# The effect of increasing the legal working age on women's fertility and infant health *

Cristina Bellés-Obrero[1], Sergi Jiménez-Martín[1,2,3] and Judit Vall-Castello[4]

[1]Department of Economics, Universitat Pompeu Fabra,
[2]Barcelona GSE
[3]FEDEA
[4]Center for research in Economics and Health, Universitat Pompeu Fabra

**Abstract**

We use an exogenous variation in the Spanish legal working age from 14 to 16 to investigate the effect of education on fertility and infant health. We show that the reform increased educational attaintment, which led to a decrease in marriage and fertility. The reform is also detrimental for the health of the offspring at the moment of delivery. We document three channels contributing to this detrimental effect: the postponement in age of delivery, the change in the maternal marital status, and a higher probability of having a high skill job of more educated women, which increases the likelihood of engaging in unhealthy behaviors.

JEL-CODES: J81, I25, I12, J13
KEYWORDS: minimum working age, education, health, fertility, child health

*We gratefully acknowledge the support from project ECO2014-52238-R. We thanks the participants of the following seminars and gatherings: UPF, BGSE Jamboree (2015), CRES, 29th Annual Conference of the European Society for Population Economics, Workshop on Applied Microeconomics and Microeconometrics (Universidad de Alicante) and Workshop on Health Economics (Universidad de Alicante). The paper was previously circulating by the title: "The unintended effects of increasing the legal working age on family behaviours". The usual disclaimer applies.

Decreasing fertility rates are one factor contributing to the ageing of the population, a major concern in many industrialized countries due to the increased pressure on the sustainability of their social security systems. Many countries in 2005-2010 have a total fertility rate (TFR) below replacement, 2.1 births per woman. For instance, the TFR is 1.5 births per woman in Europe and 1.4 births per woman in Japan. Lately many governments enacted policies to raise fertility in order to slow population aging[1]. Some researchers have pointed to the role of education in explaining the reduction in fertility rates. If this is the case, the impact of education on fertility should be considered a nonpecuniary return of education. Moreover, if parents' education affects the health outcomes of their children, there will be intergenerational education spillovers that should be considered when designing changes in the educational system.

An extensive literature ( León (2006), Black et al. (2008), Fort (2007), Monstad et al. (2008), Fort et al. (2011), Silles (2011), Cygan-Rehm and Maeder (2013), Geruso et al. (2014) ) has examined the relation between education and fertility rates. Although a negative correlation between these two variables has been well established, the debate about whether this relation is causal continues. Many researchers have argued that the presence of unobserved characteristics could be affecting schooling choices and the decision to have children, while others have pointed to potential reverse causality.

In this paper, we investigate the effect of education on fertility and infant health. To mitigate the potential endogeneity of education, we exploit a reform that introduced an exogenous variation in the Spanish legal working age. This strategy is in stark contrast to recent literature that has used, instead, changes in compulsory schooling laws as a source of exogenous variation on individual schooling choices. For instance, Black et al. (2008) exploit several reforms in compulsory schooling, both in Norway and the United States, and find a significant negative effect on the probability of having a child as a teenager. 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 et al. (2014)).

However, the evidence on the impact of education on the completed fertility rate and the probability of remaining childless is contradictory. On the one hand, Monstad et al. (2008) examine an educational reform in Norway and find that years of schooling has no effect on the probability of women remaining childless or having fewer children. Similarly, Fort (2007), Silles (2011) and Geruso et al. (2014) show that women catch up with the fertility delay in their early twenties. These papers, then, argue that education has mainly an "incarceration effect," delaying but not reducing completed fertility. On the other hand, Cygan-Rehm and Maeder (2013) exploit an exogenous variation caused by a German compulsory schooling reform which increased the amount of education received and find a reduction in completed fertility. They attribute this lack of catching-up effect to the particularly high opportunity costs of childbearing in Germany. León (2006) similarly examines compulsory schooling laws in the United States and shows that education causally reduces completed fertility. Finally, in contrast to the rest of the literature, Fort et al. (2011) find that education increases completed fertility and reduces the incidence of childlessness. The au-

[1]Source: Lee et al. (2014). For instance, France has increased its fertility rates from 1.74 to 2.08 through some pro-natalist initiatives, such as tax deductions for dependents and paid maternity leave financed through the national health insurance system.

thors exploit several compulsory schooling reforms in Europe and explain their results by arguing that compulsory schooling reforms target women at the lower end of the educational distribution who are more likely to have grown up in larger and poorer families. Thus, for these women, the positive association between years of schooling and income might outweigh the substitution effect of education on childbearing.

Education can, furthermore, affect child outcomes. Several empirical studies have analyzed the causal effect of mothers' education on the health of their offspring at the moment of delivery. If a mother's education does indeed affect child health outcomes at birth, it will also have an impact on productivity and education for these children. In this case, we should consider that the increase in education could have important intergenerational spillovers. Evidence from industrialized countries, however, found positive or no effect of education over infant health outcomes. Behrman and Rosenzweig (2002) find an improvement in child health outcomes through the channel of increasing the household budget constraint (higher earnings). Currie and Moretti (2003) also identify a positive impact of mothers' education on children health. However they propose another channel through which education affects infant health: differences in maternal behavior during pregnancy. More explicitly, they find a positive correlation between maternal education and a decreased incidence of smoking during pregnancy. In contrast, a number of studies have found no causal link between education of the mother and the health of her children (McCrary and Royer (2011)).

Finally, a recent strand of the literature has stressed the role of the woman's marriage market as a possible channel through which education might impact fertility. Education could reduce the likelihood that a woman marries and starts a family. Along these lines, previous literature has found a positive association between education and postponement of marriage, which is consistent with a short-run effect of staying in school longer. For example, Duflo et al. (2011) investigate the effects of an educational program in Kenya that provided free school uniforms, lowering the cost of education. They find that this program reduced the probability of girls being married two years later. Kırdar (2009) provides similar results from an increase in compulsory schooling in Turkey and Breierova and Duflo (2004) from an analysis of a large school construction program in Indonesia.

Education seems to have little effect on the probability of an individual being married later in life, however. Fort (2007) exploits an increase in the minimum school-leaving age in Italy and find no effect of education on age at first marriage between 18 and 26. This same result is found by Breierova and Duflo (2004) in Indonesia, Lefgren and McIntyre (2006) in the United States, and Anderberg et al. (2013) in the UK.

The majority of this literature examines only one type of family behavior outcome. In this paper, however, we explore the impact of education on three family behavior outcomes at the same time, given the important interactions among them. To untangle the possible endogeneity of education and fertility, marriage, and infant health, we take advantage of a quasi-natural experiment. In 1980, the new Workers Statute (Law 8/1980) changed the minimum legal age to work in Spain from 14 to 16 years old. This reform took place a few years after the democratization of Spain following the end of the dictatorial regime. Thus, at that time, Spain was still a developing country with a large percentage of its population achieving low levels of education and participating in the labor market from an early age. The top panel of Figure 1 displays the labor force participation and employment rates for children of 14 and 15 years old, five years before the implementation of the reform. In 1976, around 40 percent of males and 30 percent of females were already participating in the labor market at age 15. This percentage remained high until 1980. Participation rates for children age 14 were lower. Around 15 percent of males and 10 percent of females were working at age 14 during the early 1970s. These percentages dropped to 10 and 5 percent, respectively, in the first quarter of 1980 (the last period for which we have estimates of labor force participation for children under 16 years old from the Labor Force Survey). Although a substantial part of this employment was in the informal market, in the bottom panels of Figure 1 we can see that an important portion of children younger than 16 were already participating in the formal market before the 1980 reform. These bottom panels show the distribution of the age of the first Social Security contribution for pre- and post-reform cohorts. Prior to the reform, the cohorts born between 1961 and 1965, 9.22 percent of boys and 7.57 percent of girls started working in the formal labor market before age 16. After the reform, these numbers drop dramatically, with almost no one contributing before the age of 16. Thus, one third of the employment of children under 16 years old was in the formal market, and the reform mainly eliminated child formal work and likely reduced the informal part of child employment (under 16 years old).

Most of the extant literature that focuses on the impact of education on several outcomes has used changes in the state compulsory schooling laws as an instrument for years of education. Thus, our approach is different on several dimensions. First, an educational reform that increases the number of compulsory years of education is typically accompanied by other changes in the educational system. Thus, it is difficult to disentangle the effect of a simple increase in the number of years of education from the improvements in the quality of education brought by these other changes in the educational system.[2] Our study takes a different approach to examining the connection between education and family and child outcomes by focusing on a reform that only changed the minimum age to work. We exploit the interaction between the compulsory schooling age and the minimum legal working age to identify the incentives of different individuals. We argue that because both age thresholds affect the decision to remain in the educational system, it is important to consider both factors together. Finally, we argue that increasing the minimum legal working age could be a more efficient and cost-free way of increasing educational attainment than raising the number of years of compulsory schooling, providing a potential policy alternative for a number of developing countries.[3]

We use a differences-in-differences strategy to identify the reform's within-cohort effects. In our setup, treated individuals (born between January and May) and their control counterparts (born between July and December)[4] only differ in their month of birth. Consequently, our identification strategy improves upon the before-after analysis commonly used in the literature (which would identify the reform's between-cohort effects). Any concurrent social or political event that might have affected the post-reform cohorts would have a similar impact on both our treatment and control groups. Unlike most of the extant literature, we use registered data of all births and marriages

[2]For instance, Brunello and Paola (2014) examining several different educational policies concludes that the effect over early school leavers is very different.

[3]The reform in question was implemented when Spain was still a developing country. Thus, our results are particularly relevant from a policy perspective to countries whose level of education and child labor market participation are similar to the levels Spain was experiencing during the early 1980s (reported in Figure 1).

[4]Note that we deliberately exclude the month of June, as it coincides with the end of the academic year.

in Spain, which allows us to observe the universe of all birth and marriages that took place during more than 30 years. [5] This type of data has some advantages over census data, which only identify a woman's children 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 all affect this number. If the level of education affects the probability that some of these situations occur, then census data could bias the results.

We find that the reform (through increasing education), significantly increased the women's probability of remaining childless and reduced their completed fertility, with 1,786 fewer women having children and 3,307 fewer children being born in the 10 cohorts following the reform. These negative effects operate through a postponement of first births until an age when the catching-up effect cannot take place due to decreased fertility with age. We also show that the marriage market is another channel through which education affects fertility, delaying the age at which women first marry and reducing the likelihood that a woman ever marries.

Moreover, we show that the postponement in fertility is also detrimental for the health of their offspring at the moment of delivery. The reform caused 2,789 more children to be born at less than 37 weeks of gestation and 4,352 were born with low birth weight. We propose three different channels through which education could be negatively impacting infant health. The first is the postponement of the age at which women have their first child, which increases the probability of having this first child after the age of 35. Because the risk to infant health during pregnancy increases after that age, postponing motherhood translates into negative effects on infant health. We also show that the reform changed the maternal marital status, increasing the number of children whose mother were not married or that had no registered father. In addition, we show that better employment perspective of more educated women enhances unhealthier behaviors (smoking and drinking), further contributing to the negative effects that we report on infant health.

This last result may seem surprising given prior research showing a negative relation between years of schooling and smoking prevalence among women in developed countries (for instance, Currie and Moretti (2003)). This finding can be explained by differences in labor market integration and educational attainment between men and women in the pre-reform cohorts; Bilal et al. (2015) find a substantial difference in smoking prevalence by gender in cohorts born in Spain between 1940 and 1960 (pre-reform cohorts), with highly educated women having the highest smoking prevalence rates and women with fewer years of education exhibiting lower rates of smoking. This inverse gradient for Spanish women is gradually reversed until the cohorts of women born after 1980, when the country's gradient begins to mirror that of developed countries, with less educated women showing the highest smoking prevalence rates.

The remainder of the paper is organized as follows. Section 1 presents the institutional context as well as the identification strategy. In Section 2, we analyze the effect of the reform on educational attainment, and in Section 3 we present the effects of the reform on fertility rates and infant health. In Section 4, we do a broader before-after analysis of the reform. Finally, Section 5 concludes with a discussion of the main results and their policy implica-

[5] See the data appendix for a description of all datasets and registers used in this paper.

tions. At the end of the paper, a data appendix can be found with a detailed explanation of all the databases used in the paper.

# 1 Institutional Context

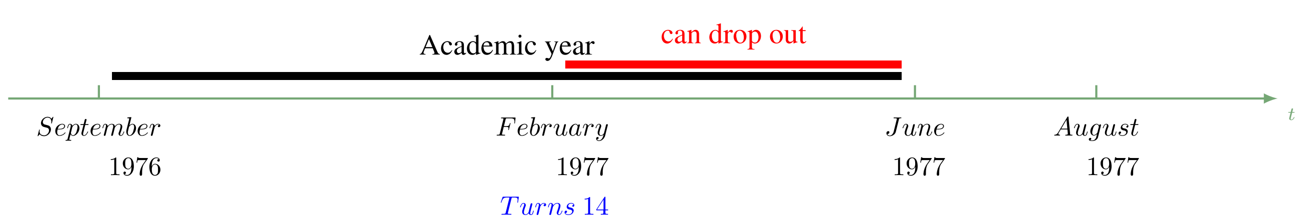
Our identification strategy builds on an exogenous variation in the incentive to stay in school induced by a legislative change in the legal age to work in Spain. Law 8/1980 "Estatuto de los Trabajadores" (ET) was introduced in March of 1980 and increased the minimum legal working age from 14 to 16 years old. Only individuals born after 1966, who were 14 at the time the reform passed, were subject to the reform. Therefore, we compare individuals who turned 14 just after the reform to those who turned 14 just before the reform.

Additionally, not all individuals from the same cohort were affected by the reform in the same way. Before the reform, students born during the first months of the year reached the minimum legal working age of 14 before finishing their last year of primary education.[6] Therefore, they had an incentive to leave school before completing primary education. On the other hand, students born during the last months of the year had reasons to finish primary education, as they were not old enough (had not turned 14 years old) to legally work before finishing in time. Consequently, we expect that before the reform was passed, those born at the beginning of the year have a lower probability of finishing primary schooling than individuals born at the end of the year.

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 age 16.

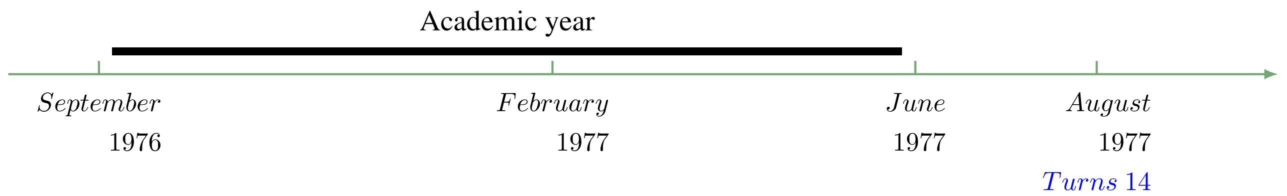
The following chart illustrates the timing of the reform by showing two individuals in the same 1963 cohort (pre-reform), during their last year of primary schooling:

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




[6]Note that in the Spanish educational system, all children from the same cohort start school 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.

