The Effect of Increasing the Legal Working Age on Women's Fertility and Infant Health *

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Abstract

We use an exogenous variation in the Spanish legal working age that was introduced in 1980 to investigate the effect of a child labor reform on fertility and infant health outcomes. We show that the reform decreased marriage and fertility rates for affected women. However, we also find evidence that the reform is 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 increase in unmarried and single mothers, and the improvement in the labor market conditions of more educated women, which increases the likelihood of engaging in unhealthy behaviors such as smoking. Our results are more relevant, from a policy perspective, to developing countries whose educational system, child labor market participation rates, and women's social development are similar to the levels that Spain was experiencing around 1980.

JEL Codes: J81, I25, I12, J13

Keywords: minimum working age, fertility, marriage, infant health

*We gratefully acknowledge the support from project ECO2014-52238-R. We thank 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), Workshop on Health Economics (Universidad de Alicante) and IZA summer school in labor economics (2016). The paper was previously circulating with the title: "The unintended effects of increasing the legal working age on family behaviours". The usual disclaimer applies.

1 Introduction

Decreasing fertility rates is one factor contributing to the aging 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 have a total fertility rate (TFR) below replacement, 2.1 births per woman. For instance, the TFR in 2015 was 1.6 births per woman in the European Union and 1.8 births per woman in the US.[1] Lately many governments enacted policies to raise fertility in order to slow population aging.[2] Children's health outcomes at birth are another crucial public policy matter. It has been well established in the literature that infant health outcomes have long-term consequences in terms of cognitive development (Figlio et al., 2014), adult health (Fletcher et al., 2010) and productivity (Smith, 2009).

Some researchers have pointed to the role of education in explaining the reduction in fertility rates and improvements in infant health outcomes. An extensive literature has examined the causality between education and fertility (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), marriage (Kırdar, 2009), or infant health (Behrman and Rosenzweig, 2002; Currie and Moretti, 2003; McCrary and Royer, 2011) using changes in compulsory schooling laws as a source of exogenous variation on individual schooling choices. Another part of the literature investigates the impact of marriage law changes on poverty (Dahl, 2010) fertility, and schooling outcomes (Bharadwaj, 2015; Buckles et al., 2011). In contrast, the effect of child labor laws on long-term outcomes has been fairly overlooked. Goldin and Katz (2011), Lleras-Muney (2002) and Edmonds and Shrestha (2012) have examined the effect of child labor laws, jointly with other compulsory schooling laws, on educational attainment. However, contrary to our setting, the minimum age to work in those settings was set at a lower age than the maximum age of compulsory schooling.

We contribute to this literature by focusing on the long-term effects of child labor laws on marriage, fertility and infant health outcomes. For that, we take advantage of a quasi-natural experiment. In 1980, a new child labor regulation was enacted, the Workers Statute (Law 8/1980), which changed the minimum legal age to work in Spain from 14 to 16 years old, while the compulsory schooling age was maintained at 14. We use a differences-in-differences strategy to identify the reforms within-cohort effects. In our setup, treated and control individuals will only differ in their month of birth.

[1] United Nations Population Division. World Population Prospects.

[2] 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 which are financed through the national health insurance system.

We find that the reform prompted a postponement of first births by 21 days on average. Importantly, we show that is translated into a long-term effect in fertility as it is not followed by a catching-up effect. The reform increased a woman's probability of ending her fertile lifecycle without any children by 0.12 percentage points and reduced her completed fertility. As a consequence, we find that 2,198 women born in the first 10 years after the reform do not become mothers. In turn, this resulted in 4,160 fewer children born from these women.

