# Women's Education, Fertility and Children's 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 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 completed fertility of affected women. We also show that the reform was detrimental for the health of the children's of affected mothers at delivery. We document two channels for this negative effect: the postponement in the entrance of motherhood and the deterioration of women's health habits (such as smoking and drinking). This last channel is a direct effect of the gender equalization process. However, in the medium run, these more educated mothers are able to reverse the negative health shocks at birth through maternal vigilance and investment in their children's health habits.

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# 1 Introduction

Women's education is generally considered to be 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 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 health care 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.[1] 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. 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

[1] Black et al. (2008) find that increases in compulsory schooling reduce the incidence of teenage childbearing in both the US and Norway. Other papers have also found the same postponement effect of childbearing away from the teenage years in Norway (Monstad et al., 2008), Italy (Fort, 2007), and the UK (Silles, 2011; Geruso and Royer, 2018). The effect of education on completed fertility is less clear. While some papers argue that education has mainly an "incarceration effect" delaying but not reducing completed fertility (Monstad et al., 2008; Fort, 2007; Silles, 2011; Geruso and Royer, 2018; McCrary and Royer, 2011), others find that education can reduce completed fertility (Cygan-Rehm and Maeder, 2013; Fort et al., 2016; León, 2006). Mother's education can have a direct impact on their children's health. Evidence from industrialized countries, however, found positive (Behrman and Rosenzweig, 2002; Currie and Moretti, 2003) or no effect of education over infant health outcomes (Lindeboom et al., 2009; McCrary and Royer, 2011). In developing countries, the majority of the papers find that parental education reduces infant and child mortality (for instance, Breierova and Duflo (2004) in Indonesia, Makate and Makate (2016) in Malawi or Grépin and Bharadwaj (2015) in Zimbabwe) and improves infant health at the moment of delivery ( Chou et al. (2010) in Taiwan and Güneş (2015) in Turkey).

compulsory education. On the other hand, individuals born during the last months of the year would have incentives to finish the last year of primary education, as they were not old enough to legally work before that. In 1980, when the reform was passed, this difference in incentives between those born at the beginning and the end of the year disappear. 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.

This reform took place in 1980, just a few years after the end of Franco's dictatorship which had lasted almost 40 years. During the dictatorship, Spain was a male-dominated society, were women's right were greatly ignored or suppressed. This meant that very few women had access to higher education, and women's labor market participation rates were low. The end of the dictatorship increased the level of 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 was effective at increasing educational attainment of both men and women. In particular, they find that the reform reduced the number of early school leavers (individuals not finishing compulsory education) by 7.6% in the case of men, and by 11% in the case of women. They also find a positive effect in the probability of attaining post-compulsory education. The reform decreased by 3.3% for men and 2.7% for women, the number of individuals that do not attain any level of post-compulsory education.

We find that the reform prompted a postponement of first births by one month on average. However, we show that this postponement is followed by a catching-up effect and the reform had no effect on completed fertility. More interesting, we show that the reform was detrimental for 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 less grams at birth after the reform.

We propose three different channels through which the reform could be negatively impacting 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[2] and the incidence of mul-

[2] Pregnant women with more than 35 years old have a higher risk of pregnancy complications and poor infant

tiple births.[3] The second channel operates through changes in maternal marital status, increasing the number of children without registered fathers. [4] Yet, the detrimental effect on infant health does not disappear when we control for father's presence at the moment of delivery. This indicates that maternal marital status is not a major mechanism. The third channel that we propose is changes in unhealthy habits of affected women, which if maintained during pregnancy could have contributed to the reported negative effects. More precisely, we find that the reform increased the probability of smoking and alcohol consumption for treated women. This last channel is a direct effect of the gender equalization that women were experiencing at the moment of the reform. More educated women undertook more unhealthy behaviors, despite the health cost for them and their children's .

When we analyze the effect of the reform on men's fertility decisions and infant health outcomes of their children, we find a similar postponement effect in fertility to the one observed for women. However, the reform had no effect on infant health outcomes of affected men's children. This important result is consistent with the theory that the mechanism through with the child labor reform is affecting infant health is 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 to explain the negative effect of the reform over 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 for educated mothers it is possible to reverse negative shocks at birth. Our data suggest that the long term reversal is 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, indistinguishable. 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 the universal public health coverage. This double coverage allows beneficiaries to avoid the system

health outcomes (Ziadeh, 2002; Astolfi and Zonta, 2002). Delayed child-bearing has been found to be 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).

[3] Multiple birth children normally have worse infant health outcomes at the moment of delivery compared with single birth children.

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

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 of education and fertility and infant health outcomes in middle-income countries that are experiencing a gender equalization process. Previous evidence on the causality between education and fertility and infant health have 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 that we are exploiting in this paper took place when Spain was a middle-income country with a large percentage of its population achieving low levels of education, high levels of labor market participation at early ages, and low levels of women participating in the labor market at their prime age.

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 month of birth. 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 rely on the assumption that individuals born in different months are equal. The only assumption we are making is that any existing differences between those born at the beginning and at the end of the year remain constant for the cohorts before and after the reform.

Thirdly, as far as we are aware of, 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 compulsory schooling laws as an instrument for years of education. Child labor reforms differ from compulsory schooling reforms in many aspects. For one, the type of individuals affected will be different with each type of reform. Compulsory schooling reforms will force children to stay in the educational system, increasing educational attainment across the board (if correctly applied). A child labor reform, on the other hand, will only act as a subtle incentive to continue studying. This means child labor reforms will lead to the increase in educational attainment mostly for children whose main motivation to drop out relates to the need to contribute to the household income by working. Therefore, the compliers of these two types of laws are different, and this should be taken into account when interpreting the results.

