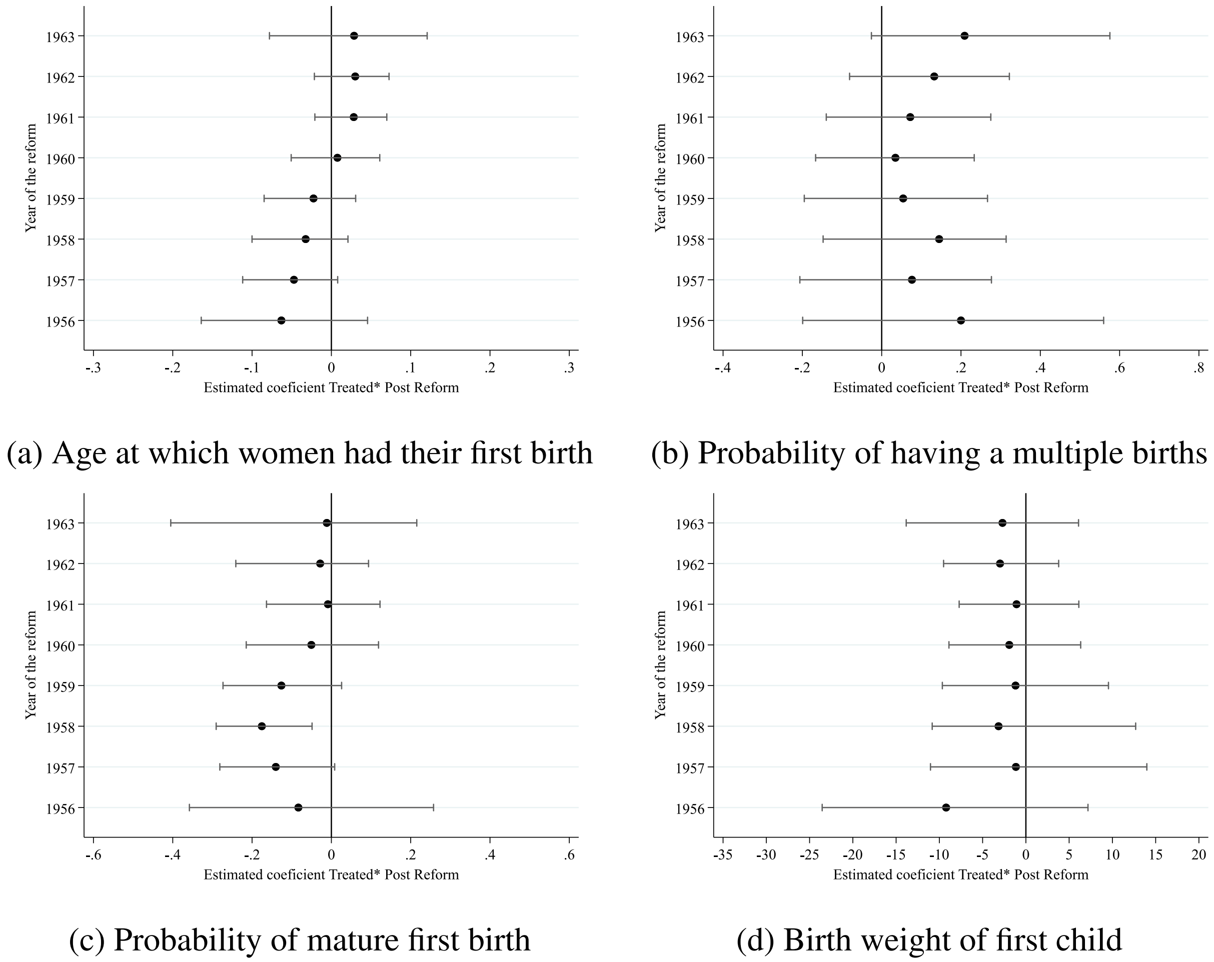


Fig. 3. Placebos.
Notes: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the "fake" reform taking place for the cohorts of 1956 to 1963. We consider cohorts not affected by the real reform: 1954–1964. Treated individuals are those women born from March to May, and the control are those born from August to October.
Source: Birth registries (1975–2018), all women from cohorts 1954–1964.

the probability of treated women having multiple first births. Even though, as we explained before, this result should be taken with caution, there is a possibility that it partially explains the detrimental effects of the reform on infant health.[21] Many of these women might start receiving infertility treatments, which are associated with higher probabilities of having a multiple pregnancy. Furthermore, the medical literature shows that after the age of 35, the probability of having multiple births increases, even without fertility treatments.