Importantly, we show that the postponement in fertility is detrimental for the health of their offspring at the moment of delivery. We find that, for affected mothers, the reform increased the probability of having a first child at less than 37 gestational weeks by 0.223 percentage points. This result implies that affected women had 2,789 more children born with less than 37 weeks of gestation. Moreover, these mothers also had a higher probability (by 0.186 percentage points) of having low birth weight babies after the reform. We propose three different channels through which the reform could be negatively impacting infant health. The first is the postponement of the age at which women have their first child, 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 outcomes. The second channel operates through changes in maternal marital status. We show that the reform increased the number of unmarried mothers as well as the number of children without fathers. Previous literature has proven that the lack of a father can be detrimental for the health of the baby at the moment of delivery. The third channel that we propose is changes in labor market prospects and unhealthy habits of affected women. More precisely, we find that the reform decreased the probability of treated women having a low skill job or a part-time job. Simultaneously, we show that better employment prospects of more educated women enhances unhealthier behaviors (smoking and drinking), further contributing to the negative effects that we report on infant health outcomes.

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. For example, 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. More importantly, this positive gradient between education and smoking for women is not unique to this setting. Previous research has established a higher smoking prevalence among high educated women than among low educated women in Eastern Europe and Eastern Mediterranean countries (Bosdriesz et al., 2014). Also, Pampel (2003) showed that high-income countries at early stages of the smoking epidemic (like the southern European countries a few decades ago) had higher rates of female smokers among the young and highly educated. This is due, primarily, to the weakening of the social and cultural constraints that prevented many women from smoking in the past (Mackay and Amos, 2003).

This paper contributes to previous literature in several ways. First, as far as we are aware, this is the first paper that investigates the effect of a child labor regulation on long-term outcomes, such as fertility and infant health. Child labor reforms differ from compulsory schooling reforms in many aspects. For one, the type of children affected will be different with each type of reform. Compulsory attendance laws normally specify a minimum and a maximum age between which school attendance is required. Then, if these law are really enforced, all affected children will see their number of years of education increased. On the other hand, child labor reforms set a minimum age to start working. If the minimum legal age to work is higher than the compulsory schooling age, as in the setting we are exploiting, the child labor reform will only act as a subtle incentive for children to continue in the educational system. Children will only be encouraged to continue studying because they are not able to work, not because the law is requiring them to attend school. Thus, child labor reforms will be more likely to increase educational attainment of those children whose main motivation to dropout was the need to contribute to the household income by working. Therefore, our strategy is substantially different than the previous literature, which has examined the role of education over these family behavior outcomes using compulsory schooling laws. We exploit the interaction between the compulsory schooling age and the minimum legal age to work to identify the incentives of different types of individuals. As Lleras-Muney (2002) pointed out when child labor laws and compulsory attendance laws are not coordinated, they are ”more likely to work in combination with one another”. Then, we argue that both age thresholds affect the decision to remain in the educational system so that it is important to consider both factors at the same time. Also, increasing the minimum legal working age could be a more efficient and cost-reducing way of increasing educational attainment as opposed to an increase in the number of years of compulsory schooling.

Secondly, it contributes to the discussion about the link of education and fertility and infant health outcomes in middle-income/developing countries. Previous evidence on the causality between education and fertility and infant health, using changes in the compulsory schooling laws, have largely focus on developed countries. For instance, previous studies have exploited several reforms in compulsory schooling in the US (Black et al. (2008), León (2006)), Norway (Monstad et al., 2008), Italy (Fort, 2007), the UK (Silles (2011), Geruso et al. (2014)), Germany (Cygan-Rehm and Maeder, 2013), and Europe (Fort et al., 2011). However, the reform that we are exploiting in this paper 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). The bottom panels of Figure 1 show the age of the first Social Security contribution for pre- and post-reform cohorts. Prior to the reform, for 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. One could think that a substantial part of this employment was in the informal market and, thus, not captured in the Spanish Labor Force Survey or the Social Security contributions. The Spanish Household Budget Survey of 1980-81 (Alonso-Colmenares et al., 1999) reveals that, after the reform, only 2.1 percent of the boys and 1.2 percent of the 14-years-old girls were participating, formally or informally, in the labor market. Likewise, 9.63 (5.1) percent of the 15-years-old boys (girls) were participating in the labor market in 1980. Thus, one third of the employment of children under 16 years old was in the formal market, and the reform not only eliminated child formal work, but also reduced the informal part of child employment (under 16 years old). Thus, our results are more relevant, from a policy perspective, to developing countries whose educational system, child labor market participation rates, and women's social development are similar to the levels that Spain was experiencing around 1980.