Fourthly, unlike most of the extant literature, we use registered data of all births in Spain, which allows us to observe the universe of all births that took place during more than 30 years. These data will allow us not only to examine completed fertility (instead of focusing on teenage fertility as most of the previous literature), but also 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, death of the mother, or the emancipation of older children can have an impact on 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 present the robustness checks. In Section 7, we show the main explanatory mechanisms behind our main results. In Section 8 we present evidence on the medium-term effects of the reform for 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 their population participating in the labor market at an early age. In the late 1970s, 40 (30) percent of 15-years-old and 15 (10) percent of 14 years-old boys (girls) were participating in the formal labor market (Labor Force Survey). Moreover, 30 (20) and 10 (8) percent of boys (girls) were formally working at the age of 15 and 14, respectively. These percentages are important if we take into account that a substantial part of the employment of children under the age of 16 was probably in the informal market and, thus, not captured in the Spanish Labor Force Survey. In fact, the Spanish Household Budget Survey of 1980/1981 (Alonso-Colmenares et al., 1999) reveals that, after the reform, only 2.1 (1.2) percent of 14-years-old and 0.63 (5.1) percent of the 15-years-old boys (girls) participated, formally or informally, in the labor market. Thus, the reform not only eliminated child formal work, but also reduced substantially informal child employment.

Yet, 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: 3 years pre-school, 8 years of primary education, 4 years of secondary education, and tertiary education. This law established compulsory education until the age of 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. In Spain all children from the same cohort start school the calendar year they turn 6 years-old. Consequently, some children were 5 years old when they started primary school, while others started with 6 years old and are thus a bit older (in months). At the same time, some children finished primary education at 13, while other finished 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.[5]

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 and women's participation in the labor market was very low. In 1975 only 27.9% ( 34.5% in 1985) of working-age women were participating in the labor market in Spain (World Bank, 2009). The end of the dictatorship increased the level of gender equality and improved women's access to economic opportunities (Philips, 2010). 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). For instance, before 1980, more educated women had the larger smoking prevalence than women with fewer years of education (Bilal et al., 2015). As women had the opportunity of entering in 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 cost. This positive correlation between unhealthy behaviors and education for Spanish women is gradually reversed until the cohorts of women born after 1980, when it begins to mirror that of developed countries, with less educated women showing the highest smoking and drinking prevalence rates.

[5]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).

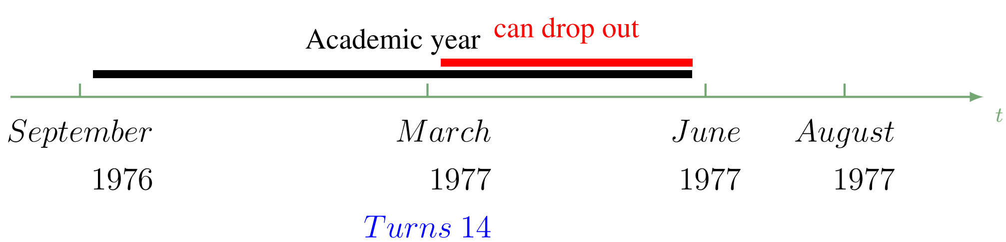
# 3 Identification strategy

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

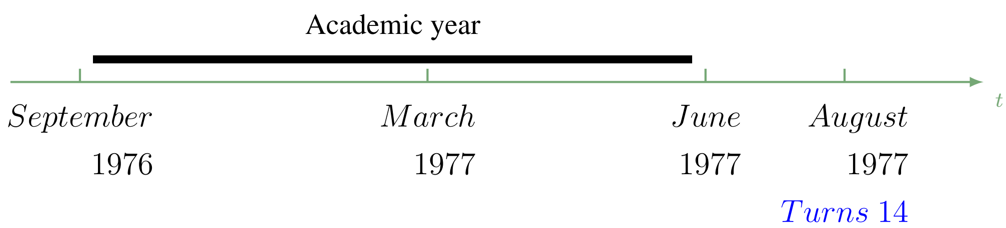
First, individuals born after 1966, who were 14 at the time the reform was passed, could not start working until they turned 16 years old, while individuals born before 1966 could start working at the age of 14. Additionally, individuals from the same cohort were affected differently by the reform depending on whether they were born at the beginning or the end of the year. Before the reform, students born during the first months of the year turned 14 before finishing their last year of primary education, and had incentives to leave the educational system without completing it. Instead, students born during the last months of the year were not old enough (had not turned 14 years old) to start legally working before finishing primary education. After the reform, this difference in incentives disappears. The reform increased the legal working age to 16 years old, but the compulsory schooling age remained at 14. Thus, after the reform, all individuals in the same cohort had similar incentives to complete the last year of primary education as they were not able to work until turning 16.

The following chart illustrates the timing of the reform by showing two individuals in the same 1963 cohort (pre- reform), during their last year of primary schooling:

1. An individual that was born on March of 1963:



2. For an individual that was born on September of 1963:




The individual that was 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 on September of 1963 was still 13 in June of 1977, when the last year of primary school finished.

We perform a within-cohort difference-in-difference strategy to identify the effect of the reform on women's fertility decisions and the (short-term and medium-term) health of the affected womens' offspring. In our identification, we are comparing women of the same cohort that only differ in the month of birth. Therefore, we will capture only the reform's within-cohort effects and our results will be robust any other concurrent events. This is important as this reform was approved during a period of significant social change in Spain.

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.[6] $Post\ Reform^{i}$ 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 in 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 ($\delta_m$, $\mu_y$, and $\theta_t$ respectively). We cluster the standard errors at the cohort level and report the wild bootstrap p-values in brackets. The effect of the reform can be identified by the coefficient of the interaction between the post-reform and the treatment dummy variables,

[6]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.

$\beta_2$.

When examining the effects of the reform over 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 ($\theta_t$ and $\gamma_n$ respectively). When examining the medium-term effects of the reform over 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 are assuming that the reform did not have any effect for the cohort of individuals that were between 14 and 16 years-old when the reform passed (individuals born in 1964, 1965 and 1966). In other words, we are assuming that when the reform was enacted these individuals, that 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.

# 4 Data and Descriptive Statistics

In order to examine the effect of the reform on the affected women's fertility and their offspring 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 between 1975 and 2018. These data are available from the Spanish National Statistics Institute and contain information about the parents and the newborn that is self-reported by parents or relatives who are compelled by law to declare the childbirth. The raw microdata contain 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.[7] We also drop births of women born in 1966 and who therefore turned 14 the year the reform took place (1980) and those of women born in January, February, June, July, November, and December. Thus, finally we observe a total of 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 certain year

[7]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.

from women born in a certain month and year by every 1,000 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 certain month and year by every 1,000 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 A1. We observe that there are on average 844 first births and 1,530 total births per 1,000 women in our sample. Moreover, Spanish women born between 1961 and 1971 were having their first child at almost 28 years old, on average.