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




Before the reform, the two individuals' incentives to stay in the educational system during the last year of primary education differed depending on whether they were born during the first part of the year (from January to May) or the last part of the year (from July to December).

We exploit the exogenous change in the incentives introduced by the ET reform to identify the causal effect of education on fertility and infant health outcomes. We focus on the variation among individuals from the same cohort but born at different times of the year, before and after the reform. Thus, we are not making a before-after comparison as the prior literature has done when analyzing the effects of changes in the educational laws. We are aware that the impact of the ET could potentially be greater than what we estimate using the within-cohort comparison and, at the end of the paper, we provide estimates of this before-after effect. However, in 1980, the year that the reform was introduced, Spain was experiencing a period of significant social change. The democratization process in Spain took place in 1979, and a number of reforms passed quickly thereafter. For instance, divorce was legalized in 1981 and abortion in 1985. Consequently, the cohorts of women that turned 14 years old before and after the reform are exposed to different environments. Even if we observe a significant change in educational attainment and other outcomes after the 1980 reform, this change could be due to the influence of other reforms that were taking place concurrently. Hence, our strategy is much more conservative as we are only exploiting the within-cohort variation but, in this setting, the identification strategy is much more reliable than a before-after modeling approach.

# 2 Effect of the reform on educational attainment

Following Jiménez-Martín et al. (2015), we examine the effect of the reform on two educational outcomes: the probability of being an early school leaver (ESL) and the probability of dropping out before or at 16 years old. We classify as ESL all women who are illiterate, have not completed primary school[7], or have been enrolled in labour market integration programs that do not require a primary school degree. We also examine the probability of dropping before or at 16 years old, that will be the age at which individuals should have finished the first part of secondary education, that was already not compulsory.

[7] Primary school at that time in Spain, correspond to the first eight years of education. School starting age was 5/6 years old, so students, at that time, used to finish primary school at 13/14 years old.

We use the Spanish Labour Force Survey (LFS) for the years 2000–2013 (see the data appendix for a description of all datasets and registers used in this paper). We cannot use previous waves of the LFS because the information on the month of birth is not reported before the year 2000.[8] We also drop from the sample all individuals not born in Spain, as we do not know if they were in the country during their primary and secondary schooling years.

Our econometric strategy exploits the fact that before the ET reform of 1980, individuals from the same cohort had different incentives to obtain their primary education degree. We posit that the introduction of the reform eliminated this difference in incentives and decreased the probability that individuals born at the begin of the year would leave school early. We use a difference-in-difference approach to identify the effects of the policy by comparing individuals born during the first five months of the year to those born during the last six months of the year, for cohorts of women who turn 14 years old before and after the reform.

Formally, we consider the following econometric model:

$$\begin{aligned} Outcome_i = \alpha + \beta_1 Treat_i + \beta_2 Postreform_i + \beta_3 Treat_i * Postreform_i \\ + \beta_4 BirthYearFE + \beta_5 CalendarYearFE + \beta_6 RegionFE + \epsilon_i \end{aligned}$$

where $Treat_i$ is a dummy variable that equals one if individual i was born between January and May and zero if she was born between July and December. $Postreform_i$ is also a dummy variable that takes a value of one if individual i turned 14 after the reform and zero otherwise. Then, we define pre-reform cohorts as those born in 1958 to 1965, and post-reform cohorts as those born between 1967 and 1974. We also include region, birthyear and calendar year fixed effects and cluster the standard errors at the region and cohort level. The effect of the reform can be identified by the coefficient of the interaction between the post-reform and the treatment dummy variables, $\beta_3$. We will use this same econometric specification in the rest of the paper.

The estimates reported in Table 1 show the effect of the reform on the probability of being an early school leaver or dropping before or at 16 years old for all women (columns 1 and 2, respectively) and men (column 3 and 4, respectively). Figure 2 reports the raw data and the predictions from the estimation model for women and men in the treatment and control groups and for all cohorts during the 1958–1974 period. Before the reform, a woman and a man born at the beginning of the year had a significantly higher probability of being an ESL or leaving school before or at 16 years old. However, this difference almost disappears after the reform is implemented.[9] More

[8]We are only interested in completion rates of primary and secondary education, and our youngest cohort (1974) was 26 in 2000. Therefore, we would not be able to use the information from previous years even if available because we need to ensure that individuals are not in primary and/or secondary education at the time of the interview.

[9]As it can be observe in the graph, 15.09% of treated women of 1965 cohort were early school leavers, compared with the 13.27% of treated women of the cohort of 1967, this is a decrease of almost 2 percentage points, that is stronger than the decrease of 0.45 for non-treated woman of these cohorts. Also, 45.80% of treated women born in 1965 have left school before or at 16 years old, while this percentage was 43.4 for treated women born in 1967. Thus, there is a decrease of 2.6 percentage points in the probability of being dropouts for treated women

precisely, Table 1 shows that the likelihood of being an ESL drops by 1 percentage points after the policy reform, implying that 1,361 fewer women of the cohort born in 1967 left school early.[10] As the number of ESLs in the last pre-reform cohort, 1965, was 20,502 for treated women, the effect of the reform implies an 6.6 percent decrease in ESLs.

The reform also decreased the probability of leaving school before or at 16 years old by 0.91 percentage point, even if secondary education was not compulsory. From the cohort born in 1967 alone, 1,243 fewer women left school before 16.[11] As the number of woman that left school before 16 in 1965 was 62,601 among treated women, the reform decreased dropouts by 1.9 percent. We also observe from Table 1 and Figure 2 that these effects were stronger for men than for women. This is likely due to the fact that before the reform, less educated men had more opportunities to work at age 14 than women. Finally, these results are robust to different specifications.[12]

# 3 Effect of the reform on family behavior outcomes

As the reform increased the educational attainment of individuals born during the first months of the year, we use the reform to identify the effect of education on family behavioral outcomes. In this section, we analyze the effects of the reform on fertility outcomes and infant health at delivery.

## 3.1 Effect of the reform on fertility

We first study the impact of the reform on several fertility outcomes. To test whether women with higher education postpone motherhood, as suggested by the literature, we first examine the impact of the reform on the age when women have their first child. Secondly, we assess whether the increase in educational achievement affected the probability of women remaining childless or the number of children they have. In other words, we want to determine whether there is a catching-up effect after the delay of motherhood.

For these estimations, we use register data on all birth certificates from 1975 to 2012, available from the Spanish National Statistics Institute (see the data appendix for a detailed explanation of the birth register ). Our pre-reform cohorts comprise women born between 1961 and 1965, and the post-reform cohorts include those born between 1967 and 1971. In addition, we restrict the sample to all births from women born in Spain and those births that took place when the mother was between the ages of 14 and 41. 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

after the reform. However, for non-treated women the decrease in the probability of dropping before 16 is only of 0.6 percentage points.

[10] This number increases to 13,512 if we take into account the cohorts from 1967 to 1976.

[11] 12,296 fewer women if we consider the cohorts from 1967 to 1976.

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

women of the last cohort (1971) were 41 in the last year of the register.

We define the probability of ending childless as the ratio between the total number of first births and the total number of women born in a certain cohort and treatment status.[13] Similarly, we define completed fertility as the ratio between the total number of births and the total number of women born in a certain cohort and treatment status. To examine the effect of the education reform on the age when women had their first child, the probability of ending childless and the completed fertility we apply the same econometric model as the one used for educational attainment. However, given the nature of the dependent variable the data has been collapsed at the cohort level, with cells defined at the level of treatment, cohort, year of birth and region.

The estimates in Table 2 show the effect of the reform on the age of women when they had their first child. Before the reform, women born at the beginning of the year had their first child a bit more than half a month earlier than women born at the end of the year. This gap in age disappears after the reform is introduced.[14]

Table 2 also shows that the postponement effect is followed by an increase in the probability of remaining childless as well as a decrease in completed fertility. [15] Thus, 222 more women born in 1967[16] decided to remain childless, a decrease of 0.19 percent in the number of women that have children after the reform was implemented.[17]

Moreover, before the reform, 3.17 more children were born per 1,000 women born at the beginning of the year with respect to women born at the end of the year. This gap is eliminated after the introduction of the reform. This means that 408 fewer children were born to the 1967 cohort of women,[18] corresponding to a 0.19 percent decrease in the total number of children born, given that women born at the beginning of 1965 had in total 209,954 children.

As a robustness check, we examine the probability of remaining childless and the completed fertility rate using data from the 2011 census, which includes a representative sample of 5 percent of the population and provides information about the number of children that women had up until 2011 (see data appendix for more information on the census of 2011).[19] Table 3 shows that the reform's effect on the probability of having at least one child and the completed fertility rate goes in the same direction as the results found using birth registries. However, the results are not significant. We believe two main factors can explain the lack of significance in the coefficients estimated with the 2011 census. First, we are only observing 5 percent of the population, so the results could be estimated

[13]The results are multiplied by 1,000.

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

[15]Be aware that we are only considering births that took place between the ages of 14 and 41. Thus, we cannot completely rule out the catching-up effect, as this effect could be taking place after the age of 41.

[16]or 2,205 born between 1967 and 1976

[17]Note that 115938 women born at the beginning of the year 1965 decided to have children.

[18]or 4.053 children in the subsequent 10 generations affected by the reform.

[19]Note that we are considering the same cohorts of women (1961–1971) and are defining treatment in the same way. Furthermore, in 2011, the last cohort we are considering(1971) had reached the age of 41. This is the same age constraint we had when using birth registries.

with more noise, and, thus, the standard errors are higher. Second, as the census does not include information on the year in which women had their children, we cannot control for calendar year effects.

On the other hand, the birth registries do not specify the number of children that each specific woman has. This information is only reported in the census. To examine the effect of the reform on the composition of families, we thus use census data on the total number of children per woman. The last regression of Table 3 shows that before the reform, women born at the beginning of the year had a higher probability of having a "large family," defined as having three or more children. This gap is significantly reduced after the introduction of the reform.

Therefore, we conclude that the reform had three main effects on fertility. First, it made some women postpone the entrance into motherhood, and this delay was not compensated for later in life.[20] Second, it increased the number of women that remained childless, and, third, it decreased the probability of having a large family.

### 3.1.1 Mechanisms

This section explores the potential channels that may be preventing the catching-up effect from taking place. The main hypothesis is that education delays entrance into motherhood for a large number of years, after which the catching-up effect can no longer take place. To check the validity of this hypothesis, we estimate some age-specific probabilities of having the first birth. More precisely, we use the same econometric model as before but with different outcomes: the probability of having the first birth before turning 18 years old, between 18 and 21 years old, between 25 and 30 years, and after age 35.[21]

Table 4 reveals no significant differences in the probability of having the first child during the teenage years between the treatment and control groups before and after the reform. In contrast to the findings of prior studies, this evidence indicates that the reform did not postpone the incidence of the first birth away from the teenage years. Thus, we can rule out the "incarceration effect"[22] as the main channel through which this reform affected fertility.

On the other hand, the reform did affect the probability of having the first child between the ages of 18 and 21 as well as after age 35. Before the reform, women born at the beginning of the year had a higher probability of having their first child between the ages of 18 and 21, and a lower probability of having their first child after the age of 35, compared with women born at the end of the year. This gap was reduced after the introduction of the reform.

[20] We also evaluate this result using the "Encuesta de Fecundidad" of 2006, available from Centro de Investigaciones Sociológicas. This questionnaire was given to 10,000 women who were over the age of 15 in 2006. Here, we also have information on the total number of children that women from the cohorts of 1964 to 1968 had in 2006. The number of observations, however, is very small (around 600 women). Thus, although results go in the same direction, they are not significant.

[21] For brevity, we do not report the estimates for the probability of having a first birth between 22 and 24 or between 31 and 34 years old as the results show that the reform did not have any significant impact on the probability of having the first birth in those age ranges.

[22] We define "incarceration effect" as a delay in fertility for the additional amount of time that women stay in school.

This effect of the reform can be clearly seen in Figure 3. Therefore, we conclude that the reform decreased the probability of pregnancy during the early twenties and increased the probability of having late first births.[23]

Even if the postponement of one month on average seems like a small effect, the increase in the incidence of first births after the age of 35 is not. The medical literature has shown that after age 35 a woman's fertility decreases. Thus, the catch-up may no longer be possible for some women, causing the observed decrease in completed fertility rates.

The reduction and postponement of fertility may be the result of a similar postponement and reduction of marriage. Note that in the 1980s in Spain, the majority of mothers had their children during their marriage. In fact, 87.11% of mothers were married when they had their first child. Thus, as an additional potential factor that may help explain the effects of the reform on fertility, we analyze whether the reform had any impact on marriage outcomes. First, we study whether increased education induced women to postpone the age when they marry for the first time. Next, we examine if this postponement reduces the number of first and total marriages over time.

For this analysis, we use register data on all marriage certificates from 1976 to 2012 (see the data appendix for a detailed explanation of the marriage register). As before, we consider the 1961–1965 cohorts to be pre-reform and the 1967–1971 to be post-reform. We restrict the sample to all marriages that took place when the woman was between the ages of 15 and 41. The definition of treatment and control is the same as before. Finally, we drop same-sex marriages due to their late acceptance in the definition of marriage.

We repeat the econometric model used to examine the effect over education. For the analysis of the impact of the reform on the number of total marriages, we collapse the data at the cohort and calendar-year level for treatment and control and divide them by the total number of women born to a certain cohort and treatment status. Similarly, to calculate the probability of never have married we divide the total number of first marriages by the total number of women born to a certain cohort and treatment status.

Table 5 shows the effects of the reform on marriage age. Before the reform, women born at the beginning of the year married, on average, almost half a month earlier than women born at the end of the year. This difference in age between women of the same cohort is almost entirely eliminated after the reform is introduced. [24]

Using data collapsed at the cohort level, with cells defined at the level of treatment, cohort, year of marriage and region, Table 5 also reveals that the postponement in marriage is accompanied by an increase in the probability of remaining single as well as a decrease in the total number of marriages per woman. After the reform, more than one in every 1,000 women born at the beginning of the year never married. Moreover, we observe a similar

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

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

reduction in the total number of marriages per woman.[25]

Summing up, we conclude that the reform postponed first marriages and consequently when women had their children. Moreover, we find that the postponement in fertility is not away from the teenage years (before age 18), as the majority of previous literature has found; instead, our results show that the reform decreased the probability of women having the first child between the ages of 18 and 21. The reform increased the incidence of first births after the age of 35, the age when women's fertility begins to drop, resulting in a reduction in completed fertility rates.[26]

## 3.2 Effect of the reform on infant health at delivery

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. We measure children's health at the moment of delivery. If we find evidence that the increase in maternal education has an impact on infant health, we can argue that education has intergenerational externalities that should be taken into account when calculating the returns to education.