We can examine the effect of the reform on the main outcomes of infant health, controlling for if the mother gave birth between 14 to 19, 20 to 34, or more than 35 years old. We also control whether the child was part of a multiple birth. Even though these variables are "bad controls" (as they are directly affected by the reform), comparing the estimates with and without these controls may be informative about the importance of these potential mechanisms. Tables 5 and 6 show that controlling for either the mother's age at the moment of birth or multiple births halves these estimates, even though the effect on maturity remains negative and significant.

[21] Given that the reform affects the probability of having multiple births, we cannot study the effects on infant health outcomes excluding multiple births, as this approach will result in a selected sample. However, if we perform this exercise and examine the reform's effects on infant health considering only singleton births, we observe that the sign of the estimates goes in the same direction as when we consider the whole sample. However, the estimates are smaller and insignificant. In fact, the point estimates are extremely similar to those reported in Tables 5 and 6 when we control for mother's age at first birth and multiple births. If we ignore the bias of the selected data, this suggests that analyzing only singletons births is almost equivalent to controlling for mothers' age at first birth or multiple births. The results on singleton births are available upon request.

Table 7
Probability of having the first birth at a certain age bracket.
Source: Birth registries (1975–2018), all women from cohorts 1961–1965 and 1967–1971.

	Probability of having first birth between the ages		
	14–19	20–34	35–48
Treated	0.188	−0.150	−0.038
	(0.123)	(0.133)	(0.156)
	[0.115]	[0.303]	[0.802]
Treated* Post Reform	−0.214	−0.148	0.362**
	(0.147)	(0.170)	(0.143)
	[0.182]	[0.416]	[0.050]
Mother Birth-Year FE	✓	✓	✓
Mother Birth-Month	✓	✓	✓
Observations	1,393,937	1,393,937	1,393,937
R^2	0.003	0.003	0.013
Dependent Variable Mean (Pre-Reform)	10.909	80.213	8.879

Notes: The dependent variables are the probability of having a first child between the ages of (1) 14 and 19, (2) 20 and 34, and (3) 35 and 48 (multiplied by 100). Regressions include mother's year of birth and month of birth fixed effects. Treated individuals are those women born from March to May, and the control are those born from August to October. Women born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8
Marital status of mothers.
Source: Birth registries (1975–2018), first children of women from cohorts 1961–1965 and 1967–1971.

	Registered father	Mother married
Treated	0.107*	0.187
	(0.053)	(0.165)
	[0.077]	[0.293]
Treated* Post Reform	−0.132**	−0.157
	(0.041)	(0.159)
	[0.014]	[0.361]
Mother Birth-Year FE	✓	✓
Mother Birth-Month FE	✓	✓
Child Birth-Year FE	✓	✓
Child Month-Year FE	✓	✓
Observations	1,393,937	1,393,937
R^2	0.027	0.044
Dependent Variable Mean (Pre-Reform)	96.920	88.701

Notes: The dependent variables are (1) the probability that the child has a registered father (multiplied by 100), and (2) the probability that the mother is married at the time of delivery (multiplied by 100). All regressions include mother's year and month of birth and the children's year and month of birth fixed effects. Treated children are those whose mother was born from March to May, and the control are those whose mother was born from August to October. Children whose mother were born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at mother's cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

These results indicate that the postponement in the entrance of motherhood after the age of 35 could explain a large bulk of the negative effect of the reform on the health of their children at the moment of delivery.

Changes in the Maternal Marital Status

The postponement of fertility may result from a similar postponement or reduction of marriage. In this section, we analyze the change in mothers' marital status. Previous literature (Gaudino et al., 1999; Bennett, 1992; Balayla et al., 2011) has established that children whose mothers are not married or have no registered father in the birth certificate data tend to have worse health outcomes at the time of delivery. Table 8 shows that the reform significantly increased the probability that first children did not have a registered father by 0.132 percentage points. On the other hand, we do not observe any effect on the mother's marital status.[22]

Therefore, a second possible mechanism through which the reform could be detrimental to infant health is the increase in the number of children without a registered father that we observe as a consequence of the reform. We examine the importance of this mechanism by analyzing the effects on infant health but controlling for if the child has a registered father or not at the moment of birth. Tables 5 and 6 show that controlling for the father's presence at the moment of delivery does not modify the results on

[22] This result is consistent with the lack of effect of the reform over marriage rates that we report in Table A9 of the Appendix.

Table 9
Labor Market Outcomes.
Source: Spanish Labor Force Survey (1999–2019), Spanish women or women with children from cohorts 1961–1965 and 1967–1971.