We measure children's health at the moment of delivery using four measures: probability that the child survives the first 24 hours, the probability that the child is born after more than 37 gestational weeks,[8] birth weight (in grams) at the moment of delivery, and the probability of being born weighing less than 2,500 grams.[9] Data on birth weight and survival within the first 24 hours 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.[10] 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 A2, the reform does not have an impact on the probability of not having information on birth weight. Moreover, we analyze if the reform had any effect on the probability that women had a first multiple birth and the sex ratio (the probability of having a male first birth).[11] We only examine health at birth 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 A1, 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 hours after delivery. Moreover, around 90% of children are born with 37 or more weeks of gestation, children are born on average with 3,192 grams and 7.4% of them are born with low birth weight.

To examine the women's risky behaviors, we use two waves (2006 and 2012) of the Spanish Na-

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

[9]Babies born with less than 2,500 grams are considered to be of low birth weight by medical standards.

[10]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 don't have a selected sample.

[11]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).

tional Health Survey. This is a nationwide cross-sectional survey that collects health related information as well as the socio-economic status and habits of adults and children (up to 15 years old). The raw microdata contain 29,478 individuals. We again restrict the sample to Spanish women born in March-May or August-October of 1961-1965 or 1967-1971.[12] In our final sample, we observe a total of 2,956 women. We use this database to assess the effect of the reform on some health behavior outcomes: smoking and drinking alcohol. In Table A3, we observe that on average 77% of women drink alcohol, although only 8.4% drink alcohol daily. Moreover, 32% of the women smoke at the moment, 41% have never smoked while 23% used to smoked in the past but quit. Finally, 17% of the women that quit smoking, did not do it because of pregnancy.

For the medium-term effects on the health of the women's offspring we use two different databases. We, first, use three waves (2003, 2006 and 2012) of the Spanish National Health Survey. We restrict our sample to mothers of children aged 2 to 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 children aged 2 to 15, which are self-reported by the mother. We also use the 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. This is a cross-sectional database that contains detailed information on income, financial situation, working life, social relations and health of the household's members of those individuals that are being interviewed. The descriptive statistics of the main variables can be observed in Table A4. We use two main measures of children's health: the probability of having self-reported (subjective) good health, 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 suffer from diabetes, asthma, chronic allergies or mental disorders, while only 35% percentage of mothers self-report that their children have a 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 the probability of exercising more than once a month, the number of sleeping hours, the probability of watching TV less than 3 hours 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.

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

# 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 is compensated later, as the reform did not have any impact on women's completed fertility and total number of children each women 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 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 has a negative impact on the health of children born to affected women.[13]

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 grams less, on average, compared to children of women born at the end of the year. While 4.15 grams 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) determine that the Supplemental Program for Women, Infants and Children in the United States led to an increase in average birth weight of around 2 grams. Similarly, Almond et al. (2011) estimate that the US Food Stamp program increased the average birth weight between 2 and 5 grams. We also observe that women born at the beginning of the year have a 0.18 percentage points (2.76%) higher probability of having a first child with a low birth weight (less than 2,500 grams). We also find that the reform increased the probability of having a multiple birth in 0.217 percentage points (8.2%). This might be a consequence of the postponement of entrance into motherhood as we have shown in Table 1.

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 three potential channels through

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

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 of 1964 and February of 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 the year 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 of 1964, the variable takes a value of 0. For women born between March of 1964 and February of 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 of 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 value of 2/24 for all women born in April of 1964, and so on. We follow this rule until women that were born in February of 1966 which were affected by the reform for 23 months (the variable takes value of 23/24).

We can observe in the first regression of Tables 3, 4, 5 and 6 that the results are robust in sign and significance when this alternative specification is used. Now the estimated delay in age at which women affected by the reform have their fist child is of 39 days instead of 29. The impact over the probability of having a premature child is also very similar in magnitude. However, the effect of the reform on the probability of having a multiple birth is now a bit smaller and is not longer significant. Finally, the effect over birth weight is stronger. Now we find that that the reform decreased the average birth weight by 6.17 grams (instead of 4.15).

An alternative assumption is to consider women born in 1964, 1965 and 1966 as potential noncompliers of the law. Then, we can check the sensitivity of our results if we drop sequentially from the analysis these cohorts. The results in the second and third regressions of Tables 3, 4, 5 and 6 indicate that the effects of the reform on fertility and infant health outcomes are unchanged when we exclude these two additional cohorts.

**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 at the end of the same year are similar. Our identification is based on a weaker assumption, which is that if there any existing differences between women born at the beginning and at 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 in order to delete from the sample the potentially ”more” different individuals.

We re-estimate our results using a broader definition of our treatment and control groups that includes women born in 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. 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. The fifth regression of Tables 3, 4, 5 and 6 shows that our findings are extremely robust to this inclusion of regional dummies.

**Placebos**

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 same 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 Figure 1 , we plot the estimates of the interaction term and the 95 percent confidence interval for our main findings. We observe that for the majority of our placebos, the effect is not significant.

# 7 Explanatory Mechanisms

**The Postponement of First Births and Multiple Births**

A 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 Table 7, we further study this postponement by looking at the effect of the reform on the probability of having a first birth in different age brackets: 14-24, 25-35 or 36-48. We observe that the reform increased the probability that women have their first child after the age of 35 by 0.28 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 have negative effects on infant health as risk during pregnancy increases after that age (Ziadeh, 2002; Astolfi and Zonta, 2002). For instance, Jolly et al. (2000) find 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 child-bearing 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), and preterm delivery (Roberts et al., 1994).

Delayed child-bearing 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 compared with single birth children. We find that the reform had an effect on the probability of treated women having multiple first births, which could partially explain the detrimental effects of the reform over infant health.[14] 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.

[14]Given that the reform affects the probability of having multiple births, we cannot examine the effects of the reform on infant health outcomes excluding multiple births, as this approach will result in a selected sample.

We can examine the effect of the reform on the main outcomes of infant health controlling for the age of the mother at the moment of the birth or if 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. Table 5 and 6 shows that that controlling for multiple births does not modify the results on the probability of having a first birth with more than 37 weeks of gestation or birthweight. On the other hand, controlling for the age of the mother at the moment of birth halves these estimates, even though the effect on maturity remains negative and significant.