In this analysis, we again use birth register data. We use four measures of newborn health: birth weight (in grams), the fraction of babies born weighing under 2,500 grams,[27] the fraction that are born after more than 37 gestational weeks,[28] and the fraction that die within the first 24 hours of life. Birth weight and survival of the first 24 hours data are only available from 1980 to 2012.[29] 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 41. [30]

Note that this analysis only examines the infant health of the woman's first child. We include this restriction because a poor health outcome from the first birth can influence the decision to have a second child, as pointed out by Wolpin (1993).

[25] However, these two results should be taken with caution, as we are only considering marriages that took place between the ages of 15 to 41. Thus, we cannot conclude that there is no catching-up effect, if this effect takes place after age 41.

[26] The fact that women's fertility rates decrease with age is well established, particularly for women over the age of 35. For instance, Leridon (2004) shows that the probability of conceiving after one year of trying decreases from 75 percent at age 30 to 66 percent at age 35.

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

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

[29] Note that we also have 11 percent of registered first births where the birth-weight is missing.

[30] Note that we already showed in Table 4 that the reform did not have an effect on the probability of women having the first child before the age of 18.

Table 6 reports the effects of the reform on the four infant health outcomes using the same econometric model as before. We find that the reform has a negative impact on the health of the children of women born at the beginning of the year.[31] After the reform, the first child of a woman born at the beginning of the year has a 0.213 percentage-point higher probability of being premature (born with less than 37 gestational weeks). This translates into 290 more children of women born in 1967 that are premature due to the reform.[32] Given that the number of premature infants per cohort of women is not very large,[33] 290 more premature children translates into an increase of 2.7 percent in the probability of having a premature child after the reform.

The reform also caused women born at the beginning of the year to have children that weighted 4.4 grams less, on average, compared to children of women born at the end of the year. Figure 4 shows the effect of the reform on children's weight.

While 4.4 grams may not seem like a lot, consider 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. Moreover, we estimate that after the reform, women born at the beginning of the year have a 0.38 percentage-point higher probability of having a first child with a low birth weight (less 2,500 grams). In absolute numbers, this implies that 453 more children are born with low weight from the cohort of women born in 1967.[34] As the percentage of children born weighing less than 2,500 grams is not very large,[35] 453 more children implies an increase of 6 percent in the number of low birth weight children due to the reform. These numbers constitute an important impact of the reform, as the long-run negative outcomes associated with low birth weight, such as labor market earnings and education, have been widely established in the literature (see Black et al. (2005), Figlio et al. (2014), Cook and Fletcher (2015) or Behrman and Rosenzweig (2004) , for instance).

Our results conflict with evidence presented in the extant literature, which mainly finds a positive impact of maternal education on child health. Thus, in the next subsection, we propose three possible channels through which the reform could have a negative impact on infant health.[36]

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

[32] 2,789 if we take into account the 10 consequent cohorts.

[33] Note that only 10,499 children were born premature from women born at the beginning of the year in 1965.

[34] 4,352 in the subsequent 10 generations of women.

[35] Only 7,474 children from the cohort of women born in 1965 were born weighing less than 2,500 grams.

[36] We are aware that the three channels we report in this paper might not be the only possible channels for the effect over infant health.

### 3.2.1 Explanatory Mechanisms

**The age and education effect**

A first channel through which the reform operates is the combination of the age and the education effects. On the one hand, we have shown that the reform postpones the entrance into motherhood. We label this the "age effect." This postponement increases the probability that women have their first child after the age of 35, which, in turn, has negative effects on infant health as risk during pregnancy increases after that age. On the other hand, we have also shown that the reform increased the educational attainment of women born during the first months of the year. We label this the "education effect." Following the previous literature, we hypothesize that increased maternal education improves infant health through an increase in household income and/or healthy behaviors during pregnancy. Thus, these two effects go in opposite directions. Therefore, the reform's negative effect on infant health could be driven by its bigger "age effect" than "education effect".

To analyze the importance of this first channel in explaining the reform's reported negative effects on infant health, we estimate a Heckman selection model for the child weight at birth.[37] We repeat the outcome equation we used to determine the impact of the reform on gestational maturity, weight, or survival during the first 24 hours. The selection equation is an age-specific regression for the probability of a mother having the first child at each age bracket. Under the assumption that the reform affected only the age at which women have their first child and there is no other other compositional effects, we can then interpret this model as the effect of the reform on children's birthweight, conditional on having the first child at a certain age. If the "age effect" is the main channel through which the reform is influencing infant health, we expect to find no effects or positive effects of the reform in the outcome equation for this Heckman selection model.

Along these lines, Table 7 shows that once we control for the age at which women have their first child, the reform has a significant positive effect. Thus, when we control for the age of the mother, the reform has a positive effect on infant health by increasing maternal educational attainment, which coincides with the findings reported in the rest of the literature.[38] Note that we should take these results with caution, as we can not rule out that the reform had any other compositional effects that are not captured by the age at which women had their first birth.

[37] We have also specified a Heckman selection model for other birth health outcomes. Results have the correct sign, although they are not significant.

[38] We also investigate whether there is selection in the children that are actually born. It could be the case that before the reform, those women with less education engaged in more unhealthy behaviors during pregnancy, which could lead to more fetal deaths. Then, the children that we observe from the women that were born at the beginning of the year would be those that come from the "better" mothers.
To check this alternative channel, we use register data on late fetal deaths, which reports all natural abortions that took place when the fetus has at least six months of gestation. We do not find any significant differences between treatment and control women before and after the reform on the probability of suffering a premature fetus death of more than six months of gestation. However, medical research indicates that the greatest risk of suffering a natural abortion is during the first three months of gestation. Therefore, we cannot completely rule out the selection hypothesis with these results. We can only conclude that there does not exist selection in those cases where the fetus survives until the sixth month of gestation.

**Changes in the maternal marital status**

We argued before that the reduction and postponement of fertility may be the result of a similar postponement and reduction of marriage. In this section, we focus on the change in the marital status of the subgroup of women that decide to become mothers. Previous literature (Gaudino et al. (1999), Bennett (1992) and Balayla et al. (2011) ) has established that children whose mothers are not married or have not father in the birth certificate 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 and the probability that the mother is not married. Therefore, a second possible mechanism through which the reform could be detrimental for infant health is the increase in the number of unmarried mothers and children without father.

**Changes in labor market behavior and health habits**

A third channel through which the reform could be affecting infant health is through changes in labor market behavior. If the reform increased not only the educational attainment of women but also the probability of working or the type of job, it would be more likely that these women worked or had higher level of stress during pregnancy, which could be affecting the health of their children at the time of delivery. Moreover, the income effect of working or having a better job could translate into a higher probability of these women engaging in more unhealthy behaviors, such as alcohol consumption or smoking, that could ultimately affect their children's health. In fact, 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).

To examine the potential labor market effects of the reform on affected women at ages 34-56 we use the Labor Force Survey (LFS), which provides labor market information from 2000 to 2007.[39] Table 9 shows that the reform seem has a positive but not significant impact on the probability of working. We also observe that after the reform, treated women have a higher probability of being high skill workers compared with women born at the end of the year. Thus, we conclude that after the reform, more educated women had a higher probability of being high skilled workers. This could imply that these women probably did not only have more income available but also faced higher level of stress.

Finally, we use data from the Spanish National Health Survey (see the data appendix for more information on this database) to determine whether the effect of having entered the labor market at a younger age affected the probability of engaging in healthier behaviors.[40] Table 10 shows that after the reform, women born at the beginning of

[39] We deliberately eliminate the crisis years of 2008 to 2012.

[40] Although this survey is available for several years, only the 2006 wave reports an individual's month of birth, which is a crucial variable for our identification strategy. Therefore, the results that we report are for the 2006 wave and include the 1961 to 1971 cohorts.

the year have a higher probability of smoking regularly and smoke more cigarettes per day.[41] Although the reform did not impact the probability that these woman are ex-smokers, after the reform, women born at the beginning of the year have a lower probability of quitting smoking during pregnancy. These outcomes could directly affect the health of their offspring.[42] This unexpected result for women is likely due to these cohorts growing up during the early post-Franco era. Women in these cohorts experienced the process of gender equalization, continuously getting more education and increasing their participation in the labor market.

Consequently, access to and social acceptance of smoking were much higher than for previous (pre-reform) cohorts. Although there is not much literature about this evolution, 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.

Thus, we find evidence that the reform has positive impacts on women, as they have a higher probability of being high skill workers and have a higher educational attainment. However, the impact of the reform on their children is negative. The increased skill level of the job also increases the women's probability of engaging in unhealthy behaviors that result in poorer health outcomes for their first child at the moment of delivery. We have also shown that part of these negative health outcomes on children can be attributed to the postponement of fertility of more educated women after the age of 35 and the increase in unmarried mothers and children without fathers.

## 3.3 Robustness checks

In this section, we perform several robustness checks of our key results. More specifically, we explore the sensitivity of our results to the exclusion of some pre-reform cohorts that could be considered noncompliers and perform some placebo tests in which we change the definition of the timing of the reform (fake reforms). Finally, we also examine the influence of father's education on fertility and infant health outcomes by the inclusion of additional variables that control for the characteristics of the father.

### 3.3.1 Exclusion of possible noncompliers

The labor market reform took place in 1980, affecting cohorts of individuals under age 14 that year, which translates into those born from the 1966 onwards. In our main analysis, we drop the 1966 cohort, which comprised individuals who turned 14 the year of the reform, as we could not predict the precise effect of the reform on this cohort. However, the cohorts of 1965 and 1964 were 15 and 16 years old the year the reform was implemented and were, therefore, likely to be partially affected by it. The 1980 law was unclear about the consequences for individuals who were already working at the age of 14 and 15 when the reform was introduced. Thus, we perform the

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

[42] However, Jiménez-Martín et al. (2015) find that the reform did not have any significant effect on health behavior for affected men.

same analysis but additionally drop the cohorts of 1964 and 1965, as they could potentially represent noncompliers.

The results in Table 11 and 12 indicate that the effects of the reform on fertility and infant health outcomes are unchanged when we exclude these two additional cohorts. Thus, we conclude that our results are robust to the exclusion of possible noncompliers.

### 3.3.2 Placebos

We also perform several placebo tests in which we use "fake" reform years. We only include those cohorts of women not affected by the "real" reform (the reform in 1980).We examine the effect of three "fake" reforms affecting the cohorts of 1961, 1962, and 1963.[43] 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 5 and 6 , we plot the estimates of the interaction term and the 95 percent confidence interval for the different fertility and infant health outcomes. Graph a) of Figure 6 shows that none of the "fake" reforms considered has a significant effect on the age at which women have their first child. However, for the probability of having the first child between ages 18 and 22, we see a change in the trend differences between the treatment and control groups for the 1963 and 1964 cohorts. Still, we find no effect of the reform for any of the cohorts for the probability of having a first child after age 35. Moreover, graphs d) and e) of Figure 5 again indicate no effect of any of the "fake" reforms on the probability of having a child or the total number of children that each woman has.

We perform the same analysis for all four infant health outcomes. We see from graphs a), b), and d) in Figure 6 that the "fake" reforms for the 1962, 1963, and 1964 cohorts do not affect the probability of having a first child after more than 37 weeks of gestation, the probability of the infant surviving the first 24 hours, or the probability of weighting more than 2,500 grams at birth. The results on birth weight are less clear, as the trend difference between the treatment and control groups seems to change for the 1964 cohort.

In sum, we believe that the placebo tests provide us with reasonable evidence to argue that there are no significant trend changes among the treatment and control groups for the cohort of women not affected by the reform for the majority of the fertility and infant health outcomes considered.

[43] We cannot replicate the placebo tests for the cohorts of 1964 and 1965 because, as explained above, they are potentially partially influenced by the reform. These two cohorts were 15 and 16 years old when the reform was introduced. Thus, if they were not working at that moment, the reform would have prevented them from starting working. Moreover, these cohorts could also still be in the last year of primary schooling if they had to retake a year at school.

### 3.3.3 Influence of father's education on fertility and infant health outcomes

It is reasonable to think that couples make fertility decisions jointly. If this is the case, many of our results related to fertility and infant health outcomes may not only be driven by the increase in education of the mother but also by the increase in education of the child's father. In an accompanying paper, Bellés-Obrero et al. (2015)), we study the effect of the reform on educational assortative mating and divorces. In this section, we examine whether the effect of maternal education on fertility and infant health outcomes hold up when controlling for paternal education. We proxy education of the father by the average age at which men have their first child, calculated by cohort, region, and treatment status. We also control for the probability that children have a registered father, similarly calculated by cohort, region, and treatment status.

Unfortunately, we do not have information on the education of the father in our dataset. We can indirectly check the relevance of this instrumental variable by analyzing the effect of the reform on the age when fathers had their first child. The first regression in Table 13 shows that the reform increased the age at which fathers had their first child by almost a month. Thus, as Jiménez-Martín et al. (2015) ) find that the reform increased male educational attainment, this regression indicates that more education also translates into increases in the age at which fathers had their first child, indicating that our instrument is relevant.

Table 13 also shows that when controlling for the fathers' characteristics, the effect of the reform on the fertility outcomes is considerably reduced. This confirms our hypothesis that not only the education of the mother but also the education of the father is affecting the age at which women have their first child. In comparison, the effect of the reform on infant health outcomes is quite robust to the inclusion of controls for paternal characteristics. In addition, the instruments for fathers' characteristics are not significantly associated with any of the infant health outcomes. This last result implies that mothers' education could be a more important determinant of infant health at the time of delivery, reinforcing our finding that part of the negative infant health outcomes are attributable to the effect of the reform on women's health behaviors such as smoking.

# 4 Pre-Post analysis of the reform

Our identification strategy relies on comparing individuals in the same cohort before and after the implementation of the policy. Because the reform took place during a time of social upheaval in Spain, we do not want to rely solely on before-and-after differences.[44] Thus, we employ a more conservative strategy, comparing women within the same cohorts. In this section, however, we provide some graphical evidence of the potential overall effect of the ET reform on some of the more important outcomes.