	Employed	Unemployed	Inactive	High-skilled job	Semi-skilled job	Low-skilled job
Panel A: All women						
Treated	−0.785	−0.084	0.869*	−0.452	−0.580	0.497
	(0.499)	(0.297)	(0.418)	(0.432)	(0.393)	(0.452)
	[0.154]	[0.787]	[0.054]	[0.322]	[0.169]	[0.340]
Treated*Post Reform	0.699	−0.055	−0.644	0.818	0.236	−0.402
	(0.532)	(0.368)	(0.483)	(0.454)	(0.422)	(0.445)
	[0.225]	[0.873]	[0.235]	[0.133]	[0.619]	[0.379]
Observations	524,034	524,034	524,034	512,375	512,375	512,375
Dependent Variable Mean (Pre-Reform)	59.218	10.925	29.857	14.615	31.136	12.563
Panel B: Mothers						
Treated	−0.120	−0.117	0.237	−0.628*	−0.209	0.688
	(0.690)	(0.365)	(0.552)	(0.328)	(0.880)	(0.411)
	[0.846]	[0.759]	[0.680]	[0.088]	[0.817]	[0.145]
Treated*Post Reform	0.954*	−0.125	−0.829	0.690*	0.265	−0.011
	(0.471)	(0.356)	(0.466)	(0.334)	(0.553)	(0.366)
	[0.085]	[0.777]	[0.124]	[0.083]	[0.644]	[0.979]
Observations	424,044	424,044	424,044	417,599	417,599	417,599
Dependent Variable Mean (Pre-Reform)	57.910	10.929	31.160	13.841	30.461	12.840
Women Birth-Year FE	✓	✓	✓	✓	✓	✓
Women Birth-Month	✓	✓	✓	✓	✓	✓
Quarter-Year FE	✓	✓	✓	✓	✓	✓

Notes: The dependent variables are the probability that the woman (Panel A) or the woman with at least one child (Panel B) (1) is employed, (2) unemployed, (3) inactive, (4) has a high-skilled job, (5) has a semi-skilled job, and (6) has a low-skilled job (all multiplied by 100). All regressions include women's year and month of birth and quarter-year of interview fixed effects. Treated individuals are those women born from March to May, and the control are those born from August to October. Women born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at women's cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

the probability of having a first birth with more than 37 weeks of gestation or birth weight. This indicates that this might not be a major mechanism behind the negative effects of the reform on infant health.

Changes in Mother's Employment

Mothers' working status during pregnancy could also affect children's health outcomes at birth. On the one hand, women who work during pregnancy could suffer from larger psychological and physiological stress levels. Previous literature has pointed out that prenatal stress could lead to adverse effects on birth outcomes (Black et al., 2016; Persson and Rossin-Slater, 2018). Moreover, pregnant working women might be exposed to specific occupational exposures (such as second-hand tobacco smoke Bharadwaj et al., 2014, chemicals Snijder et al., 2012, or shift work Bonzini et al., 2011) that could be detrimental to their child's health. Finally, expecting mothers that are not working (and therefore have more leisure time) may have more time to rest, attend their prenatal check-ups and follow a healthier diet.

Previous literature investigating the effect of employment during pregnancy on children's health finds mixed results. Del Bono et al. (2012) find that not working during the last two months of pregnancy has positive effects on birth weight and fetal growth. Rossin (2011) also finds that the enactment of unpaid maternity leave in the U.S. improved child health at birth for mothers with high education and married. On the contrary, Wüst (2015) shows that women who work late in their pregnancy are 2.8 percentage points less likely to experience premature birth. Finally, Ahammer et al. (2020) evaluated a maternity leave extension in Austria and found no evidence of significant effects of this extension on children's health at birth.

A third potential channel through which the reform has detrimental effects on infant health is the change in the mother's employment during pregnancy. We cannot investigate this channel directly as there is no information about mothers' working status or occupation during pregnancy. Nonetheless, we can use the Spanish Labor Force Survey between 1999 and 2019 to examine the reform's effect on women's employment prospects between 28 and 57 years old.

It is important to note that, in Spain, women have never had access to a voluntary or mandatory maternity leave during the pregnancy. However, women can take sick leave during the last weeks/months of their pregnancy if their employment entails a risk for the development of their pregnancy. After delivery, mothers have the right to take up to 16 weeks of leave, from which the first six weeks are compulsory. This maternity leave system was established in 1989, and there has not been any change in the regulation until 2022 (which is not included in our database).