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 be the result of a similar postponement or reduction of marriage. In this section, we analyze the change in the marital status of mothers. 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 an effect on the marital status of the mother.[15]

Therefore, a second possible mechanism through which the reform could be detrimental for 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. Table 5 and 6 shows that that controlling for father’s presence at the moment of delivery does not modify the results on the probability of having a first birth with more than 37 weeks of gestation or birthweight. This indicates that this might not be a major mechanism behind the negative effects of the reform over infant health.

**Changes in Health Habits**

A third channel through which the reform could be affecting infant health is through changes in health habits of mothers. Table 9 shows that, after the reform, women drink alcohol more often. In

[15] This result is consistent with the lack of effect of the reform over marriage rates that we report in Table A5 of the Appendix.

particular, the reform increased by 4.8 percentage points (46%) the probability of drinking alcohol daily and decreased by 11.6 percentage points (29%) the probability of consuming alcohol less than once a month. Moreover, the reform increased by 6.8 percentage points the probability (24%) of being a current smoker and decreased by 6.8 percentage points (17%) the probability of never smoked. Although the reform did not impact the probability that these women are ex-smokers, we do find that after the reform women have a lower probability of quitting smoking during pregnancy (conditional on being ex-smokers and having kids). Therefore, these outcomes could directly affect the health of their offspring.

Interestingly, in Table A6, we do not find that the reform deteriorated the health habits of men, which indicates that the deterioration of women's health behaviors were likely linked to the gender equalization process that these women were experiencing at the moment of the reform. For women in these cohorts, access to, and social acceptance of, smoking were 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 prevalence of smoking for women cannot be considered a particular case of Spain. In many countries in the world the number of smoking women is increasing, even though smoking prevalence among 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 countries and Eastern Mediterranean countries a high smoking prevalence among high 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 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' life style.

To sum up, we find evidence that the child labor reform had a negative impact on the short-term health of children due to an increase in unhealthy behaviors of more educated mothers and to the postponement of fertility after the age of 35. The deterioration of mothers' health behaviors was a direct effect of the gender equalization process that these women were experiencing at that moment. At the same time, as we can observe in the Appendix (Table A7), the reform had no effect over 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 with the child labor reform is affecting infant health has to be related to the mother's characteristics or behaviors during pregnancy. This observation reinforces our finding that delayed child-bearing and bad behaviors during pregnancy (such as smoking) are key to explain the negative effect of the reform over infant health.

# 8 Medium-term Outcomes

Table 10 reports the medium-run effects of the reform on these children's health outcomes. As one can easily see, in the first column of Tables 10, the objective good health status seems to be affected in the opposite way. Children from mothers affected by the reform had a 5.6 higher probability of having a good objective health. The reversal of the negative 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. Parents can contribute to a better health by ensuring their offspring make 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.

More educated mothers seem to impact positively over some habits of the children. Children of mothers affected by the reform watch less TV and have better eating habits. In particular, the reform decreased by 2.8 percentage points the probability that children watch TV more than 3 hours a day, and increased by 2.9 percentage points the probability that they follow a Mediterranean diet. On the other hand, the reform had no impact on children's sleeping or exercise habits. Even though this change of habits may have contributed to the reversal of outcomes at delivery of children, 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 10, we observe that the children born from mothers affected by the reform have a 5.2 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 8 percentage points higher probability of having private insurance (in Spain, where public coverage is universal, this is something done by individuals wishing to have a premium quality care as well as quicker access to specialized care).

The picture that emerges is one of mothers who had smaller, more fragile children, and who even at age 15 still worry more about them (even if, objectively, the health does not seem to differ). 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 negative effects from birth.

# 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 (that had 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 had impacts on 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. We find that, for mothers born at the beginning of the year, the reform increased the probability of having a first child at less than 37 gestational weeks and 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's health could be partly driven by treated mothers having their first child at an older age, making their pregnancies more risky and increasing the chances of poor infant health outcomes. The second channel is changes in unhealthy habits of affected women. More precisely, we find that the reform increased the probability of smoking and drinking alcohol for treated women. Additionally, we also find that the probability of smoking cessation during pregnancy is reduced for women born at the beginning of the year after the reform.

In the Appendix, we analyze the effect of the reform on men's fertility decisions and infant health outcomes of their children. The postponement effect in fertility of men is very similar to the one observed for women. However, the reform had no effect over the infant health of the children of affected men. This important result suggests that the mechanism through which the child labor reform is affecting infant health has to be related to 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.

But these negative effects at birth disappear in the medium run. We next explore the channels that can explain this important reversal of early life negative conditions in the medium term. First, mothers contribute to a better health of their offspring 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 for the health of their children. 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 the health of their children (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 negative effects at birth during childhood.

Our results must be taken within the social context in Spain at the time of the reform, just a few years after the end of Franco's dictatorship that lasted 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 the level of gender equality and improved women's access to economic opportunities. This gender equalization process then led to a convergence of health risk factors between men and women. Therefore, our results will provide important policy implications for middle income countries that are undergoing those gender equalization processes right now.

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# Tables and Figures

Table 1: Fertility Outcomes

| | Number first births per 1,000 women | Total number births per 1,000 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 1,000 women born in each year-month, (2) the total number of births per 1,000 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

Table 2: Infant Health Outcomes

| | Infant health of the woman's first child | | | | | |
|---|---|---|---|---|---|---|
| | Male | Multiple birth | Survives first 24h | Weeks of gestation $\geq 37$ | Birth weight | Weight $<$ 2,500g |
| 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 hours 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 2,500 grams (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%. *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.