Thirdly, the majority of the literature has used changes in the state compulsory schooling laws as an instrument for years of education. We normally expect an educational reform to not only increase the number of compulsory years of education, but to be accompanied by other changes in the educational system. This makes it difficult to disentangle the effect of a simple increase in years of education from the improvement of the quality of education [3]. In this paper, instead, we are exploiting an increase in the incentives to finish primary school that do not affect in any other way the educational system. Thus, we are able to isolate the effect of an increase in the number of schooling years, with everything else remaining equal.

Fourthly, unlike most of the extant literature, we use registered data of all births and marriages in Spain, which allows us to observe the universe of all birth and marriages that took place during more than 30 years.[4] This type of data has some advantages over census data, which only identifies 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 have an impact on this number. If the level of education affects the probability that some of these situations occur, then census data could bias the results.

Finally, we are able to estimate the within-cohort effects of the reform, improving upon the identification strategies previously used in the literature. Large part of the literature performed a between-cohort estimation, comparing cohorts of individuals affected or not affected by a law that increased the year of compulsory schooling. In our setup, instead, treated individuals (born between January and May) and their control counterparts (born between July and December)[5] only differ in their month of birth. Consequently, our identification strategy will be robust to any concurrent social or political event, as they will impact in the same way both our treatment and control groups. Other strand of the literature exploit within-cohort differences in the years of compulsory schooling. For instance, Angrist and Keueger (1991) take advantage of the fact that compulsory schooling laws specify the age at which children could leave school, so those children born at the beginning of the year obtain fewer years of schooling. However, there has been lately a controversy about whether the month of birth affects education only through compulsory schooling laws. In our identification, thought, we do not rely on the assumption that individuals born in different months are equal. As we perform a difference-in-differences approach, our assumption is that if there exist differences between those born at the beginning and at the end of the year, these differences do to change between cohorts except for application of the reform.

The remainder of the paper is organized as follows. Section 2 presents the institutional context

[3] Brunello and Paola (2014) examined several policies that are expected to affect early school leavers, including minimum school leaving age and improvement in the quality of teaching. They concluded that, even though the comparison of different policies on the basis of cost-benefit analysis is very difficult, responsiveness differs between policies.

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

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

as well as the identification strategy. In Section 3 we present the effects of the reform on fertility rates and infant health outcomes. In Section 4, we perform a number of robustness checks while Section 5 concludes with a discussion of the main results and their policy implications. At the end of the paper, a data appendix can be found, with a detailed explanation of all the databases used in the paper, as well as a broader before-after analysis of the reform.

2 Institutional Context and identification strategy

Our identification strategy builds on an exogenous variation in the incentive to stay out of the labor market 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 as a child labor law that 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 was 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.

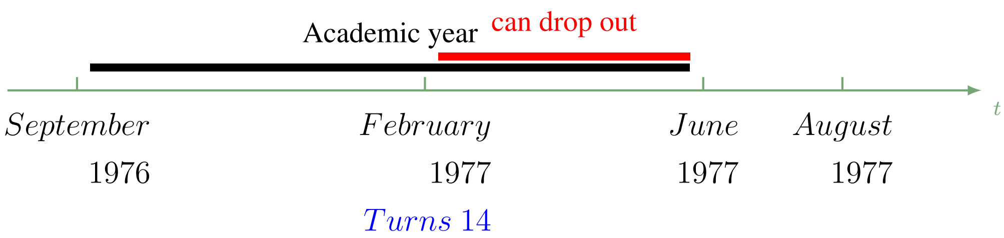
Additionally, not all individuals from the same cohort were affected by the reform in the same way. In the Spanish educational system, all children from the same cohort start school the same year. Consequently, children born at the beginning of the year start school when they have already 6 years-old, while those born at the end of the year are still 5 years-old. In the same way, children born at the beginning of the year turn 14 during the last year of primary education, while those born at the end of the year are still 13 years old. Then, 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. 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 primary education. Consequently, we expect that before the reform was passed, those born at the beginning of the year would 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.