In Table 9, we can observe that the reform did not have any effect on women's participation in the labor market, employment, unemployment, or occupational choice. This result is the same as the one found in Del Rey et al. (2018). However, when we focus on women with at least one child, the reform increased by 0.95 percentage points (1.6%) the probability that mothers are employed and increased by 0.69 percentage points (4.9%) the probability that they are working in a high-skilled job.

This result suggests that more educated mothers could have worked more during pregnancy, especially in high-skilled occupations. Then, being exposed to an environment with higher stress levels, certain occupation conditions, or having less leisure time

Table 10
Risky health behaviors of mothers.
Source: Spanish National Health Survey (2003, 2006 and 2012), all mothers from cohorts 1961–1965 and 1967–1971.

	Risky health behaviors						
	Ever drank alcohol	Drinks alcohol daily	Drinks alcohol less once month	Never Smoked	Smokes	Num cigarettes weekly	Pregnancy as motive to quit smoking
Treated	−1.405	−2.256	−0.261	3.649	−1.951	−1.136	13.971**
	(2.723)	(1.264)	(2.053)	(2.163)	(3.912)	(0.793)	(3.675)
	[0.612]	[0.123]	[0.906]	[0.185]	[0.642]	[0.250]	[0.040]
Treated*Post Reform	1.699	3.472*	−5.984**	−1.352	11.560**	2.078**	−19.899**
	(2.129)	(1.494)	(2.422)	(4.512)	(4.574)	(0.763)	(7.287)
	[0.454]	[0.056]	[0.026]	[0.794]	[0.032]	[0.027]	[0.037]
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓
Mother Birth-Month FE	✓	✓	✓	✓	✓	✓	✓
Interview Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	3102	3102	3102	3108	1843	1843	537
R^2	0.071	0.019	0.093	0.010	0.030	0.025	0.078
Dependent Variable Mean (Pre-Reform)	71.804	6.972	65.443	39.890	61.280	8.166	15.035

Notes: The dependent variables are (1) the probability that the mother has ever drunk alcohol, (2) the probability that the mother consumes alcohol daily, (3) the probability of that the mother consumes alcohol less than once a month, (4) the probability of never have smoke, (5) the probability that the mother smokes at the moment of the interview (conditional on her ever smoked before),(6) number of cigarettes consumed daily (conditional on her ever smoked before), and (7) the probability that the mother has quited smoking during pregnancy, conditional on being an ex-smoker (all multiplied by 100). The last variable is only available in waves 2006 and 2011. Regressions include year of interview, year of birth and month of birth dummies. Treated individuals are those women born from March to May, and the control are those born from August to October. Women born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

during pregnancy could have negatively impacted their children's health at the moment of delivery. However, given that we cannot investigate this channel directly on women during their fertile years (or during pregnancy), these results should be taken with caution.

Lastly, we want to point out that a larger effect of the reform on the employment prospects of mothers could be driven by the acquisition of more education during the gender equalization process that Spain was experiencing at the moment of the reform. During the dictatorship, women could not participate in the labor market without the permission of their husbands or their father. The end of the dictatorship increased gender equality and improved women's access to the labor market, especially among more educated women. Therefore, the effect of the gender equalization process could have been more prominent for the group of women whose rights were more repressed during the dictatorship (educated women that were married with children).

Changes in Health Habits

A fourth channel through which the reform could be affecting infant health is through changes in the health habits of mothers. In a previous paper, Bellés-Obrero et al. (2021) examine the effect of this reform on mortality. They also found that the reform deteriorated women's health habits. In this paper, we will follow the same analysis to examine if the health habits of affected mothers have also deteriorated due to the reform. Table 10 shows that, after the reform, mothers drink alcohol more often. In particular, the reform increased by 3.47 percentage points (49%) the probability of consuming alcohol daily and decreased by 5.98 percentage points (9%) the probability of consuming alcohol less than once a month. Moreover, the reform increased by 11.56 percentage points the probability (18%) of being a current smoker, and mothers affected by the reform, on average, smoked two more cigarettes daily. We also find that after the reform, mothers have a lower probability of quitting smoking during pregnancy (conditional on being ex-smokers).[23] Therefore, these outcomes could directly affect the health of their offspring.

Interestingly, in Table A10, we do not find that the reform deteriorated the health habits of fathers. This result suggests that the deterioration of mothers' health behaviors was likely linked to acquiring more education during a gender equalization process. For women in these cohorts, access to, and social acceptance of, smoking was much higher than for previous (pre-reform) cohorts. For instance, a recent paper by Bilal et al. (2015) shows a high negative correlation between gender inequality and the female-to-male smoking ratio in Spain from the 1960s to the 2010s. Previous literature has demonstrated the association between increased education and the prevalence of unhealthy behaviors (especially smoking) among Spanish women, converging toward men's behaviors (see Pampel, 2003 and Schiaffino et al., 2003 for the Spanish case).