Table 3: Robustness Check: Age of the Mother at First Child

| | Age Mother First Birth | | | | |
|---|---|---|---|---|---|
| | 1964, 1965 and 1966 partially affected | Drop 1966 and 1965 | Drop 1966, 1965 and 1964 | Treated Months 1-5 Control 6-12 | Region FE |
| Treated | -0.098*** | -0.094*** | -0.130*** | -0.070** | -0.084** |
| | (0.022) | (0.020) | (0.031) | (0.026) | (0.029) |
| | [0.007] | [0.008] | [0.000] | [0.028] | [0.032] |
| Treated* Post Reform | 0.109*** | 0.103*** | 0.112*** | 0.074*** | 0.072** |
| | (0.024) | (0.027) | (0.029) | (0.018) | (0.027) |
| | [0.002] | [0.004] | [0.008] | [0.001] | [0.017] |
| Post Reform | -0.097 | | | | |
| | (0.062) | | | | |
| | [0.308] | | | | |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Region FE | | | | | ✓ |
| Observations | 1,535,301 | 1,250,301 | 1,103,481 | 2,511,592 | 1,393,937 |
| $R^2$ | 0.033 | 0.039 | 0.042 | 0.036 | 0.067 |
| Dependent Variable Mean (Pre-Reform) | 26.605 | 26.770 | 26.613 | 26.915 | 26.920 |

*Notes*: The dependent variable is the age (in years) of the woman when she had her first child. Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) assumes treated women are those born from January to May and control women those born from July to December, and (5) 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

Table 4: Robustness Check: Multiple Birth

| | Multiple Birth | | | | |
|---|---|---|---|---|---|
| | 1964, 1965 and 1966 partially affected | Drop 1966 and 1965 | Drop 1966, 1965 and 1964 | Treated Months 1-5 Control 6-12 | Region FE |
| Treated | 0.105 | 0.095 | -0.058 | 0.100 | 0.026 |
| | (0.078) | (0.071) | (0.128) | (0.100) | (0.098) |
| | [0.257] | [0.242] | [0.672] | [0.302] | [0.804] |
| Treated* Post Reform | 0.099 | 0.165* | 0.178 | 0.254*** | 0.218** |
| | (0.093) | (0.084) | (0.094) | (0.073) | (0.092) |
| | [0.348] | [0.086] | [0.133] | [0.009] | [0.038] |
| Post Reform | 0.591 | | | | |
| | (0.694) | | | | |
| | [0.701] | | | | |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Region FE | | | | | ✓ |
| Observations | 1,535,301 | 1,250,301 | 1,103,481 | 2,511,592 | 1,393,937 |
| $R^2$ | 0.027 | 0.028 | 0.028 | 0.028 | 0.028 |
| Dependent Variable Mean (Pre-Reform) | 2.435 | 2.547 | 2.438 | 2.623 | 2.633 |

*Notes*: The dependent variable is the probability that the woman has a first multiple birth (multiplied by 100). Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) assumes treated women are those born from January to May and control women those born from July to December, and (5) 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

# Table 5: Robustness Check: Mature First Birth

| | Weeks of Gestation $\geq 37$ | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 1964, 1965 and 1966 partially affected | Drop 1966 and 1965 | Drop 1966, 1965 and 1964 | Treated Months 1-5 Control 6-12 | Region FE | Control for age mother | Control for multiple birth | Control for registered father |
| Treated | 0.111 | 0.105 | 0.031 | -0.029 | -0.029 | 0.021 | 11.461 | 0.010 |
| | (0.069) | (0.082) | (0.109) | (0.073) | (0.070) | (0.072) | (38.144) | (0.068) |
| | [0.115] | [0.230] | [0.789] | [0.697] | [0.745] | [0.779] | [0.767] | [0.887] |
| Treated* Post Reform | -0.237*** | -0.227*** | -0.188** | -0.234*** | -0.216*** | -0.120* | -0.209*** | -0.207*** |
| | (0.051) | (0.055) | (0.050) | (0.047) | (0.049) | (0.063) | (0.053) | (0.053) |
| | [0.004] | [0.004] | [0.016] | [0.000] | [0.000] | [0.099] | [0.001] | [0.001] |
| Post Reform | -0.159 | | | | | | | |
| | (0.121) | | | | | | | |
| | [0.238] | | | | | | | |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Month-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Region FE | | | | | ✓ | | | |
| Observations | 1,535,301 | 1,250,301 | 1,103,481 | 2,511,592 | 1,393,937 | 1,393,937 | 1,393,937 | 1,393,937 |
| $R^2$ | 0.009 | 0.009 | 0.009 | 0.009 | 0.014 | 0.072 | 0.009 | 0.009 |
| Dependent Variable Mean (Pre-Reform) | 90.107 | 90.223 | 90.099 | 90.370 | 90.362 | 90.362 | 90.362 | 90.362 |

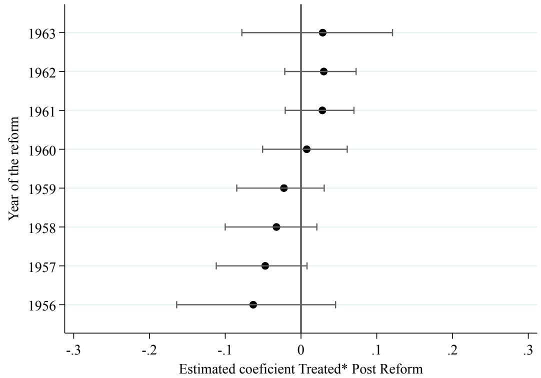
*Notes*: The dependent variable is the probability that the woman has a first child with more than 37 weeks of gestation (multiplied by 100). Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) assumes treated women are those born from January to May and control women those born from July to December, (5) include regional FE, (6) controls for the age of the mother at the moment of delivery, (7) controls if the birth is single or multiple, and (8) 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