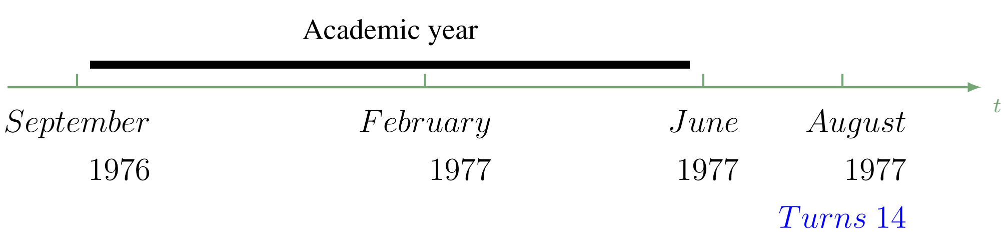
The following chart illustrates the timing of the reform by showing two individuals in the same

1963 cohort (pre-reform), during their last year of primary school:

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



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



Thus, 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). After the reform these differential incentives disappeared.

2.1 Identification strategy

We exploit the exogenous change in the incentives introduced by the ET reform to identify the causal effect of a child labor regulation 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, in the appendix, 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 fertility and marriage 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.

Formally, we consider the following econometric model for the different family behavior outcomes of woman i from cohort c observed in year t:

$$Outcome_{ict} = \alpha + \beta_1 Treated_i + \beta_2 Treated_i * Post\ Reform_i + BY_c + CY_t + R_s + \epsilon_{ict}$$

where $Treat_i$ is a dummy variable that equals one if the woman i was born between January and May and zero if she was born between July and December. $Post\ Reform_i$ is also a dummy variable that takes a value of one if the woman i turned 14 after the reform and zero otherwise. Then, we define pre-Reform cohorts as those born in 1961 to 1965, and post-Reform cohorts as those born between 1967 and 1971. We also include region (R_s), cohort (BY_c) and calendar year (CY_t) fixed effects. Note that we will cluster the standard errors at the cohort level. They will be reported always in parenthesis. However, given the small number of clusters, we also perform a wild bootstrap with 1,000 repetitions and we report the p-value in brackets. The effect of the reform can be identified by the coefficient of the interaction between the post-reform and the treatment dummy variables, β_2. We will use this same econometric specification in the rest of the paper. All results are robust to the exclusion of region fixed effects and the inclusion of the interaction between cohort and region fixed effects, as well as the substitution of cohort time dummies by linear, quadratic and quartic pre- and post-reform trends.[6]

When examining the effects of the reform over the infant health at delivery of their offspring, we use the same econometric model as before but set at the level of child j of mother i from cohort c observed in year t:

$$Outcome_{jt}^{ic} = \alpha + \beta_1 Treated_i + \beta_2 Treated_i * Post\ Reform_i + BY_c + CY_t + R_s + \epsilon_{jt}^{ic}$$

Note that we are assuming that the reform did not have any effect for the cohort of individuals that

[6]Results of these experiment are available upon request.

were between 14 and 16 years-old when the reform passed (those individuals born in 1964, 1965 and 1966). In other words, we are assuming that when the reform was enacted all those individuals that were older than 14 but younger than 16, and could have been working before the reform, had to quit their jobs and returned to the educational system. We are aware that this is a strong assumption, so we evaluate the robustness of our results to the soften this assumption in Section 4.1.