More importantly, this positive association between education and the prevalence of smoking for women cannot be considered a particular case of Spain. In many countries, the number of women smoking is increasing, even though smoking prevalence among

[23] This last question is only available in the survey waves of 2006 and 2011.

Table 11
First children's medium-term outcomes.

	Good subjective health	Good objective health	Hospital visit last 12 m	Private insurance	Exercise more once month	Sleep hours	Watch tv more 3 h a day	Mediterranean diet
Treated	3.813	1.775	2.332	0.459	1.731	−0.114	2.201	−1.119
	(3.283)	(1.915)	(1.416)	(3.197)	(3.253)	(0.083)	(1.768)	(1.315)
	[0.300]	[0.371]	[0.154]	[0.861]	[0.634]	[0.231]	[0.238]	[0.380]
Treated* Post Reform	−7.284**	5.063**	0.161	8.020**	−5.953	0.254**	−2.169	2.479*
	(2.419)	(1.790)	(2.631)	(3.131)	(3.930)	(0.111)	(1.410)	(1.415)
	[0.032]	[0.012]	[0.960]	[0.013]	[0.216]	[0.049]	[0.183]	[0.075]
Mother Birth-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Interview Year FE	✓	✓	✓		✓	✓	✓	✓
Children's Age FE	✓	✓	✓		✓	✓	✓	✓
Children's Gender	✓	✓	✓		✓	✓	✓	✓
Observations	2293	2290	2293	1036	2283	2292	1991	2287
R^2	0.075	0.033	0.021	0.019	0.159	0.230	0.086	0.018
Dependent Variable Mean (Pre-Reform)	33.306	80.217	4.174	0.170	63.591	9.133	6.173	4.443

Notes: The dependent variables are the probability that (1) the mother self-reports that her first child has very good health (multiplied by 100), (2) the first child not suffer from diabetes, asthma, mental disorders or chronic allergies (multiplied by 100), (3) the first child had to go to the hospital in the last 12 months (multiplied by 100), (4) the family has a private insurance (multiplied by 100), (5) the first child exercise at least once a month (multiplied by 100), (6) the number of hours that the first child sleeps, (7) the first child watches TV more than 3 h a day (multiplied by 100), and (8) the first child has a Mediterranean diet (eats fruit, vegetables and milk every day and legumes and fish at least three times a week) (multiplied by 100). Regressions (1–3 and 5–7) include year of interview, mother's year and month of birth dummies, children's age dummies and children's gender fixed effects. Regression 4 only includes mother's year and month of birth dummies. Treated individuals are those women born from March to May, and the control are those born from August to October. Women born between 1967 and 1971 are affected by the reform. Robust standard errors clustered at cohort level in parentheses, and the *p*-value of the wild bootstrap with 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. For regression (4): European Community Household Panel (2000), families with at least one child where the mother is born in 1961–1965 and 1967–1971.

women is still lower than among men. This phenomenon can be attributed to the weakening of the social and cultural constraints that prevented many women from smoking in the past (Mackay and Amos, 2003). In some Eastern European and Eastern Mediterranean countries, a high smoking prevalence among highly educated women compared to low educated women has been established by previous literature (Bosdriesz et al., 2014). This same pattern has been found to hold (Pampel, 2003) in other high-income countries at the early stages of the smoking epidemic. Then, the process of gender equalization and the initial adoption of tobacco consumption that was taking place in the early post-Franco era in Spain could explain the positive correlation that we find between smoking prevalence and education among women. Those women affected by the child labor reform had higher education and financial independence that improved their social status and hence an autonomy to emulate their male counterparts' lifestyle.

To sum up, we find evidence that the child labor reform had a negative impact on the short-term health of children. Even though we investigated four different channels, we believe that this detrimental impact on infant health was due mainly to an increase in unhealthy behaviors of more educated mothers and to the postponement of fertility after 35. The deterioration of mothers' health behaviors was an interaction effect between acquiring more education and the gender equalization process these women were experiencing at that moment. At the same time, as we can observe in the Appendix (Table A11), the reform did not affect the infant health of the children of the affected men (even though we find a similar postponement of fertility for them too). This important result suggests that the mechanism through the child labor reform affecting infant health has to be related to the mother's characteristics or behaviors during pregnancy. This observation reinforces our finding that delayed childbearing and bad behaviors during pregnancy (such as smoking) are essential to explain the negative effect of the reform on infant health.