# Table 6: Robustness Check: Birth Weight

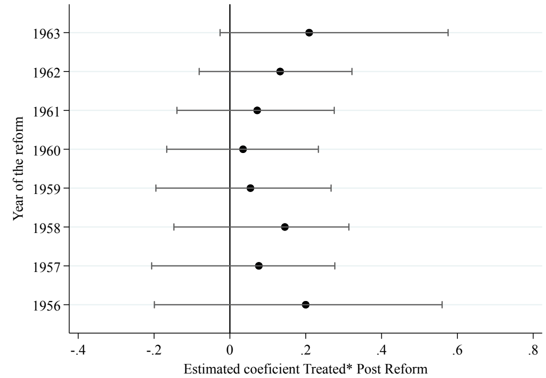
| | Birth Weight | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 1964, 1965 and 1966 partially affected | Drop 1966 and 1965 | Drop 1966, 1965 and 1964 | Treated Months 1-5 Control 6-12 | Region FE | Control for age mother | Control for multiple birth | Control for registered father |
| Treated | 4.931* | 4.042 | 2.743 | 1.204 | 3.489 | 3.270 | -1229.063** | 2.751 |
| | (2.201) | (2.421) | (1.571) | (2.628) | (1.925) | (2.360) | (513.339) | (1.988) |
| | [0.087] | [0.141] | [0.094] | [0.680] | [0.094] | [0.254] | [0.043] | [0.211] |
| Treated* Post Reform | -6.175*** | -5.144** | -6.966** | -4.783 | -4.004** | -2.246 | -4.161** | -4.059** |
| | (1.376) | (1.868) | (1.394) | (2.158) | (1.548) | (2.214) | (1.675) | (1.650) |
| | [0.004] | [0.023] | [0.016] | [0.105] | [0.020] | [0.426] | [0.023] | [0.023] |
| Post Reform | 21.284** | | | | | | | |
| | (6.949) | | | | | | | |
| | [0.062] | | | | | | | |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Month-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Region FE | | | | | ✓ | | | |
| Observations | 1,207,357 | 963,975 | 841,292 | 1,953,792 | 1,085,865 | 1,085,865 | 1,085,865 | 1,085,865 |
| $R^2$ | 0.010 | 0.011 | 0.011 | 0.011 | 0.012 | 0.120 | 0.010 | 0.011 |
| Dependent Variable Mean (Pre-Reform) | 3226.377 | 3221.523 | 3227.268 | 3217.073 | 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) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) assumes treated women are those born from January to May and control women those born from July to December, (5) include regional FE, (6) controls for the age of the mother at the moment of delivery, (7) controls if the birth is single or multiple, and (8) 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

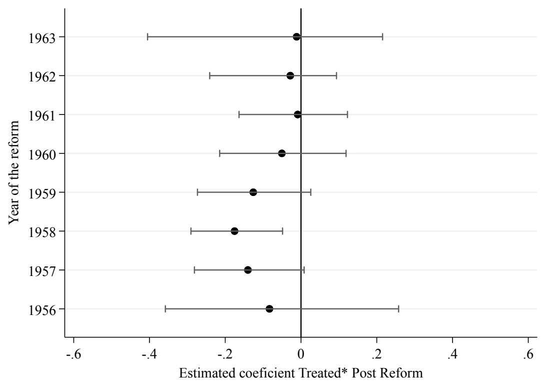
Figure 1: Placebos



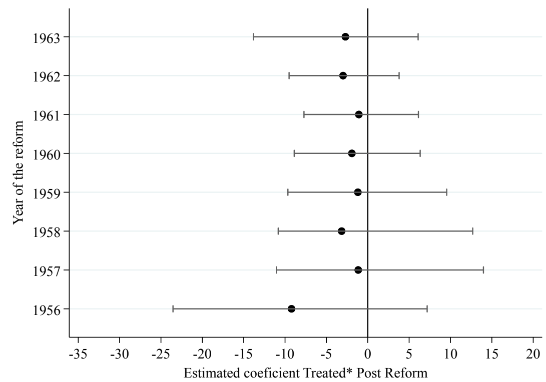

(a) Age at which women had their first birth




(b) Probability of having a multiple births




(c) Probability of mature first birth




(d) Birth weight of first child

*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.

Table 7: Probability of Having the First Birth at a Certain Age Bracket

| | First birth between the ages | | |
|---|---|---|---|
| | 14-24 | 25-35 | 36-48 |
| Treated | 0.545* | -0.506* | -0.039 |
| | (0.233) | (0.253) | (0.122) |
| | [0.089] | [0.098] | [0.754] |
| Treated* Post Reform | -0.402 | 0.122 | 0.280** |
| | (0.236) | (0.244) | (0.105) |
| | [0.134] | [0.601] | [0.033] |
| Mother Birth-Year FE | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ |
| Observations | 1,393,937 | 1,393,937 | 1,393,937 |
| $R^2$ | 0.023 | 0.008 | 0.009 |
| Dependent Variable Mean (Pre-Reform) | 39.241 | 53.964 | 6.794 |

*Notes*: The dependent variables are the probability of having a first child between the ages of (1) 14 and 24, (2) 25 and 35, and (3) 36 and 48. 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%. *Source*: Birth registries (1975-2018), all women from cohorts 1961-1965 and 1967-1971.

# Table 8: Marital Status of Mothers

| | 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%. *Source*: Birth registries (1975-2018), first children of women from cohorts 1961-1965 and 1967-1971.

## Table 9: Risky Health Behaviors

| | Risky Health Behaviors | | | | | | |
|---|---|---|---|---|---|---|---|
| | Ever drank alcohol | Drinks alcohol daily | Drinks alcohol less once month | Smokes | Never smoked | Ex-smoker | Pregnancy as motive to quit smoking |
| treat | 0.008 | -0.041** | 0.042 | -0.065** | 0.056** | 0.015 | 0.118** |
| | (0.040) | (0.019) | (0.029) | (0.020) | (0.014) | (0.014) | (0.031) |
| | [0.799] | [0.020] | [0.159] | [0.041] | [0.039] | [0.368] | [0.017] |
| treatpost | -0.013 | 0.048** | -0.116*** | 0.078** | -0.068** | -0.029* | -0.174* |
| | (0.049) | (0.021) | (0.031) | (0.023) | (0.025) | (0.016) | (0.069) |
| | [0.806] | [0.036] | [0.010] | [0.012] | [0.045] | [0.099] | [0.051] |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Interview Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 2,949 | 2,029 | 2,029 | 2,956 | 2,956 | 2,956 | 683 |
| $R^2$ | 0.012 | 0.026 | 0.027 | 0.011 | 0.016 | 0.008 | 0.064 |
| Dependent Variable Mean (Pre-Reform) | 0.770 | 0.104 | 0.395 | 0.325 | 0.396 | 0.246 | 0.115 |

*Notes*: The dependent variables are (1) the probability that the woman has ever drunk alcohol, (2) the probability that the woman consumes alcohol daily, (3) the probability of that the woman consumes alcohol less than once a month, (4) the probability that the woman smokes, (5) the probability of never have smoke, (6) the probability that the woman is an ex-smoker, and (7) the probability that the woman has quitted smoking during pregnancy, conditional on being an ex-smoker. 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%. *Source*: Spanish National Health Survey (2006 and 2012), all women from cohorts 1961-1965 and 1967-1971.