Figure 7 shows that the reform decreased the probability of having the first child before the age of 18 by 0.19 percentage points and the probability of having the first child between the age of 25 and 30 by 1.2 percentage

[44] For instance, divorce was legalized in 1981 and abortion in 1985.

points.[45] However, the probability of having a first child after age 35 increased by 0.13 percentage points. Thus, for the cohort of women born in 1966, 433 women delayed their first birth until the age of 35.[46]

This postponement of motherhood is accompanied by a decrease in the number of women that become a mother by 1.24 for every 1,000 women and by a decrease of 1.418 children for every 1,000 women. The first two graphs of Figure 8 illustrate these effects. Thus, we estimate that 412 fewer women born in 1966 had children (469 fewer children were born).[47]

Finally, we also find that the reform has a negative impact on infant health. The probability of having a premature child increases by 0.386 percentage points, and the probability of having a child with low birth weight (less than 2,500 grams) rises by 0.39 percentage points, as seen in the last two graphs of Figure 8. This means that around 1,090 children born from women of the 1966 cohort had low birth weight.[48]

# 5 Discussion

This study investigates the effect of education on fertility and infant health at the time of delivery. 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 six months of the year, before and after the reform.

We find that through an increase in educational attainment, the reform prompted a postponement of first births by a bit more than half month, on average. This number is very similar to the results in the majority of the previous literature. However, our results show that this postponement is not followed by a catching-up effect, as the reform increased a woman's probability of ending her fertile lifecycle without any children and reduced her completed fertility. We find that after the reform 2,205 women born between 1967 to 1976 do not become mothers. In turn, this resulted in 4,053 fewer children born from the 1967–1976 cohorts of women.

We provide evidence that the lack of catching-up effect and the reduction in completed fertility operate through a postponement of first births until an age when the catching-up is more difficult. In fact, we show that the reform

[45] In this estimation, we consider the cohorts of 1961–1965 to be the pre-reform cohorts, and those of 1966–1971 to be the post-reform cohorts. We do not drop the cohort of 1966 in this analysis, as these women turned 14 the year the reform was introduced, 1980. The econometric model includes linear and quadratic trends, and clusters the standard errors at the cohort level.

[46] increasing to 4,392 if we consider the cohorts of 1966–1976.

[47] This estimate increases to 4,735 fewer mothers and 5,380 fewer children born as a result of the reform for the subsequent 10 cohorts of women.

[48] 11,573 children born from women of the 1966–1976 cohorts were premature and had low birth weight.

decreased the probability of pregnancy during the early twenties while increasing the probability of having late first births (after the age of 35).

The marriage market is another factor that contributes to the postponement of first births. We find that the reform increased the age at which women marry for the first time by almost half a month. This postponement of marriage also leads to a decrease in the likelihood of getting married and the total number of marriages per woman.

Finally, we 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 by 0.213 percentage points. This result implies that women born between 1967 and 1976 had 2,789 more children born with less than 37 weeks of gestation. Moreover, these mothers also had a higher probability of having low birth weight babies after the reform.

We propose three different channels that could lead to this detrimental effect of mother's education on children's health. The first is the effect of age on pregnancy. When we control for the age at which women had their first child, the reform has a positive rather than a negative effect on infant health. This result is consistent with the positive impact of education on babies' health found in previous literature. Thus, we attribute our finding of negative impacts on health to more educated mothers having their first child at an older age, making their pregnancies more risky and increasing the chances of poor infant health outcomes. This age effect suppresses the positive effect of education on newborn health.

The second channel is the changes in the maternal marital status. We show that the reform increased the number of unmarried mothers and children without fathers and this could be detrimental for the health of their offspring at the moment of delivery.

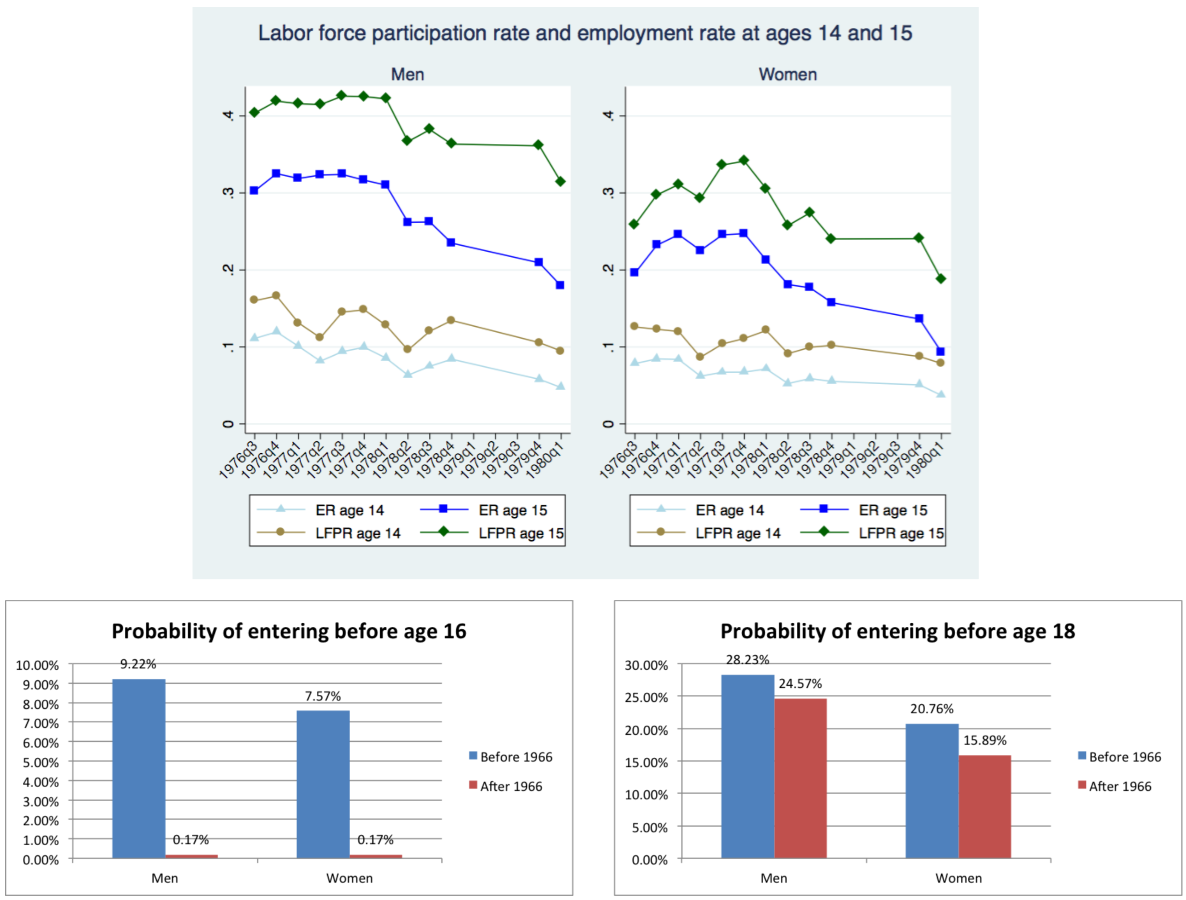
The third channel that we propose through which education has a negative effect on infant health is changes in labor market prospects and unhealthy habits of affected women. More precisely, we find that the reform increased the probability of treated women having a higher skill job. Simultaneously, their unhealthy habits increased as they increased their smoking prevalence. Thus, the fact that after the reform more educated women had better labor market outcomes has a negative impact on pregnancy through the increase in unhealthy behaviors. More precisely, we find that the probability of quitting smoking during pregnancy is reduced for women born at the beginning of the year after the reform.

Therefore, we conclude that even though the reform had positive impacts on women by increasing their educational attainment and the probability of being high skilled workers, the reform had negative consequences for their children. This effect is driven both by the increase in women's age at delivery, the increase of unmarried mothers and children without fathers, and by the increase in their unhealthy habits. Thus, as women's education has an impact on the health of their offspring, we can argue that education has intergenerational externalities that should be taken into account when calculating the returns to (increased) education. These results are driven by the fact that the reform we are analyzing took place during the 1980s in Spain. At that time, Spain was still a developing country, and a high percentage of its population had low levels of education and entered the labor market at an early age. Furthermore, as we show in this paper, the level of labor market integration and educational attainment among pre-reform women cohorts was very different from that of men. Thus, the results we find in this paper are more relevant, from a policy perspective, to developing countries whose educational system, child labor market participation rates, and social development are similar to the levels that Spain was experiencing around 1980.

# 6 Tables and Figures

## 6.1 Labor Market Attachment

Figure 1: Labor force attachment and age of entry distribution of affected cohorts




Note: "Before 1966" refers to the cohorts born in 1961-1965. "After 1966" refers to the cohorts born in 1967-1971. Number of observations: Men: 123,050; Women: 108,483.

Source: Spanish Labor Force Survey and Muestra de condiciones de vida Laboral (MCVL).

## 6.2 Educational Outcomes

Table 1: Effect of the reform on the probability of being an early school leaver and the probability of leaving school before or at 16 years old

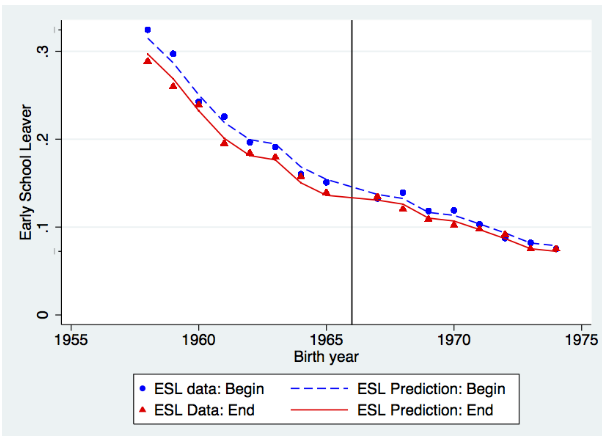
| | Women | | Men | |
|---|---|---|---|---|
| | Early School Leaver<br>(1) | Drop with $\leq$ 16 years old<br>(2) | Early School Leaver<br>(3) | Drop with $\leq$ 16 years old<br>(4) |
| Treated | 0.0174*** | 0.0145*** | 0.0167*** | 0.0144*** |
| | (0.00324) | (0.00372) | (0.00378) | (0.00449) |
| Post Reform | -0.213*** | -0.175*** | -0.142*** | -0.0535*** |
| | (0.00955) | (0.00900) | (0.00951) | (0.00819) |
| Treated*Post Reform | -0.0103** | -0.00918* | -0.0161*** | -0.0144** |
| | (0.00439) | (0.00551) | (0.00479) | (0.00618) |
| Constant | 0.430*** | 0.636*** | 0.397*** | 0.604*** |
| | (0.00996) | (0.00925) | (0.0102) | (0.00862) |
| Observations | 164,023 | 164,023 | 156,543 | 156,543 |
| R-squared | 0.065 | 0.045 | 0.048 | 0.028 |
| BirthYear FE | YES | YES | YES | YES |
| Region FE | YES | YES | YES | YES |
| CalendarYear FE | YES | YES | YES | YES |

Note: The dependent variables are the probability that (1) a woman is an early school leaver, (2) a woman leaves school with 16 or less years old, (3) a man is an early school leaver and, (4) a man leaves school with 16 or less years old. We include as early school leaver all women/men that are illiterate, have not completed the first eight year of education , or have been enrolled in labour market integration programs that do not require having finished the first eight years of education. Regressions include cohort, calendar time, and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. The results are robust in sign and significance to the substitution of the cohort time dummies by linear, quadratic and quartic pre- and post-reform trends. Robust standard errors clustered at cohort and region level in parentheses.

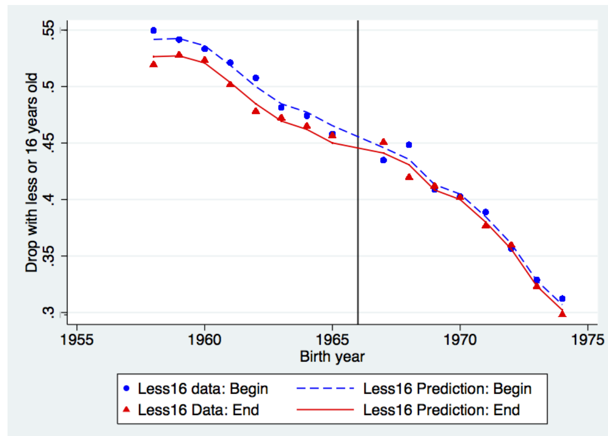
Source: Spanish Labour Force Survey, EPA, (2000- 2013), all individuals from cohorts 1958-1974

Significance: *** p<0.01, ** p<0.05, * p<0.1.

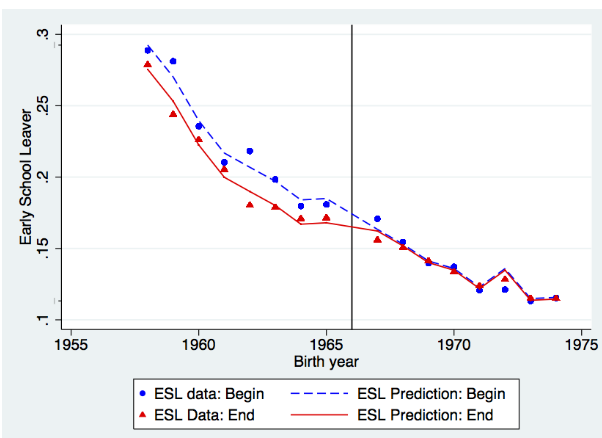
Figure 2: Probability of being an early school leaver and leaving school before or at 16 years old



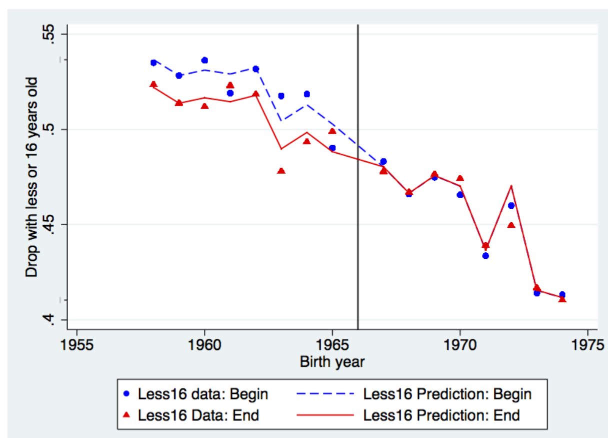

(a) Women: Early School Leaver




(b) Women: Dropping with less or 16 years old




(c) Men: Early School Leaver




(d) Men: Dropping with less or 16 years old

Note: The predictions are from a regression of the probability of (treated and nontreated) women and men of being (a, c) an early school leaver (b, d) leaving school before or at 16 years old . *Treated* are individuals born from January to May, and *control* are those born from July to December.