8. Medium-term outcomes

Table 11 reports the medium-run effects of the reform on these children's health outcomes. We first report the medium-term health status and habits of their first children. As one can easily see, in the first column of Table 10, the objective good health status seems to be affected in the opposite way. Children from mothers affected by the reform had a 5.1 higher probability of having good objective health. The reversal of the adverse effects of the reform for children between the moment of delivery and the teenage years is a very striking finding. In the present section, we investigate two potential mechanisms for achieving this effect: habits and maternal vigilance. More educated parents can contribute to better health by ensuring their offspring makes lifestyle choices that are more conducive to good outcomes. They can also invest in other preventive measures, like taking the children more often to the doctor. This could be driven by both the fact that mothers affected by the reform have more education (information, knowledge) and/or greater resources (income, stability) because they have their offspring later in life. Yet, we will not be able to distinguish between these two potential drivers of mothers' behavior (resources or education), as we do not have these specific outcomes in our database, and they are probably significantly correlated.

More educated mothers seem to impact some habits of the children positively. Children of mothers affected by the reform sleep more and have better eating habits. In particular, the reform increased by 0.25 h the children slept at night and by 2.9 percentage points the probability that they follow a Mediterranean diet. On the other hand, the reform did not impact the amount of tv they watch or their exercise habits. Even though this change of habits may have contributed to the reversal of children's health outcomes at the moment of delivery, it is unlikely that this is the only explanation.

Maternal vigilance could be another important mechanism. We use the self-reported probability of having good health as a proxy for subjective health measures. In Table 11, we observe that the children born from mothers affected by the reform have a 7.3 percentage points lower probability of having subjective good health. Clearly, this would trigger a larger preoccupation by mothers about their offspring's health. This is further reinforced by our finding that children of treated mothers have eight percentage points higher probability of having private insurance (in Spain, where public coverage is universal, this is something done by individuals wishing to have premium quality care as well as quicker access to specialized care).

In Table A12, we report the reform's effect on the medium-run health status and habits of all children, regardless of whether they are firstborn or not. The results are very similar.

The potential picture that emerges is one of the mothers who had smaller, more fragile children and who, even at age 15, still worry more about them (even if, objectively, the health is already better). Thus, they put a lot of care (like expensive private insurance and better habits) to guarantee they have a positive health status. That, in turn, leads to a reversal of the adverse effects from birth.[24]

9. Discussion

This study investigates the effect of women's education on fertility and children's health outcomes during a time of gender equalization. We exploit a reform implemented in Spain in 1980 that increased the minimum legal working age from 14 to 16 years old. Before the reform, students born at the beginning of the year had different incentives to finish primary education than those born at the end of the year. The introduction of the reform abolished these different incentives. Thus, we exploit the within-cohort variation, following a difference-in-difference approach by comparing individuals born during the first or last months of the year, before and after the reform.

Del Rey et al. (2018) showed that the reform was enforced and was effective. Those women and men born at the beginning of the year (with lower educational attainment before the reform) had higher incentives to finish primary education and continue secondary and post-secondary education after the reform.

We add to this previous literature and find that the child labor reform also impacted fertility and infant health outcomes of affected women. More specifically, the reform prompted a postponement of first births by a month, on average. However, our results show that this postponement is followed by a catching-up effect and a zero impact on completed fertility.

We then focus on the effects of the reform on children's health at the moment of delivery. For mothers born at the beginning of the year, the reform increased the probability of having a first child with less than 37 gestational weeks and it also decreased birth weight. We document two different channels that could lead to this detrimental effect of the child labor law on children's health. The first channel is the increase in the age at which treated women get pregnant for the first time. The negative impacts of the child labor law on infant health could be partly driven by treated mothers having their first child at an older age, making their pregnancies riskier and increasing the chances of poor infant health outcomes. The second channel is changes in unhealthy habits of affected others. More precisely, the reform increased the probability of smoking and drinking alcohol for treated mothers. Additionally, we also find that the probability of smoking cessation during pregnancy is reduced for mothers born at the beginning of the year after the reform. These last results are very similar to the ones previously found by Bellés-Obrero et al. (2021) for all women (independently of whether they became mothers or not).

In the Appendix A, we analyze the reform's effect on men's fertility decisions and infant health outcomes of their children. The postponement effect in men's fertility is very similar to the one observed for women. However, the reform did not affect the infant health of the children of affected men. This important result suggests that the mechanism through which the child labor reform affects infant health has to be related to the mother's characteristics or behaviors during pregnancy. This reinforces our finding that delayed child-bearing and bad behaviors during pregnancy (such as smoking) are key in explaining the negative effect of the reform on infant health.