## Table 10: 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 hours a day | Mediterranean diet |
|---|---|---|---|---|---|---|---|---|
| Treated | 0.039 | 0.004 | 0.009 | 0.005 | 0.022 | -0.086 | 0.033* | -0.007 |
| | (0.024) | (0.025) | (0.011) | (0.032) | (0.021) | (0.065) | (0.016) | (0.019) |
| | [0.100] | [0.882] | [0.454] | [0.861] | [0.353] | [0.230] | [0.095] | [0.725] |
| Treated* Post Reform | -0.052* | 0.056* | 0.007 | 0.080** | -0.017 | 0.116 | -0.028*** | 0.029** |
| | (0.022) | (0.025) | (0.020) | (0.031) | (0.031) | (0.113) | (0.008) | (0.014) |
| | [0.066] | [0.065] | [0.768] | [0.013] | [0.611] | [0.363] | [0.003] | [0.018] |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Interview Year FE | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Children's Age Controls | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Children's Gender | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Observations | 3,094 | 3,090 | 3,094 | 1,036 | 3,077 | 3,091 | 2,680 | 3,077 |
| $R^2$ | 0.062 | 0.029 | 0.011 | 0.019 | 0.174 | 0.218 | 0.081 | 0.014 |
| Dependent Variable Mean (Pre-Reform) | 0.332 | 0.822 | 0.041 | 0.170 | 0.627 | 9.244 | 0.061 | 0.046 |

*Notes*: The dependent variables are the probability that (1) the mother self-reports that her child has very good health, (2) the child not suffer from diabetes, asthma, mental disorders or chronic allergies, (3) the child had to go to the hospital in the last 12 months, (4) the family has a private insurance, (5) the child exercise at least once a month, (6) the number of hours that the child sleeps, (7) the child watchs tv more than 3 hours a day, and (8) the child has a mediterranean diet (eats fruit, vegetables and milk every day and legums and fish at least three times a week). Regressions (1-3 and 5-7) include year of interview, mother's year and month of birth dummies, children's age and age squared variables 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%. *Source*: For regressions (1-3 and 5-7): Spanish National Health Survey (2003, 2006 and 2012), all children aged 2-15 from mothers' born in 1961-1965 and 1967-1971. 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.

# Appendix Tables and Figures

Table A1: Descriptive Statistics of the Birth Registries

| | Mean | Std. Dev. | Min | Max | Observations |
|---|---|---|---|---|---|
| Number of first births per 1,000 women | 844.182 | 18.574 | 805.816 | 885.461 | 60 |
| Total number of births per 1,000 women | 1530.804 | 60.375 | 1418.962 | 1652.663 | 60 |
| Age women at first birth | 27.891 | 5.889 | 14 | 48 | 1393937 |
| First birth between ages 14-24 | 33.168 | 47.082 | 0 | 100 | 1393937 |
| First birth between ages 25-35 | 57.630 | 49.414 | 0 | 100 | 1393937 |
| First birth between ages 36-48 | 9.202 | 28.905 | 0 | 100 | 1393937 |
| First birth with registered father | 97.298 | 16.215 | 0 | 100 | 1393937 |
| Married mother | 85.877 | 34.826 | 0 | 100 | 1393937 |
| Male first birth | 51.696 | 49.971 | 0 | 100 | 1393937 |
| Multiple births | 3.318 | 17.910 | 0 | 100 | 1393937 |
| Survive first 24h | 99.822 | 4.219 | 0 | 100 | 1217395 |
| Mature first birth | 90.827 | 28.864 | 0 | 100 | 1393937 |
| Birth weight of first birth | 3192.469 | 514.856 | 300 | 6590 | 1085865 |
| Weight less 2500 | 7.428 | 26.222 | 0 | 100 | 1085865 |

*Source*: Birth registries (1975-2018), all births of Spanish women from cohorts 1961-1965 and 1967-1971.

## Table A2: Missing Birth Weight

| | Weight missing |
|---|---|
| Treated | 0.000 |
| | (0.002) |
| | [0.997] |
| Treated* Post Reform | 0.001 |
| | (0.002) |
| | [0.721] |
| Mother Birth-Year FE | ✓ |
| Mother Birth-Month | ✓ |
| Child Birth-Year FE | ✓ |
| Child Month-Year FE | ✓ |
| Observations | 1,393,937 |
| $R^2$ | 0.111 |
| Dependent Variable Mean (Pre-Reform) | 0.165 |

*Notes*: The dependent variable is the probability that the first birth has not registered birth weight. The regression includes mother's year and month of birth and the children's year and moth 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%. *Source*: Birth registries (1980-2018), all women from cohorts 1962-1965 and 1967-1971.

Table A3: Descriptive Statistics of Women's Risky Health Behaviors

| | Mean | Std. Dev. | Min | Max | Observations |
|---|---|---|---|---|---|
| Ever drank alcohol | 0.777 | 0.417 | 0 | 1 | 2949 |
| Drinks alcohol daily | 0.084 | 0.278 | 0 | 1 | 2029 |
| Drinks alcohol less than once a month | 0.418 | 0.493 | 0 | 1 | 2029 |
| Smokes | 0.324 | 0.468 | 0 | 1 | 2956 |
| Nover smoked | 0.413 | 0.493 | 0 | 1 | 2956 |
| Ex-smoker | 0.231 | 0.422 | 0 | 1 | 2956 |
| Pregnancy as motive to quit smoking | 0.174 | 0.380 | 0 | 1 | 683 |

*Source*: Spanish National Health Survey (2006 and 2012), all Spanish women from cohorts 1962-1965 and 1967-1971.

Table A4: Descriptive Statistics of Children's Medium-term Outcomes

| | Mean | Std. Dev. | Min | Max | Observations |
|---|---|---|---|---|---|
| Good subjective health | 0.357 | 0.479 | 0 | 1 | 3435 |
| No diabetes, asthma, mental or alergies | 0.820 | 0.384 | 0 | 1 | 3431 |
| Hopital visit in the last 12 months | 0.044 | 0.205 | 0 | 1 | 3435 |
| Exercise more once month | 0.584 | 0.493 | 0 | 1 | 3415 |
| Sleep hours | 9.444 | 1.329 | 1 | 19 | 3432 |
| More 3 hours tv/day | 0.072 | 0.259 | 0 | 1 | 2969 |
| Mediterranean diet | 0.045 | 0.208 | 0 | 1 | 3416 |
| Private insurance | 0.108 | 0.311 | 0 | 1 | 1036 |

*Source*: Spanish National Health Survey (2003, 2006 and 2012), all children aged 2-15 from mothers' born in 1961-1965 and 1967-1971 and European Community Household Panel (2000), families with at least one child where the mother is born in 1961-1965 and 1967-1971.