We borrow the first stage of the reform from Jiménez-Martín et al. (2015). They show that the reform was effective in providing incentives for treated individuals to, not only finish primary education, but also to continue in the educational system afterwards. In particular, they find that the increase in the minimum statutory working age decreased the number of early school leavers (individuals not finishing primary education) in 1.61 percentage points (7.16%) for men and 0.98 percentage points (7.7%) for women. At the same time, the reform decreased the number of treated individuals not attaining post-compulsory education (dropouts) by 1.82 percentage points (3.7%) for men and 1 percentage points (2.28%) for women. These results certify that the reform was effective in increasing educational attainment of affected individuals and, thus, in restricting child labor. For the rest of the paper we will analyze the effects of the reform on fertility and marriage outcomes as well as on infant health at delivery.

3 Effect of the Reform on Family Behavior Outcomes

3.1 Effect of the Reform on Fertility

We first study the impact of the reform on several fertility outcomes. To test whether women affected by the reform postpone motherhood, we first examine the impact of the reform on the age at which women have their first child. Secondly, we assess whether the reform 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 2014, 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 43. This age restriction allows us to include the same ages for all the cohorts considered, as women of the first cohort (1961) were 14 in the first year of the register and women of the last cohort (1971) were 43 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.[7] 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. Thus, to examine the effect of the reform on the probability of ending childless and completed fertility, 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 1 show the effect of the reform on the age at which women had their first child. Before the reform, women born at the beginning of the year had their first child almost a month (28 days) earlier than women born at the end of the year. This gap in age disappears (is reduced by 21 days) after the reform is introduced.[8] Similarly, in graph a) of Figure 2 we can observe that the difference in the age at which women have their first child between women born at the beginning and at the end of the year is significantly negative for all the pre-reform cohorts, while this difference is not longer significant for cohorts affected by the reform.

Table 1 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.[9] Thus, 221 more women born in 1967[10] decided to remain childless, a decrease of 0.19 percent in the number of women that have children after the reform was implemented.[11] Given that we are exploiting, on average, three additional months of education,[12] the effect of the reform over the probability of not having any children[13] is lower than León (2006) in the US (who find that an increase in an additional year of schooling raises the probability of not having any children by almost 2 percentage points), and Cygan-Rehm and Maeder (2013) in Germany (that find that an increase in an additional year of schooling raises the probability of not having any children by 5.1 percentage points).[14]

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

[8]Results are robust in sign and significance to the substitution of cohort time dummies by linear, quadratic and quartic pre- and post-reform trends, the exclusion of region fixed effects, and the incorporation of the interactions of cohort and region fixed effects.

[9]Be aware that we are only considering births that took place between the ages of 14 and 43. Thus, we cannot completely rule out the catching-up effect, as this effect could be taking place after the age of 43. However, only 0.66% of women of these cohorts have their first child with more than 43 years old.

[10]or 2,198 born between 1967 and 1976

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

[12]The introduction of the reform increased the incentives to continue five additional months of primary school for children born during January, four months for those born in February, three months for those born in March, two months for those born in April and two month for children born in May.

[13]We find that an increase of three months of schooling increases the probability of not having a child by 0.16 percentage points, which would be equivalent to an increase of 0.64 percentage points for an additional year of education.

[14]The rest of the papers in the literature do only find a postponement effect of fertility away from the teenage years

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 419 fewer children were born to the first post-reform cohort of women (born in1967)[15] 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. This effect on completed fertility is lower to León (2006) and Cygan-Rehm and Maeder (2013), which find that an additional year of education reduces fertility by 0.25 and 0.1 children respectively, while the reduction on fertility as a consequence of the reform in Spain is of 0.012 children for additional year of schooling. On the other hand, the estimated effect on completed fertility is bigger than the zero effect found before by Black et al. (2008), Fort (2007), Monstad et al. (2008), Silles (2011), or Geruso et al. (2014). In graphs b) and c) of Figure 2 we can observe that the difference in the probability of having children or the total number of children between women born at the beginning and at the end of the year is significantly positive for all the pre-reform cohorts, while this difference is not longer significant or even negative for cohorts affected by the reform.