Nevertheless, these negative effects at birth disappear in the medium run. We next explore the channels that can explain this important reversal of early-life adverse conditions in the medium term. First, mothers contribute to their offspring's better health, making lifestyle choices that lead to good outcomes (better diet, for instance). Second, we show that mothers report that those (already) healthy children have lower (subjective) good health. And we show that children of treated mothers have a significantly higher probability of having private insurance. This is consistent with the idea that more educated mothers remain worried about their children's health. This is true even if they have been able to reverse the negative health outcomes at birth.

Summing up, more educated mothers had smaller and more premature children due to delayed child-bearing and bad behaviors during pregnancy. They remain worried about their children's health (as they assess their health status not to be good when their actual health is, indeed, good). Because of that, they put a lot of care (for example, by providing them with private insurance) to compensate the negative health effects at birth. That, in turn, leads to a reversal of the adverse effects at birth during childhood.

Our results must be interpreted within the social context of Spain at the time of the reform, just a few years after the end of Franco's dictatorship, which lasted for almost 40 years. During this time, the country's levels of educational attainment, child labor, and women's social development were closer to those of a middle-income country. It must be noted that during the dictatorship, Spain was a male-dominated society. The end of the dictatorship raised gender equality and improved women's access to economic opportunities, which led to a convergence of health risk factors between men and women. Therefore, our results will provide important policy implications for middle-income countries undergoing those gender equalization processes right now. In particular,

[24] Another potential explanation we cannot completely rule out is that affected mothers try to reverse the negative effect from birth through maternal vigilance and better habits. However, this reversal does not completely take place, and their children are still less healthy but on dimensions that the objective health measure does not capture.

we believe that increases in education could potentially have the same short-term detrimental effects on infant health in countries where we still observe high smoking prevalence among highly educated women compared to low-educated women (while the correlation between education and smoking is negative for men). This situation will most likely emerge as a consequence of a gender equalization process, where women are experiencing the weakening of the social and cultural constraints that prevented them from smoking in the past, leading to a positive correlation between smoking and education only for women. Previous research has established a higher smoking prevalence among highly educated women than among low-educated women in Eastern Europe and Eastern Mediterranean countries (Bosdriesz et al., 2014). Also, Pampel (2003) showed that high-income countries at the early stages of the smoking epidemic (like the southern European countries a few decades ago) had higher rates of female smokers among the young and highly educated. Moreover, in the 2019 wave of the European Health Interview Survey, we observe that in Romania, 19% of women with tertiary education smoke, but only 15% with secondary education do (for men, the correlation is reversed, 45% for secondary education and 40% for tertiary education). A similar image emerges in Turkey, where 47% of men and 15% of women with primary education smoke, while the numbers are 39% of men and 22% of women for those with tertiary education. Thus, we believe the results we find in this paper are more relevant, from a policy perspective, to these middle-income countries with social development similar to the levels that Spain was experiencing around 1980.

Data availability

Data will be made available on request.

Appendix A. Data appendix

Throughout this paper we use different databases. In this section, we aim to describe these databases and explain the main variables used in our previous analysis.

A.1. Birth statistics

This database contains administrative data from birth certificates for the universe of children born in Spain between 1975 and 2018. The information is self-reported as it comes from the Statistical Birth Bulletins, that are filled out by the parents, relatives or persons so obligated by law to declared the childbirth. Here we define the main fertility and infant health variables used throughout the paper:

- **Number of first births per 1000 women**: We collapse the number of first birth by month and year of birth of the mother. Then we divide it by the number of women that were born that month and year, and we multiply it by 1000. This measure reflects the number of women born in a certain month and year that became mothers.
- **Total number of births per 1000 women**: We collapse the number of all birth by month and year of birth of the mother. Then we divide it by the number of women that were born that month and year, and we multiply it by 1000. This measure reflects the total number of children born from women of a certain month and year of birth.
- **Age women at first birth**: Age of the women when they had their first child.
- **First birth between certain ages**: A dummy variable that is equal to one if the woman has her first child between that ages, and zero otherwise (multiplied by 100).
- **Male**: A dummy variable that is equal to one if the first child of the woman is male, and zero otherwise (multiplied by 100).
- **Multiple birth**: A dummy variable that is equal to one if the woman has a first multiple birth, and zero otherwise (multiplied by 100).
- **Survives first 24h**: A dummy variable that is equal to one if the first child of the woman survives the first 24 h after delivery, and zero otherwise (multiplied by 100).
- **Weeks of gestation $\geq$ 37**: A dummy variable that is equal to one if the first child of the woman was born with 37 or more weeks of gestations, and zero otherwise (multiplied by 100).
- **Birth Weight**: Weight at birth of the woman's first child (in grams).
- **Weight < 2500**: A dummy variable that is equal to one if the woman's first child is born with less than 2500 grams, and zero otherwise (multiplied by 100).
- **Registered father**: A dummy variable that is equal to one if the woman's first child has a registered father, and zero otherwise (multiplied by 100).
- **Mother married**: A dummy variable that is equal to one if the woman's is married when she had her first child, and zero otherwise (multiplied by 100).