Source: Spanish Labour Force Survey, EPA, (2000- 2013), all individuals from cohorts 1958-1974

## 6.3 Fertility

Table 2: Effect of the reform on the age at which women had their first birth, the probability of remaining childless and the number of total children

| | Age first birth (1) | Perc. women in each cohort become a mother (2) | Number of children per women in each cohort (3) |
|---|---|---|---|
| Treated | -0.0821*** | 1.371*** | 3.179*** |
| | (0.0105) | (0.191) | (0.341) |
| Post Reform | 3.226*** | -4.841* | -17.95*** |
| | (0.0843) | (2.542) | (4.102) |
| Treated* Post Reform | 0.0550*** | -1.632*** | -3.022*** |
| | (0.0157) | (0.227) | (0.386) |
| Constant | 25.08*** | -5.178** | -6.107* |
| | (0.0937) | (2.423) | (3.297) |
| Observations | 2,469,113 | 9,338 | 9,352 |
| R-squared | 0.067 | 0.331 | 0.368 |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | NO | YES | YES |

Note: The dependent variables are (1) the age of the women they had their first child, (2) the percentage of (treated and control) women that had at least one child (multiplied by 1,000), and (3) the total number of children divided by the total number of women born in each cohort (multiplied by 1,000). Regressions include cohort time and region fixed effects and (2-3) calendar year dummies. Note that we cannot include calendar year dummies when the dependant variable is age as it takes out all the variation. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Effect of the reform on the probability of remaining childless, number of total children, and probability of having at least 3 children

| | Prob. of having a child<br>(1) | Total number of children<br>(2) | Prob. of having 3 or more children<br>(3) |
|---|---|---|---|
| Treated | 0.00281 | 0.0166*** | 0.00799*** |
| | (0.00220) | (0.00558) | (0.00177) |
| Post Reform | -0.0589*** | -0.223*** | -0.0542*** |
| | (0.00387) | (0.0194) | (0.00956) |
| Treated*Post Reform | -0.000295 | -0.00482 | -0.00374* |
| | (0.00329) | (0.00809) | (0.00223) |
| Constant | 0.864*** | 1.849*** | 0.197*** |
| | (0.00317) | (0.0194) | (0.00990) |
| Observations | 269,392 | 269,392 | 269,392 |
| R-squared | 0.009 | 0.025 | 0.016 |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | NO | NO | NO |

Note: The dependent variables are (1) the probability that a woman has at least one children, (2) total number of children per women and (3) the probability that a woman has at least 3 children. Regressions include cohort and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Census 2011, data of women born in cohorts 1961-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Effect of the reform on the probability of having the first birth at a certain age bracket

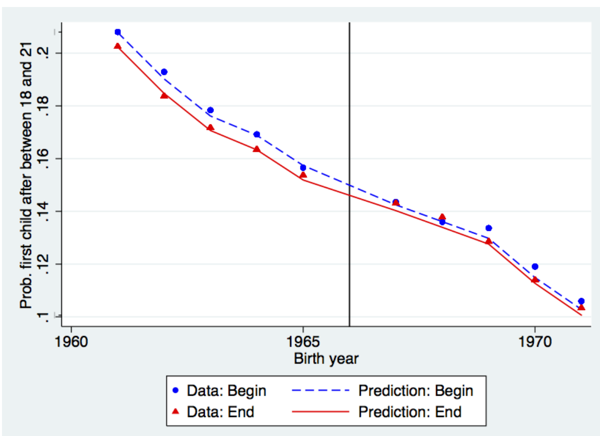
| | First births | | | |
|---|---|---|---|---|
| | Before 18 | Between 18 and 21 | Between 25 and 30 | More than 35 |
| | (1) | (2) | (3) | (4) |
| Treated | -0.000381 | 0.00543*** | -0.00477*** | -0.00204*** |
| | (0.000333) | (0.000727) | (0.000915) | (0.000468) |
| Post Reform | -0.0157*** | -0.101*** | -0.0428*** | 0.0979*** |
| | (0.00163) | (0.00393) | (0.00715) | (0.00453) |
| Treated* Post Reform | 2.07e-05 | -0.00279*** | -3.12e-05 | 0.00494*** |
| | (0.000516) | (0.00103) | (0.00123) | (0.000824) |
| Constant | 0.0553*** | 0.239*** | 0.320*** | 0.0346*** |
| | (0.00142) | (0.00297) | (0.00517) | (0.00332) |
| Observations | 2,469,113 | 2,469,113 | 2,469,113 | 2,469,113 |
| R-squared | 0.006 | 0.025 | 0.007 | 0.017 |
| BirthYear FE | YES | YES | YES | YES |
| Region FE | YES | YES | YES | YES |

Note: The dependent variables are the probability of having the first child (1) before age 18, (2) between age 18 and 21, (3) between age 25 and 30, and (4) after age 35. Regressions include cohort and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

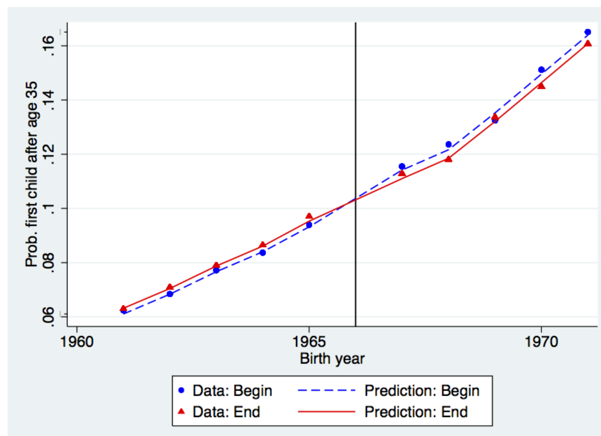
Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 3: Probability of having a first child at a certain age bracket




(a) Between the age of 18 and 21




(b) After the age of 35

Note: The predictions are from a regression of (a) a dummy variable that takes value 1 if the woman has her first child between the age of 18 and 21, and zero otherwise and (b) a dummy variable that takes value 1 if the woman has her first child after the age of 35 and zero otherwise. *Treated* are individuals born from January to May and *control* are those born from July to December.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

### 6.3.1 Marriage

Table 5: Effect of the reform on the age women marry for the first time, the probability of remaining single and the number of total marriages

| | Age first marriage (1) | Number of first marriages per woman in each cohort (2) | Number marriages per woman in each cohort (3) |
|---|---|---|---|
| Treated | -0.0563*** | 0.444** | 0.384* |
| | (0.0105) | (0.209) | (0.206) |
| Post Reform | 2.613*** | 4.161 | 8.336** |
| | (0.0619) | (3.119) | (3.204) |
| Treated*Post Reform | 0.0468*** | -1.293*** | -1.264*** |
| | (0.0153) | (0.270) | (0.268) |
| Constant | 23.70*** | 1.690 | 1.674 |
| | (0.0568) | (2.397) | (2.398) |
| Observations | 2,322,360 | 9,106 | 9,118 |
| R-squared | 0.051 | 0.372 | 0.387 |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | NO | YES | YES |

Note: The dependent variables are (1) the age of the women they married for the first time, (2) the percentage of (treated and control) women that married at least one time (multiplied by 1,000) and, (3) the total number of marriages divided by the total number of women born in each cohort (multiplied by 1,000). Regressions include cohort time and region fixed effects and (2-3) calendar year dummies. Note that we cannot include calendar year dummies when the dependant variable is age as it takes out all the variation. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Marriage registries (1976-2012), all women from cohorts 1961-1971.

Significance: *** p<0.01, ** p<0.05, * p<0.1.

## 6.4 Infant Health

Table 6: Effect of the reform on infant health outcomes

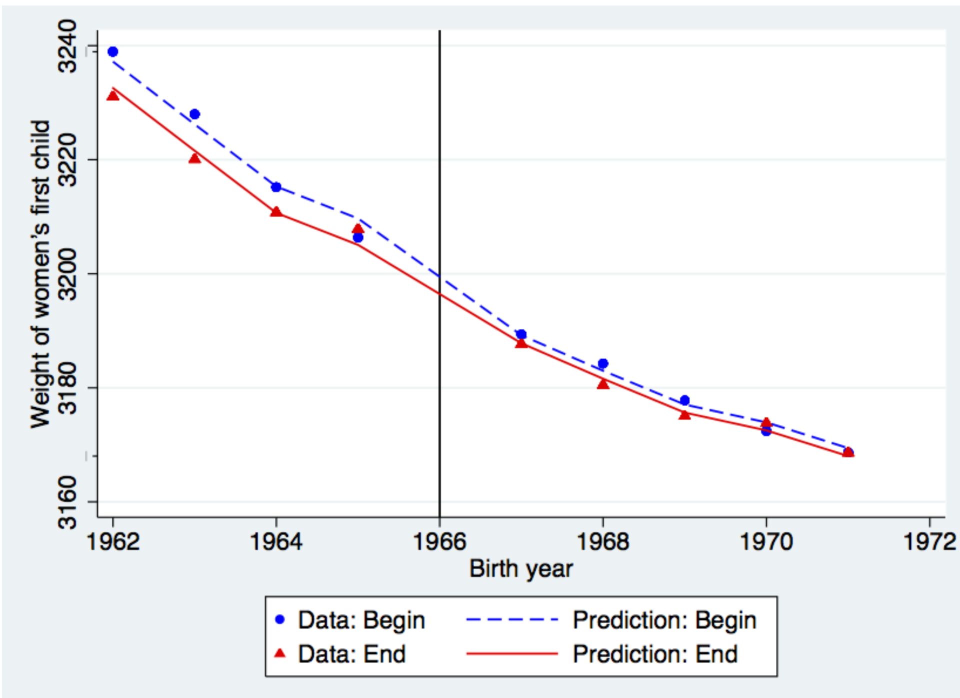
| | Infant health | | | |
|---|---|---|---|---|
| | Maturity<br>(1) | Survival 24h<br>(2) | Weight<br>(3) | Weight less 2,500<br>(4) |
| Treated | 0.000653 | 0.000129 | 2.016 | -0.000406 |
| | (0.000630) | (0.000123) | (1.351) | (0.000611) |
| Post Reform | 0.00405 | 0.000218 | -10.24** | -0.00383** |
| | (0.00684) | (0.000172) | (4.628) | (0.00159) |
| Treated* Post Reform | -0.00213*** | -0.000202 | -4.453** | 0.00173** |
| | (0.000771) | (0.000138) | (1.751) | (0.000869) |
| Constant | 0.922*** | 0.997*** | 3,304*** | 0.0505*** |
| | (0.0234) | (0.000852) | (9.662) | (0.00519) |
| Observations | 2,469,113 | 2,150,649 | 1,916,854 | 1,916,854 |
| R-squared | 0.013 | 0.000 | 0.010 | 0.008 |
| BirthYear FE | YES | YES | YES | YES |
| Region FE | YES | YES | YES | YES |
| CalendarYear FE | YES | YES | YES | YES |

Note: The dependent variables are (1) the probability of having a first child 37 weeks of gestation (2), the probability of having a first child that survives the first 24 hours after delivery, (3) the weight at birth of the woman's first child and, (4) the probability that the first child is born with less than 2,500 grams. Regressions include cohort, calendar time and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971. For birth-weight, only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 4: Birth weight of the first child




Note: The predictions are from a regression of the weight at the moment of delivery of the woman's first child. *Treated* are individuals born from January to May, and *control* are those born from July to December.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

### 6.4.1 Channel 1

Table 7: Heckman selection model with birthweight

| **First stage**: | Between 18 and 21<br>(1) | Between 25 and 30<br>(2) | More 35<br>(3) |
|---|---|---|---|
| Treated | 0.0206** | -0.0101*** | -0.0146*** |
| | (0.00317) | (0.00258) | (0.00375) |
| Post Reform | 313.0*** | -58.14 | 380.5* |
| | (0.0170) | (0.0180) | (0.0111) |
| Treated*Post Reform | -0.00757 | -0.00428 | 0.0278*** |
| | (0.00489) | (0.00349) | (0.00491) |
| Constant | -0.696*** | -0.401*** | -1.621*** |
| | (0.0159) ¡ | (0.0127) | (0.00657) |
| **Second stage**: Birth Weight | | | |
| Treated | -15.93** | -3.273 | -25.96** |
| | (7.117) | (6.398) | (10.76) |
| Post Reform | 313.0*** | -58.14 | 380.5* |
| | (119.6) | (90.52) | (199.0) |
| Treated*Post Reform | 7.915 | -4.292 | 36.63* |
| | (4.963) | (3.905) | (19.39) |
| Lambda | -1,345*** | 505.9 | 1,353 |
| | (379.6) | (828.9) | (823.2) |
| Constant | 5,036*** | -1.621*** | 240.2 |
| | (487.2) | (886.9) | (1,669) |
| Observations | 256,577 | 625,773 | 233,546 |
| R-squared | 0.007 | 0.005 | 0.003 |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | YES | YES | YES |

Note: The outcome equation has as a dependent variable the weight of the woman's first child at the moment of delivery. The dependent variable of the selection equations (that are not reported) are the probability of having a first child (1) before age 21, (2) between age 25 and 30, and (3) after the age of 35. The outcome regressions include cohort, calendar time, and region dummies while the selection equations only include cohort and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1980-2012), all women from cohorts 1962-1971.

Significance: *** p<0.01, ** p<0.05, * p<0.1.