## Table A5: Marriage

| | Number first marriages per 1,000 women | Total number marriages per 1,000 women | Age first marriage |
|---|---|---|---|
| Treated | 17.851** | 18.297** | -0.041 |
| | (6.389) | (7.234) | (0.035) |
| | [0.016] | [0.030] | [0.327] |
| Treated* Post Reform | 2.667 | 1.026 | 0.085** |
| | (5.132) | (5.648) | (0.035) |
| | [0.572] | [0.846] | [0.043] |
| Woman Birth-Year FE | ✓ | ✓ | ✓ |
| Woman Birth-Month FE | ✓ | ✓ | ✓ |
| Observations | 60 | 60 | 1,320,691 |
| $R^2$ | 0.475 | 0.456 | 0.027 |
| Dependent Variable Mean (Pre-Reform) | 800.310 | 832.716 | 25.139 |

*Notes*: The dependent variables are (1) the number of first marriages per 1,000 women born in each year-month, (2) the total number of marriages per 1,000 women born in each year-month, and (3) the age (in years) of the woman when she got married for the first time. Regressions include women'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%. *Source*: Marriage Registries (1976-2018), all women from cohorts 1961-1965 and 1967-1971.

## Table A6: Risky Health Behaviors of Men

| | Risky Health Behaviors | | | | | |
|---|---|---|---|---|---|---|
| | Ever drank alcohol | Drinks alcohol daily | Drinks alcohol less once month | Smokes | Never smoked | Ex-smoker |
| treat | 0.011 | 0.005 | 0.016 | -0.034 | 0.021 | 0.015 |
| | (0.024) | (0.028) | (0.013) | (0.031) | (0.034) | (0.014) |
| | [0.565] | [0.880] | [0.291] | [0.353] | [0.626] | [0.413] |
| treatpost | 0.004 | -0.030 | -0.058** | 0.069 | -0.052 | -0.029 |
| | (0.031) | (0.032) | (0.022) | (0.052) | (0.051) | (0.024) |
| | [0.913] | [0.398] | [0.024] | [0.222] | [0.334] | [0.284] |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Interview Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 2,438 | 2,074 | 2,074 | 2,441 | 2,441 | 2,441 |
| $R^2$ | 0.005 | 0.057 | 0.040 | 0.005 | 0.028 | 0.020 |
| Dependent Variable Mean (Pre-Reform) | 0.908 | 0.242 | 0.237 | 0.392 | 0.287 | 0.286 |

*Notes*: The dependent variables are (1) the probability that the man has ever drunk alcohol, (2) the probability that the man consumes alcohol daily, (3) the probability of that the man consumes alcohol less than once a month, (4) the probability that the man smokes, (5) the probability of never have smoke, and (6) the probability that the man is an ex-smoke. Regressions include year of interview, year of birth and month of birth dummies. Treated individuals are those men born from March to May, and the control are those born from August to October. Men 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%. *Source*: Spanish National Health Survey (2006 and 2012), all men from cohorts 1961-1965 and 1967-1971.

## Table A7: Fertility and Infant Health Outcomes for Men

| | Fertility for men | | | Infant health of the man's first child | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Number first births per 1,000 men | Total number births per 1,000 men | Age first birth | Male | Multiple birth | Survives first 24h | Weeks of gestation $\geq$ 37 | Birth weight | Weight $<$ 2,500g |
| Treated | 21.181** | 43.370*** | -0.124*** | 0.179 | -0.009 | 0.028** | 0.008 | -0.427 | 0.155 |
| | (6.836) | (9.879) | (0.008) | (0.150) | (0.105) | (0.009) | (0.137) | (1.844) | (0.147) |
| | [0.009] | [0.003] | [0.000] | [0.280] | [0.953] | [0.046] | [0.953] | [0.950] | [0.432] |
| Treated* Post Reform | -1.629 | -2.797 | 0.117*** | -0.013 | 0.021 | -0.000 | -0.025 | -2.251 | -0.030 |
| | (2.579) | (5.319) | (0.026) | (0.203) | (0.071) | (0.008) | (0.083) | (2.414) | (0.107) |
| | [0.483] | [0.547] | [0.001] | [0.965] | [0.809] | [0.968] | [0.758] | [0.420] | [0.770] |
| Mother Birth-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Birth-Month | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Birth-Year FE | | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Child Month-Year FE | | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 60 | 60 | 1,288,472 | 1,288,472 | 1,288,472 | 1,156,355 | 1,288,472 | 1,054,212 | 1,054,212 |
| $R^2$ | 0.686 | 0.868 | 0.028 | 0.000 | 0.019 | 0.000 | 0.004 | 0.006 | 0.007 |
| Dependent Variable Mean (Pre-Reform) | 796.234 | 1454.790 | 30.115 | 51.621 | 2.976 | 99.836 | 91.011 | 3201.595 | 6.955 |

*Notes*:
The dependent variables are (1) the number of first births per 1,000 men born in each year-month, (2) the total number of births per 1,000 men born in each year-month, (3) the age (in years) of the man when she had her first child, (4) the probability that the first birth is a boy (multiplied by 100), (5) the probability of having multiple births (multiplied by 100), (6) the probability of having a first child that survives the first 24 hours after delivery (multiplied by 100), (7) the probability of having a first child with more than 37 weeks of gestation (multiplied by 100), (8) the weight at birth (in grams) of the man's first child and, (9) the probability that the first child is born with less than 2,500 grams (multiplied by 100). Regressions (1-3) include father's year of birth and month of birth fixed effects, while regression (4-9) also control for child's year and month of birth fixed effects. Treated individuals are those men born from March to May, and the control are those born from August to October. Men 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%.
*Source*: Birth registries (1975-2018), all men from cohorts 1961-1965 and 1967-1971.