A.2. Spanish labor force survey

This database is continuous quarterly survey that contains information related to the labor market, active unemployment, and inactivity of the population living in a family dwelling in Spain between 1999 and 2019. Here we define the main labor market variables used throughout the paper:

- **Employed**: A dummy variable that is equal to one if the woman is working at the moment of the interview, and zero otherwise (multiplied by 100).
- **Unemployed**: A dummy variable that is equal to one if the woman is unemployed at the moment of the interview, and zero otherwise (multiplied by 100).
- **Inactive**: A dummy variable that is equal to one if the woman is not participating in the labor market at the moment of the interview, and zero otherwise (multiplied by 100).
- **High-skilled job**: A dummy variable that is equal to one if the woman is working at a high-skilled job at the moment of the interview, and zero otherwise (multiplied by 100). We include managerial and professional occupations as high-skilled jobs.
- **Semi-skilled job**: A dummy variable that is equal to one if the woman is working at a semi-skilled job at the moment of the interview, and zero otherwise (multiplied by 100). We include technical, administrative, service and sales, skilled agriculture, and skilled trade occupations as semi-skilled jobs.
- **Low-skilled job**: A dummy variable that is equal to one if the woman is working at a low-skilled job at the moment of the interview, and zero otherwise (multiplied by 100). We include machine operative and elementary occupations as low-skilled jobs.

A.3. Spanish national health survey of 2003, 2006 and 2011

This database if a representative nationwide cross-sectional survey that collects health related information as well as the socio-economic status of children and adults. Here we define the main marriage variables used throughout the paper:

- **Ever drank alcohol**: A dummy variable that is equal to one if the mother has even drunk alcohol, and zero otherwise (multiplied by 100).
- **Drinks alcohol daily**: A dummy variable that is equal to one if the mother drinks alcohol everyday, and zero otherwise (multiplied by 100).
- **Drinks alcohol less than once month**: A dummy variable that is equal to one if the mother drinks alcohol less than one time a month, and zero otherwise (multiplied by 100).
- **Never smoked**: A dummy variable that is equal to one if the mother has never smoked, and zero otherwise (multiplied by 100).
- **Smokes**: A dummy variable that is equal to one if the mother smokes at the moment conditional on her having ever smoked before, and zero otherwise (multiplied by 100).
- **Num cigarettes daily**: Number of cigarettes the mother consumes daily conditional on her having ever smoked before.
- **Pregnancy as motive for being ex-smoker**: A dummy variable that is equal to one if the mother quited smoking during pregnancy conditional on being and ex-smoker, and zero otherwise (multiplied by 100). This variable is only available for the waves 2006 and 2011.
- **Good subjective health**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) has very good health at the moment of the interview, and zero otherwise (multiplied by 100).
- **Good objective health**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) does not suffers from diabetes, asthma, mental disorders or chronic allergies at the moment of the interview, and zero otherwise (multiplied by 100).
- **Hospital visit in the last 12 months**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) had a visit to the hospital in the last 12 months before the moment of the interview, and zero otherwise (multiplied by 100).
- **Exercise more than once a month**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) exercises more than once a month at the moment of the interview, and zero otherwise (multiplied by 100).
- **Sleep hours**: Number of hours that the mother's first child sleeps at night.
- **Watch TV less than 3 h a day**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) watches TV more than 3 h a day at the moment of the interview, and zero otherwise (multiplied by 100).
- **Mediterranean diet**: A dummy variable that is equal to one if the mother self-reports that her first child (between the ages of 2 and 15) eats fruit, vegetables and milk every day and legumes and fish at least three times a week at the moment of the interview, and zero otherwise (multiplied by 100).

A.4. European community household panel: extended panel of 2002

This database if a representative nationwide cross-sectional survey that contains information on income, financial situation, working life, social relations and health of the household. Here we define the main marriage variables used throughout the paper:

- **Private insurance**: A dummy variable that is equal to one if the household with at least one child has a private health insurance, and zero otherwise.

Appendix B. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2023.104411.

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