#### 6.4.2 Channel 2

Table 8: Effect of the reform on marital status of mothers

| | Has father | Mother married |
|---|---|---|
| | (1) | (2) |
| Treated | 0.000337** | -0.000681 |
| | (0.000169) | (0.000467) |
| Post Reform | 0.00407*** | 0.0345*** |
| | (0.00109) | (0.00697) |
| Treated* Post Reform | -0.000586*** | -0.00159** |
| | (0.000211) | (0.000695) |
| Constant | 0.813*** | 0.757*** |
| | (0.0304) | (0.0317) |
| Observations | 2,469,113 | 2,469,113 |
| R-squared | 0.029 | 0.143 |
| BirthYear FE | YES | YES |
| CalendarYear FE | YES | YES |
| Region FE | YES | YES |

Note: The dependent variables are (1) the probability that the child has a father , and (2) the probability that the mother is married. Regressions include cohort, calendar year, and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1980-2012), all women from cohorts 1962-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

#### 6.4.3 Channel 3

Table 9: Effect of the reform on some labor outcomes of women

| | Work<br>(1) | High skill job<br>(2) | Low skill job<br>(3) |
|---|---|---|---|
| Treated | -0.686 | 0.679 | 0.598 |
| | (0.485) | (0.604) | (0.528) |
| Post Reform | 7.530*** | -4.027*** | -3.914*** |
| | (0.840) | (1.440) | (1.277) |
| Treated*Post Reform | 0.743 | 1.402* | -0.880 |
| | (0.687) | (0.786) | (0.772) |
| Constant | 38.56*** | 29.17*** | 31.23*** |
| | (0.840) | (1.413) | (1.119) |
| Observations | 91,794 | 54,758 | 54,758 |
| R-squared | 0.036 | 0.004 | 0.014 |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | YES | YES | YES |

Note: The dependent variables are (1) the probability of working at the time of the survey in EPA, (2) the probability of being inactive at the time of the survey in EPA, (3) the probability of having a high skill job and, (4) the probability of having a low skill job. Regressions include cohort, calendar year, and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Spanish Labour Force Survey (EPA) (2000- 2007), all women from cohorts 1958-1974.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 10: Some behavioral and health outcomes of women

| | Smoke/day (1) | Smoke regular (2) | Ex-smoker (3) | Pregnancy as motive for being ex-smoker (4) |
|---|---|---|---|---|
| Treated | -0.0526** | -0.0626*** | 0.00659 | 0.152*** |
| | (0.0218) | (0.0220) | (0.0235) | (0.0367) |
| Post Reform | -0.0776* | -0.0653 | 0.00828 | 0.372*** |
| | (0.0394) | (0.0408) | (0.0342) | (0.0699) |
| Treated*Post Reform | 0.115*** | 0.111*** | -0.0341 | -0.191*** |
| | (0.0305) | (0.0308) | (0.0307) | (0.0596) |
| Constant | 0.409*** | 0.436*** | 0.168*** | 0.0149 |
| | (0.0422) | (0.0437) | (0.0333) | (0.0531) |
| Observations | 3,151 | 3,151 | 3,151 | 714 |
| R-squared | 0.018 | 0.019 | 0.014 | 0.068 |
| BirthYear FE | YES | YES | YES | NO |
| Region FE | YES | YES | YES | YES |

Note: The dependent variables are (1) the probability of smoking at least one cigarette a day, (2) probability of smoking regularly, (3) the probability of having quit smoking and (4) the probability of having quitted smoking during pregnancy, conditional on being an ex-smoker. The regression include cohort and region dummies. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Encuesta Nacional de Salud 2006, all women from cohorts 1961-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

## 6.5 Robustness checks

### 6.5.1 Exclusion of possible noncompliers

Table 11: Effect of the reform on fertility outcomes excluding possible noncompliers

| | Drop cohort 1966 | Drop cohorts 1966-65 | Drop cohorts 1966-65-64 |
|---|---|---|---|
| | (1) | (2) | (3) |
| | | Age when first child | |
| Treated | -0.0821*** | -0.0908*** | -0.0950*** |
| | (0.0105) | (0.0117) | (0.0141) |
| Post Reform | 3.226*** | 2.806*** | 3.220*** |
| | (0.0843) | (0.0820) | (0.0828) |
| Treated* Post Reform | 0.0550*** | 0.0638*** | 0.0679*** |
| | (0.0157) | (0.0166) | (0.0183) |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | NO | NO | NO |
| | | Perc. women in each cohort become a mother | |
| Treated | 1.371*** | 1.411*** | 1.433*** |
| | (0.191) | (0.219) | (0.266) |
| Post Reform | -4.841* | -7.657*** | -6.609** |
| | (2.542) | (2.515) | (2.745) |
| Treated* Post Reform | -1.632*** | -1.604*** | -1.538*** |
| | (0.227) | (0.254) | (0.298) |
| | | Number of children per women in each cohort | |
| Treated | 3.179*** | 3.319*** | 3.345*** |
| | (0.341) | (0.384) | (0.464) |
| Post Reform | -17.95*** | -16.10*** | -21.16*** |
| | (4.102) | (4.448) | (4.455) |
| Treated* Post Reform | -3.022*** | -3.054*** | -2.938*** |
| | (0.386) | (0.426) | (0.501) |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | YES | YES | YES |

Note: The dependent variables are (Panel 1) the age at which women had their first child, (Panel 2) the percentage of women in each cohort that had at least one children, and (Panel 3) the total number of children per each cohort. Regressions include cohort, region dummies and (Panel 2 and 3) calendar year. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: Effect of the reform on infant health outcomes excluding possible noncompliers

| | Drop cohort 1966 (1) | Drop cohorts 1966-65 (2) | Drop cohorts 1966-65-64 (3) |
|---|---|---|---|
| Infant health: maturity | | | |
| Treated | 0.000653 | 0.000860 | 0.000962 |
| | (0.000630) | (0.000725) | (0.000923) |
| Post Reform | 0.00405 | -1.35e-05 | 0.00227 |
| | (0.00684) | (0.00664) | (0.00675) |
| Treated*Post Reform | -0.00213*** | -0.00229*** | -0.00233** |
| | (0.000771) | (0.000859) | (0.00104) |
| Infant health: survival 24 h | | | |
| Treated | 0.000129 | 0.000207 | 0.000403** |
| | (0.000123) | (0.000147) | (0.000176) |
| Post Reform | 0.000218 | 0.000241 | 0.000305* |
| | (0.000172) | (0.000181) | (0.000178) |
| Treated*Post Reform | -0.000202 | -0.000279* | -0.000474** |
| | (0.000138) | (0.000160) | (0.000187) |
| Infant health: less 2500 grams at birth | | | |
| Treated | -0.000406 | -0.000997 | -0.00141* |
| | (0.000611) | (0.000703) | (0.000830) |
| Post Reform | -0.00383** | -0.00662*** | -0.00593*** |
| | (0.00159) | (0.00170) | (0.00173) |
| Treated*Post Reform | 0.00173** | 0.00227** | 0.00260** |
| | (0.000869) | (0.000931) | (0.00103) |
| BirthYear FE | YES | YES | YES |
| Region FE | YES | YES | YES |
| CalendarYear FE | YES | YES | YES |

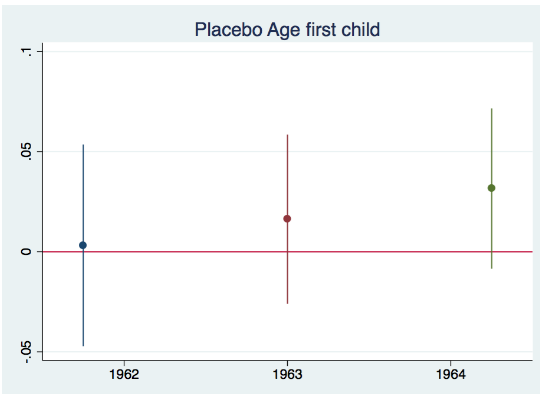
Note: The dependent variables are (Panel 1) the probability of having a first child with equal or more than 37 weeks of gestation, (Panel 2) the probability of having a first child that survives the first 24 hours after delivery, and (Panel 3) the probability that the first child is born with less than 2,500 grams. Regressions include cohort, calendar time, and region dummies. *Treated* are individuals born from January to May, and *contro*l are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971. For birth-weight, only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

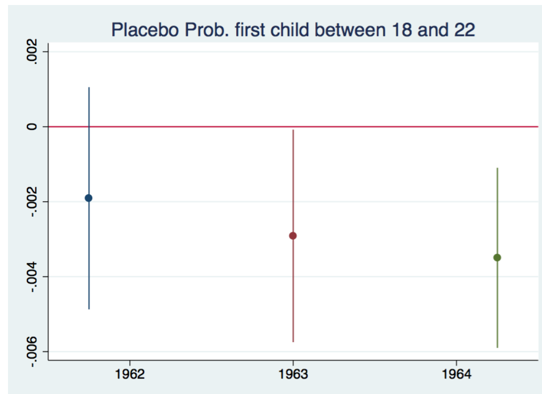
Significance: *** p<0.01, ** p<0.05, * p<0.1.

### 6.5.2 Placebos

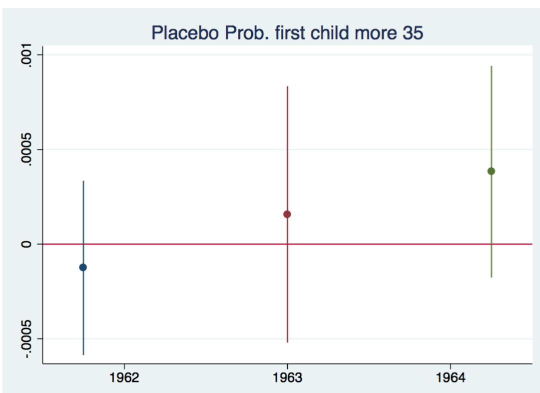
Figure 5: Placebos on Fertility



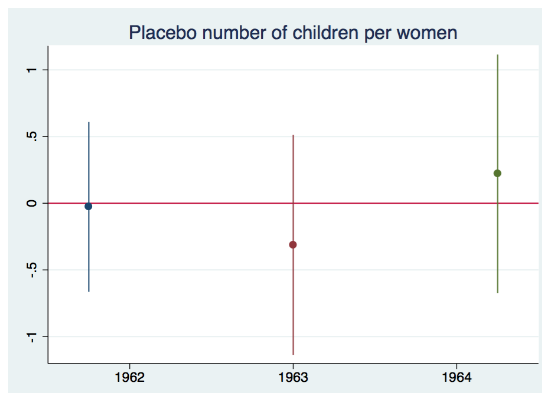

(a) Age at which women had their first birth



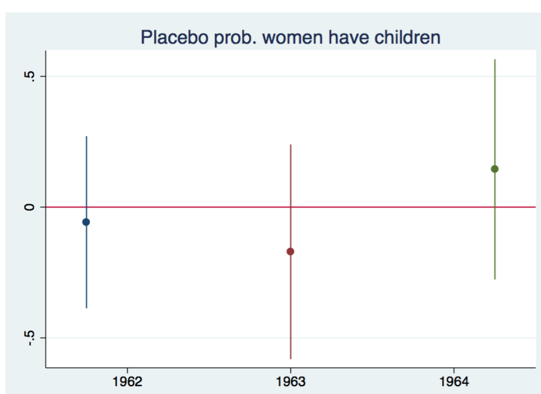

(b) Probability of first birth between the age of 18 and 21




(c) Probability of first birth with more than 35 years old




(d) Number of total children

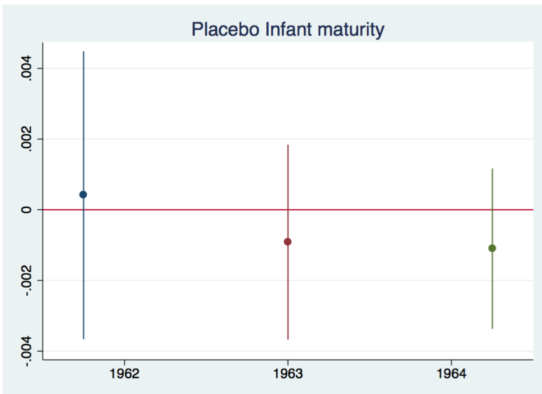



(e) Probability of having children

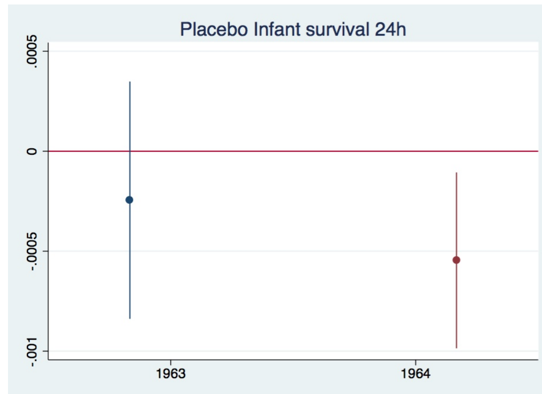
Note: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the ”fake” reform taking place for the cohorts of 1962, 1963, and 1964. We only consider cohorts not affected by the real reform: 1961-1965. The *treatment* is defined as those women born from January to June and *control* those women born from July to December.

Source: Birth registries (1975-2012), all women from cohorts 1961-1965 that had a child for the first time.

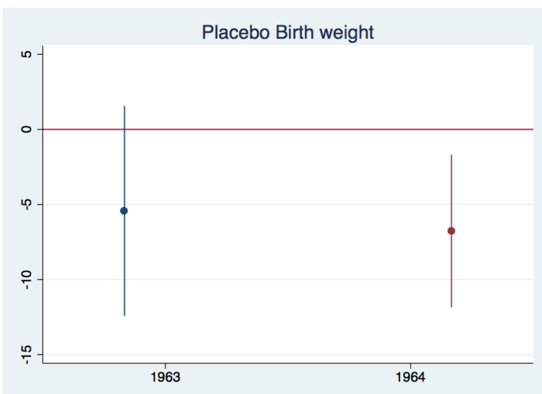
Figure 6: Placebos on Infant Health



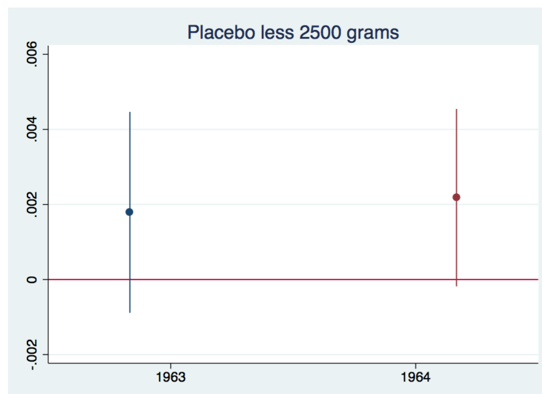

(a) Maturity of first child




(b) Survival first 24 hours




(c) Birth-weight of first child




(d) Probability of weighting less 2,500 grams

Note: 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 1962, 1963, and 1964. We only consider cohorts not affected by the real reform: 1961-1965. The *treatment* is defined as those women born from January to June and *control* those women born from July to December.

Source: Birth registries (1975-2012), all women from cohorts 1961-1965 that had a child for the first time.

Table 13: Effect of the reform on the age at which men had their first child and women's fertility and infant health outcomes controlling for the father

| | Age men first birhts | Age women first births | Infant Health | | | |
|---|---|---|---|---|---|---|
| | | | Maturity | Survival 24h | Weight | Weight less 2,500 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | -0.0811*** | 0.0643*** | -0.00192 | 0.000142 | 2.872* | -0.000514 |
| | (0.00923) | (0.00880) | (0.00146) | (0.000125) | (1.675) | (0.000821) |
| Post Reform | 2.404*** | 0.548*** | 0.0512** | -6.59e-05 | -0.135 | -0.0105 |
| | (0.0414) | (0.100) | (0.0250) | (0.000809) | (16.91) | (0.00714) |
| Treated* Post Reform | 0.0870*** | 0.0251** | -0.00160* | -0.000202 | -4.800*** | 0.00143 |
| | (0.0157) | (0.0106) | (0.000819) | (0.000137) | (1.785) | (0.000938) |
| Aver. age men | | 0.884*** | -0.0157** | 0.000143 | 1.936 | -0.00170 |
| | | (0.0356) | (0.00768) | (0.000299) | (5.931) | (0.00252) |
| Perc. fathers | | -14.79*** | 0.298 | -0.0126 | 416.8* | -0.0371 |
| | | (1.660) | (0.238) | (0.0147) | (234.0) | (0.0972) |
| Constant | 27.95*** | 15.65*** | 1.059*** | 1.005*** | 2,848*** | 0.146** |
| | (0.0358) | (1.029) | (0.150) | (0.00997) | (150.8) | (0.0592) |
| Observations | 2,191,245 | 2,469,113 | 2,469,113 | 2,150,649 | 1,916,854 | 1,916,854 |
| R-squared | 0.059 | 0.068 | 0.013 | 0.000 | 0.010 | 0.006 |
| BirthYear FE | YES | YES | YES | YES | YES | YES |
| Region r FE | YES | YES | YES | YES | YES | YES |
| CalendarYear FE | NO | NO | YES | YES | YES | YES |

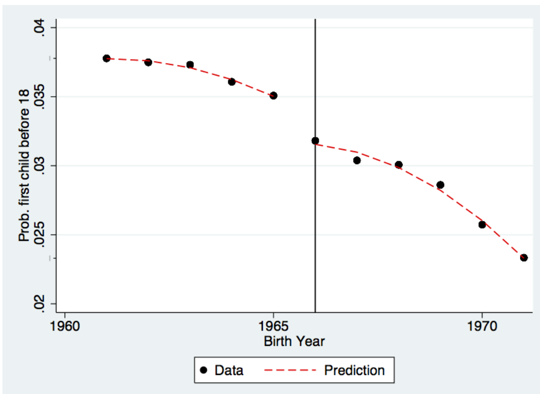
Note: The dependent variables are (1)the age of the men when they ahd their first child, (2) the age of the women when they had their first child, (3) the probability of having a first child with 37 weeks or more of gestation, (4) the probability of having a first child that survives the first 24 hours after delivery, (5) birth-weight at the time of delivery, and (6) the probability that the first child is born with less than 2,500 grams. Regressions (1-6) include cohort, (3-6) calendar time and (1-6) region dummies. We also control for the average age of the father of the first birth and for the probability that the first child has a father. These two variables are calculated by cohort-region and treatment. *Treated* are individuals born from January to May and *control* are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

Source: Birth registries (1975-2012), all individuals from cohorts 1961-1971. For birthweight, only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

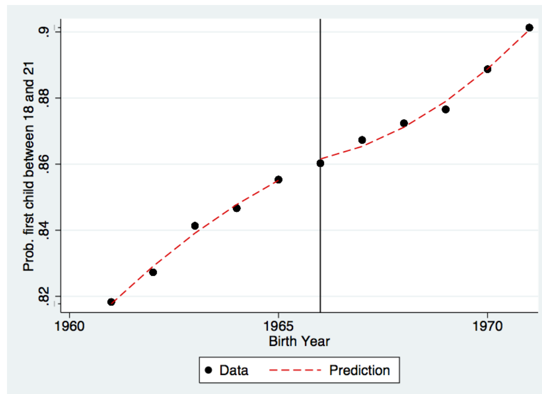
Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

### 6.5.3 Pre-Post analysis of the reform

Figure 7: Probability of having the first child at a certain age bracket

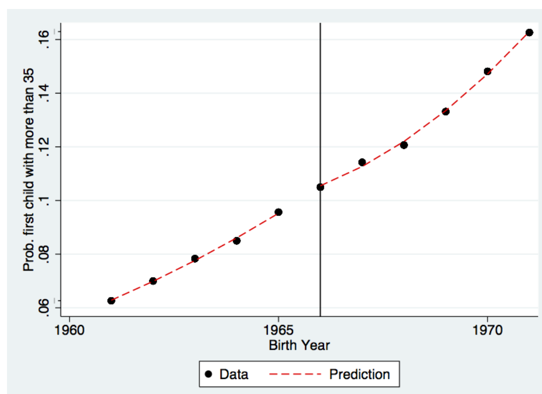



(a) Before the age of 18




(b) Between the age of 18 and 21


(c) Between the age of 25 and 30

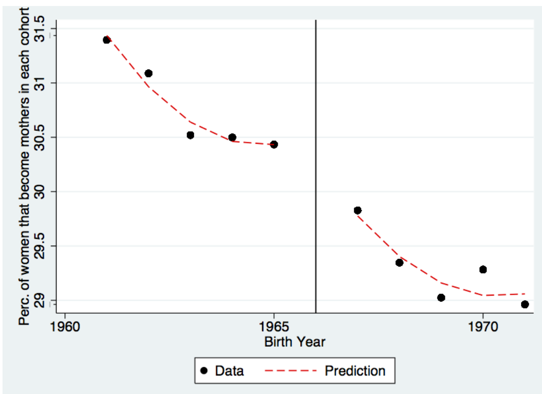



(d) After the age of 35

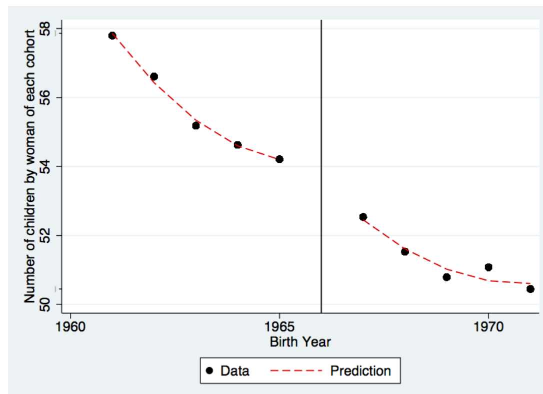
Note: The predictions are from a regression (with linear and quadratic trends) of the probability of women of having the first child (a) before the age of 18, (b) between the age of 18 and 21, (c) between the age of 25 and 30, and (d) after the age of 35. We consider the cohorts from 1961 to 1965 to be the cohorts before the reform and cohorts from 1966 to 1971 for after the reform.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971 that had a child for the first time.

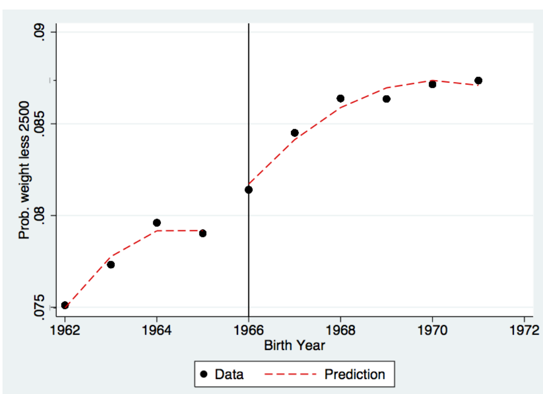
Figure 8: Impact of the reform on completed fertility and infant health



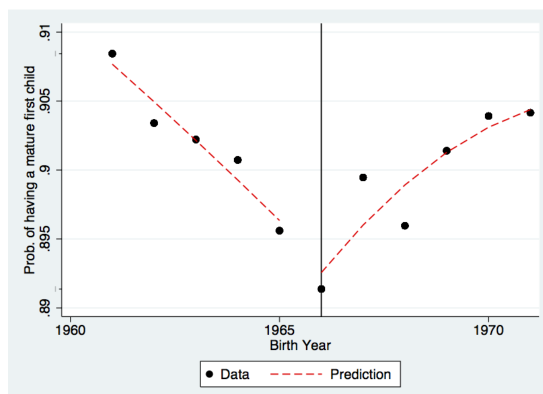

(a) Catching-up




(b) Completed fertility




(c) Low Birth Weight




(d) Maturity

Note: The predictions are from a regression (with linear and quadratic trends) of (a) the percentage of women in each cohort that have at least one child, (b) total number of children per women of each cohort, (c) probability of having a first child that weight less than 2,500 grams, and (d) probability of having a first child with less than 37 gestational weeks. We consider the cohorts from 1961 to 1965 to be the cohorts before the reform and cohorts from 1966 to 1971 for after the reform.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971 that had a child for the first time.

Almond, D., H. W. Hoynes, and D. W. Schanzenbach (2011). Inside the war on poverty: The impact of food stamps on birth outcomes. *The Review of Economics and Statistics 93*(2), 387–403.

Anderberg, D., T. Hener, and T. Wilson (2013). Assortative mating–the role of education in marital age gaps.

Anderberg, D. and Y. Zhu (2010). The effect of education on marital status and partner characteristics: evidence from the uk. Technical report, CESifo working paper Social Protection.

Balayla, J., L. Azoulay, and H. A. Abenhaim (2011). Maternal marital status and the risk of stillbirth and infant death: a population-based cohort study on 40 million births in the united states. *Women's Health Issues 21*(5), 361–365.

Behrman, J. R. and M. R. Rosenzweig (2002). Does increasing women's schooling raise the schooling of the next generation? *American Economic Review*, 323–334.

Behrman, J. R. and M. R. Rosenzweig (2004). Returns to birthweight. *Review of Economics and Statistics 86*(2), 586–601.

Bellés-Obrero, C., S. Jiménez-Martín, and J. Vall-Castelló (2015). The effects of increasing the legal working age on educational assortative mating. *mimeo*.

Bennett, T. (1992). Marital status and infant health outcomes. *Social science & medicine 35*(9), 1179–1187.

Bilal, U., P. Beltrán, E. Fernández, A. Navas-Acien, F. Bolumar, and M. Franco (2015). Gender equality and smoking: a theory-driven approach to smoking gender differences in spain. *Tobacco control*.

Black, S. E., P. J. Devereux, and K. Salvanes (2005). From the cradle to the labor market? the effect of birth weight on adult outcomes. Technical report, National Bureau of Economic Research.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2008). Staying in the classroom and out of the maternity ward? the effect of compulsory schooling laws on teenage births*. *The Economic Journal 118*(530), 1025–1054.

Breierova, L. and E. Duflo (2004). The impact of education on fertility and child mortality: Do fathers really matter less than mothers? *NBER Working Paper No. 10513.*.

Brunello, G. and M. D. Paola (2014). The costs of early school leaving in europe. *IZA Journal of Labor Policy 3*(1), 22.

Buckles, K. S. and D. M. Hungerman (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics 95*(3), 711–724.

Cook, C. J. and J. M. Fletcher (2015). Understanding heterogeneity in the effects of birth weight on adult cognition and wages. *Journal of Health Economics 41*, 107–116.

Currie, J. and E. Moretti (2003). Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *Quarterly Journal of Economics*, 1495–1532.

Cygan-Rehm, K. and M. Maeder (2013). The effect of education on fertility: Evidence from a compulsory schooling reform. *Labour Economics 25*, 35–48.

Duflo, E., P. Dupas, and M. Kremer (2011). Education, hiv and early fertility: Experimental evidence from kenya.

Fan, E., J.-T. Liu, and Y.-C. Chen (2014). Is the 'quarter of birth'endogenous? evidence from one million siblings in taiwan. Technical report, National Bureau of Economic Research.

Figlio, D., J. Guryan, K. Karbownik, and J. Roth (2014). The effects of poor neonatal health on children's cognitive development. *The American Economic Review 104*(12), 3921–3955.

Fort, M. (2007). Just a matter of time: Empirical evidence of the causal effect of education on fertility in italy.

Fort, M., N. Schneeweis, and R. Winter-Ebmer (2011). More schooling, more children: Compulsory schooling reforms and fertility in europe.

Gaudino, J. A., B. Jenkins, and R. W. Rochat (1999). No fathers' names: a risk factor for infant mortality in the state of georgia, usa. *Social science & medicine 48*(2), 253–265.

Geruso, M., D. Clark, and H. Royer (2014). The impact of education on family formation: Quasi-experimental evidence from the uk. Technical report, mimeo, University of California, Santa Barbara.

Hoynes, H., M. Page, and A. H. Stevens (2011). Can targeted transfers improve birth outcomes?: Evidence from the introduction of the wic program. *Journal of Public Economics 95*(7), 813–827.

Jiménez-Martín, S., E. Del Rey, and J. Vall (2015). The effect of changes in the statutory minimum working age on educational, labor and health outcome. *mimeo*.

Kırdar, M. G. (2009). The impact of schooling on the timing of marriage and fertility: Evidence from a change in compulsory schooling law. Technical report, Society for Economic Dynamics.

Lappegård, T. and M. Rønsen (2005). The multifaceted impact of education on entry into motherhood. *European Journal of Population/Revue européenne de Démographie 21*(1), 31–49.

Lavy, V. and A. Zablotsky (2011). Mother's schooling, fertility, and children's education: Evidence from a natural experiment. *NBER Working Paper No. 16856.*.

Lee, R., A. Mason, E. Amporfu, C.-B. An, L. R. Bixby, J. Bravo, M. Bucheli, Q. Chen, P. Comelatto, D. Coy, et al. (2014). Is low fertility really a problem? population aging, dependency, and consumption. *Science 346*(6206), 229–234.

Lefgren, L. and F. McIntyre (2006). The relationship between women's education and marriage outcomes. *Journal of Labor Economics 24*(4), 787–830.

León, A. (2006). The effect of education on fertility: Evidence from compulsory schooling laws. Technical report, University of Pittsburgh, Department of Economics.

Leridon, H. (2004). Can assisted reproduction technology compensate for the natural decline in fertility with age? a model assessment. *Human Reproduction 19*(7), 1548–1553.

McCrary, J. and H. Royer (2011). The effect of female education on fertility and infant health: Evidence from school entry policies using exact date of birth. *American Economic Review 101*, 158–195.

Monstad, K., C. Propper, and K. G. Salvanes (2008). Education and fertility: Evidence from a natural experiment. *The Scandinavian Journal of Economics 110*(4), 827–852.

Oreopoulos, P. and K. G. Salvanes (2011). Priceless: The nonpecuniary benefits of schooling. *The Journal of Economic Perspectives*, 159–184.

Osili, U. O. and B. T. Long (2008). Does female schooling reduce fertility? evidence from nigeria. *Journal of Development Economics 87*(1), 57–75.

Pampel, F. C. (2003). Age and education patterns of smoking among women in high-income nations. *Social Science & Medicine 57*(8), 1505–1514.

Schiaffino, A., E. Fernandez, C. Borrell, E. Salto, M. Garcia, and J. M. Borras (2003). Gender and educational differences in smoking initiation rates in spain from 1948 to 1992. *The European Journal of Public Health 13*(1), 56–60.

Silles, M. A. (2011). The effect of schooling on teenage childbearing: evidence using changes in compulsory education laws. *Journal of Population Economics 24*(2), 761–777.

Wolpin, K. I. (1993). Determinants and consequences of the mortality and health of infants and children. *Handbook of Population and Family Economics 1*, 483–557.

# 7 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.

## 7.1 Spanish Labor Force Survey

The Spanish Labor Force Survey is a continuous quarterly survey that contains information related to the labor market, active unemployment and inactivity of the population living in family dwelling in Spain. This database is available since 1964 however, in this paper, we use this database from 2000 to 2013 (for education outcomes) or from 2000 to 2007 (labor market outcomes), as the month of birth was not specified before. We will only consider in our sample women born between 1958 and 1974. We drop from our final sample all individuals not born in Spain and those individuals born in 1966 and therefore turned 14 the year the reform took place (1980). At the end we have information about 320,566 individuals from 2000 to 2013 and 180,573 from 2000 to 2007.

We use this data to assess the impact of the reform on education attainment (see Section 2) and labor market outcomes (see Section 3.2.1). For educational attainment, we use a variable that specifies the maximum level of education attained by all individuals with more than 16 year old at the moment of the interview, as well as, the self-reported age that they had when they acquired the maximum level of education. For the labor market outcomes we use a variable that ask for the employment situation the week before the interview of all individuals older than 16 at the moment of the interview, as well as, the type of ocupation they have.

Therefore, the main variables used are the following and their descriptive statistics can be found in Table A1 :

- **Early School Leaver**: A dummy that is equal to one if the individual is illiterate, have not completed the first eight years of education, or has been enrolled in labor market integration programs that do not require finishing the first eight year os education, or zero otherwise.
- **Drop with less or 16 year old**: A dummy that is equal to one if the individual has drop out of school before or at 16 years old. Note thateducation is only compulsory until 14 years old, or zero otherwise.
- **Work**: A dummy that is equal to one if the woman was working the week before to the interview, or zero otherwise
- **Inactivity**: A dummy that is equal to one if the woman was not participating in the labor market one week before the interview, or zero otherwise.
- **High skill job**: A dummy that is equal to one if the woman has a job that can be classified as business manager or administrator, civil servant, scientific and intellectual technician or professional, or as support technician or professional, and zero otherwise.
- **Low skill job**:A dummy that is equal to one if the woman has a job that can be classified as blue-collar in manufacturing factories, construction, minery, plant and machine operators and assemblers, or other unskilled jobs , and zero otherwise.

## 7.2 Birth Statistics

This database contains administrative data from birth certificates for the universe of children born in Spain between 1975 and 2012. 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.

The raw microdata contains 17,749,354 births. We, then, restrict the sample to births of Spanish women born between 1961 and 1971 that had 14 to 41 years old at the moment of delivery. 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 July. Thus, finally we observe a total of 4,453,087 births or 2,469,113 first births in our sample.

We use this database to assess the impact of the reform on fertility (see Section 3.1) and infant health outcomes (See Section 3.2). Here we define the main fertility and infant health variables used throughout the paper whose descriptive statistics could be found in Table A2:

- **Age**: Age of the women when they had their first child.
- **Percentage of women in each cohort that become a mother**: Total number of first births for a cohort of women divided by the total number of women of that same cohort multiplied by 1000.
- **Number of children per women in each cohort**: Total number of births for a cohort of women divided by the total number of women of that cohort multiplied by 1000.
- **First birth before 18**: A dummy variable that is equal to one if the woman has her first child before the age of 18, and zero otherwise.
- **First birth between 18 and 21**: A dummy variable that is equal to one if the woman has her first child between the age of 18 and 21, and zero otherwise.
- **First birth between 25 and 30**: A dummy variable that is equal to one if the woman has her first child between the age of 25 and 30, and zero otherwise.
- **First birth after 35**: A dummy variable that is equal to one if the woman has her first child after the age of 35, and zero otherwise.
- **Maturity**: A dummy variable that is equal to one if the woman has her first child with 37 or more weeks of gestations, and zero otherwise.
- **Survival 24h**: A dummy variable that is equal to one if the woman has her first child that survives the first 24 hours after delivery, and zero otherwise.
- **Weight**: Weight at birth of the woman's first birth.
- **Weight less 2,500**: A dummy variable that is equal to one if the woman's first child is born with less than 2,500 grams, and zero otherwise.
- **Weeks of gestation**: Number weeks of gestation of the first child of the woman.

## 7.3 Population and Housing Census of 2011

This database surveys a representative sample of 5 percent of the population living in Spain in 2011 and collects information about some the persons, households, buildings and dwellings.

The raw microdata contains information about 4,107,465 families. We, then, restrict the sample to f Spanish women born between 1961 and 1971. We also drop women born in 1966 and who therefore turned 14 the year the reform took place (1980) and those born in July. Thus, finally we observe a total of 269,392 women in our sample.

We use this database as a robustness check of the impact of the reform on some fertility outcomes (see Section 3.1 ) . Here we define the main fertility variables used throughout the paper whose descriptive statistics could be found in Table A3:

- **Probability of having a child**: Dummy variable that is equal to one if the woman had at least one children, and zero otherwise.
- **Total number of children**: Total number of children that each woman has.
- **Probability of having 3 or more children**: Dummy variable that is equal to one if the woman had at least 3 children, and zero otherwise.

## 7.4 Marriage Statistics

This database contains administrative data from marriage certificates for the universe of marriages held in Spain between 1976 and 2012. The information is self-reported as it comes from the Statistical Marriage Bulletins, that is filled out by the spouses at the time of registering this demographic event in the Civil Register.

The raw microdata contains 7,727,917 marriages. We, then, restrict the sample to marriages of Spanish women born between 1961 and 1971 that had 15 to 41 years old at the moment of the wedding. We also drop marriages of women born in 1966 and who therefore turned 14 the year the reform took place (1980) and those of women born in July. Thus, finally we observe a total of 2,389,673 marriages or 2,322,361 first marriages in our sample.

We use this database to assess the effect of the reform on some marriage outcomes (See Section 3.1.1). Here we define the main marriage variables used throughout the paper whose descriptive statistics could be found in Table A4:

- **Age**: Age of the women when they married for the first time.
- **Number of first marriages per women in each cohort**: Total number of first marriages for a cohort of women divided by the total number of women of that same cohort multiplied by 1000.
- **Number of marriages per women in each cohort**: Total number of marriages for a cohort of women divided by the total number of women of that cohort multiplied by 1000.

## 7.5 Spanish National Health Survey of 2006

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.

The raw microdata contains 29,478 individuals. We, then, restrict the sample to Spanish women born between 1961 and 1971. We also drop women born in 1966 and who therefore turned 14 the year the reform took place (1980) and those born in July. Thus, finally we observe a total of 3,151 women in our sample.

We use this database to assess the effect of the reform on some health behavior outcomes (See Section 3.2.1). Here we define the main marriage variables used throughout the paper whose descriptive statistics could be found in Table A5:

- **Smoke/day**: A dummy variable that is equal to one if the woman smokes at least one cigarette a day, and zero otherwise.
- **Smoke regular**: A dummy variable that is equal to one if the woman smokes at least one cigarette a day, and zero otherwise.
- **Ex-smoker**: A dummy variable that is equal to one if the woman is an ex-smoker, and zero otherwise.
- **Pregnancy as motive for being ex-smoker**: A dummy variable that is equal to one if the woman quitted smoking during pregnancy conditional on being and ex-smoker , and zero otherwise.

# 8 Tables of the Data Appendix

Table A1: Descriptive statistics of the Spanish Labour Force Survey

| | Treatment 1 | | | | | Treatment 0 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Min. | Max. | Observations | Mean | Std. Dev | Min. | Max. |
| Women: Early School Leaver | 75631 | 0.17 | 0.37 | 0.00 | 1.00 | 88392 | 0.16 | 0.36 | 0.00 | 1.00 |
| Women: Drop with less or 16 years old | 75631 | 0.45 | 0.50 | 0.00 | 1.00 | 88392 | 0.44 | 0.50 | 0.00 | 1.00 |
| Men: Early School Leaver | 72261 | 0.18 | 0.38 | 0.00 | 1.00 | 84282 | 0.17 | 0.38 | 0.00 | 1.00 |
| Men: Drop with less or 16 years old | 72261 | 0.49 | 0.50 | 0.00 | 1.00 | 84282 | 0.48 | 0.50 | 0.00 | 1.00 |
| Women: Work | 42390 | 59.37 | 49.12 | 0.00 | 100.00 | 49404 | 59.90 | 49.01 | 0.00 | 100.00 |
| Women: Inactive | 42390 | 30.77 | 46.16 | 0.00 | 100.00 | 49404 | 30.27 | 45.94 | 0.00 | 100.00 |
| Women: High skill job | 25165 | 26.48 | 44.12 | 0.00 | 100.00 | 29593 | 25.24 | 43.44 | 0.00 | 100.00 |
| Women: Low skill job | 25165 | 23.03 | 42.10 | 0.00 | 100.00 | 29593 | 22.75 | 41.92 | 0.00 | 100.00 |

Source: Spanish Labour Force Survey (2000-2013), for spanish women from cohorts 1961-1971, except the cohort of 1966.

Table A2: Descriptive statistics of the Birth Statistics

| | Treatment 1 | | | | | Treatment 0 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Min. | Max. | Observations | Mean | Std. Dev | Min. | Max. |
| Age women when first child | 1130537 | 27.60 | 5.60 | 14.00 | 41.00 | 1338576 | 27.66 | 5.59 | 14.00 | 41.00 |
| First birth before 18 | 1130537 | 0.03 | 0.18 | 0.00 | 1.00 | 1338576 | 0.03 | 0.18 | 0.00 | 1.00 |
| First birth between 18 to 21 | 1130537 | 0.15 | 0.36 | 0.00 | 1.00 | 1338576 | 0.15 | 0.36 | 0.00 | 1.00 |
| First birth between 25 to 30 | 1130537 | 0.31 | 0.46 | 0.00 | 1.00 | 1338576 | 0.32 | 0.47 | 0.00 | 1.00 |
| First birth more than 35 | 1130537 | 0.11 | 0.31 | 0.00 | 1.00 | 1338576 | 0.11 | 0.31 | 0.00 | 1.00 |
| Maturity of first child | 1130537 | 0.91 | 0.29 | 0.00 | 1.00 | 1338576 | 0.91 | 0.29 | 0.00 | 1.00 |
| Weeks of gestation of first child | 834539 | 38.99 | 2.23 | 20.00 | 45.00 | 1001204 | 39.00 | 2.22 | 20.00 | 46.00 |
| Survival 24h of first child | 1117355 | 1.00 | 0.05 | 0.00 | 1.00 | 1328234 | 1.00 | 0.04 | 0.00 | 1.00 |
| Weight of fist child | 981450 | 3201.91 | 512.20 | 500.00 | 6500.00 | 1173683 | 3198.46 | 511.67 | 500.00 | 6400.00 |
| Weight less of 2, 500 grams | 981450 | 0.07 | 0.26 | 0.00 | 1.00 | 1173683 | 0.07 | 0.26 | 0.00 | 1.00 |
| Number of first births by cohort and treatment | 10 | 113053.70 | 3673.79 | 107583.00 | 120082.00 | 10 | 133857.60 | 4018.42 | 129375.00 | 141034.00 |
| Number of total births by cohort and treatment | 10 | 204581.00 | 10470.53 | 188545.00 | 219516.00 | 10 | 240727.70 | 10698.19 | 226947.00 | 255488.00 |

Source: Birth Statistics (1975-2012), all births of spanish women from cohorts 1961-1971, except the cohort of 1966.

## Table A3: Descriptive statistics of the Population and Housing Census of 2011

| | Treatment 1 | | | | | Treatment 0 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Min. | Max. | Observations | Mean | Std. Dev | Min. | Max. |
| Percentage of woman with children | 136170 | 0.80 | 0.40 | 0.00 | 1.00 | 160290 | 0.80 | 0.40 | 0.00 | 1.00 |
| Number of children by woman | 136170 | 1.53 | 1.02 | 0.00 | 17.00 | 160290 | 1.52 | 1.01 | 0.00 | 18.00 |
| Percentage of woman with more than 3 children | 136170 | 0.12 | 0.32 | 0.00 | 1.00 | 160290 | 0.11 | 0.31 | 0.00 | 1.00 |

Source: Population and Housing Census (2011), for spanish women from cohorts 1961-1971, except the cohort of 1966.

## Table A4: Descriptive statistics of the Marriage Statistics

| | Treatment 1 | | | | | Treatment 0 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Min. | Max. | Observations | Mean | Std. Dev | Min. | Max. |
| Age women when first marriage | 1062004 | 25.50 | 4.97 | 15.00 | 41.00 | 1260357 | 25.53 | 4.96 | 15.00 | 41.00 |
| Number of first marriages by cohort and treatment | 10 | 106200.40 | 3325.58 | 100871.00 | 112261.00 | 10 | 126035.70 | 3951.76 | 121942.00 | 132402.00 |
| Number of total marriages by cohort and treatment | 10 | 109216.60 | 3246.67 | 104271.00 | 115196.00 | 10 | 129750.70 | 3927.31 | 125034.00 | 135854.00 |

Source: Marriage Statistics (1975-2012), all marriages of spanish women from cohorts 1961-1971, except the cohort of 1966.

## Table A5: Descriptive statistics of the Spanish National Health Survey of 2006

| | Treatment 1 | | | | | Treatment 0 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Min. | Max. | Observations | Mean | Std. Dev | Min. | Max. |
| Smoke/day | 1445 | 0.32 | 0.47 | 0.00 | 1.00 | 1706 | 0.32 | 0.47 | 0.00 | 1.00 |
| Smoke regular | 1445 | 0.35 | 0.48 | 0.00 | 1.00 | 1706 | 0.35 | 0.48 | 0.00 | 1.00 |
| Ex-smoker | 1445 | 0.22 | 0.42 | 0.00 | 1.00 | 1706 | 0.23 | 0.42 | 0.00 | 1.00 |
| Pregnancy as motive for being ex-smoker | 1445 | 0.05 | 0.22 | 0.00 | 1.00 | 1706 | 0.04 | 0.20 | 0.00 | 1.00 |

Source: Spanish National Health Survey (2006), all spanish women from cohorts 1961-1971, except the cohort of 1966.