Fertility and completed fertility are calculated as the ratio between the first births or total number of births and the total number of women born in a certain cohort and treatment status. One potential concern is that the reform impacts mortality and migration differently for treated and control women born after the reform. If, as a consequence of the reform, the mortality rate or migration of treated women born after the reform decreased, we could incorrectly estimate a decrease in the fertility and completed fertility rates by affecting the denominator (treated women of childbearing age living in Spain) rather than the numerator (births). To address this potential concern, we test if the ratio between the treated and control women observed in census and born with in each cohort varies with the reform. In Figure A1 we can observe that the proportion of treated and control women within each cohort that are represented in the Census of 2001 is quite constant around 0.048 for all the cohorts of women. Thus, we believe that differential migration and mortality rates for treated and control women is not driving the fertility results.

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).[16] Table 2 shows that the reform's

(Black et al., 2008; Fort, 2007; Silles, 2011; Geruso et al., 2014).

[15] or 4.160 children in the subsequent 10 generations affected by the reform.

[16] Note that we are considering the same cohorts of women (1961–1971) and are defining treatment in the same way. However, in 2011, the last cohort we are considering (1971) had only reached the age of 40, so we do not observe the

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 three 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 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. Third, we only observe those children that are still in the household of their parents at the moment of the interview. It is plausible that older children from the less educated households are no longer living with their parents. This makes the census data a selected sample that biases the results. Thus, we strongly believe that the census data represent a worse database to analyze the effect of the reform on fertility outcomes.

Therefore, we conclude that the reform had two main effects on fertility. First, it made some women postpone the entrance into motherhood and this delay was not compensated for later in life.[17] Second, it increased the number of women that remained childless.

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 the reform delays entrance into motherhood until an age 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 the outcome now is the probability of having the first birth at a certain age bracket. We choose the age brackets to all have the same number of years (5 years).

Table 3 reveals no significant effect of the reform on the probability of having the first child during the teenage years (between 14 and 18 years old). In contrast to the findings of prior studies, this evidence indicates that the reform did not induce a postponement of first births away from the teenage years. Thus, we can rule out the "incarceration effect"[18] as the main channel through

late births of the younger cohorts. Also, we can no use the age constraint, as we do not have information on the age of the mother when they had their children. So, we observe a higher number of births from the older (and probably less educated) women.

[17] We also evaluate this result using the 2006 "Encuesta de Fecundidad" (i.e. *Fertility Survey*), available from *Centro de Investigaciones Sociológicas* (CIS). 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.

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

which the child labor reform affected fertility.

On the other hand, the reform did affect the probability of having the first child after age 29. Our results show that, after the reform, affected women had a lower probability of having their first child between the ages of 29 and 33, as well as a higher probability of having their first child after the age of 34. Therefore, we conclude that the reform decreased the probability of pregnancy during the early thirties and increased the probability of having late first births.[19]

Even if the postponement of 21 days on average seems like a small effect, the increase in the incidence of first births after the age of 34 is not. The medical literature has shown that after age 35 a woman's fertility decreases. Thus, catching-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, 88.86% of mothers were married when they had their first child. Thus, as an additional potential factor that may help explain the effects of the child labor reform on fertility, we analyze whether the reform had any impact on marriage outcomes. First, we study whether the reform induced women to postpone the age at which 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.

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 the treatment and control groups and divide them by the total number of women born to a certain cohort and treatment status. Similarly, to calculate the probability of having never married we divide the total number of first marriages by the total number of women born to a certain cohort and treatment status.

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

Table 4 shows the effects of the reform on the age at the time of marrying. 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.[20] Note that this postponement in the age of first marriage is almost identical to the postponement in the age of having a first birth.

Using data collapsed at the cohort level, with cells defined at the level of treatment, cohort, year of marriage and region, Table 4 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 reduction in the total number of marriages per woman.[21]

Summing up, we conclude that the reform postponed first marriages and, consequently, postponed the age at which 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 29 and 33. The reform increased the incidence of first births after the age of 34, which is an age at which women's fertility begins to drop, resulting in a reduction in completed fertility rates.[22]

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 reform has an impact on infant health, we can argue that child labor regulations can have intergenerational externalities that should be taken into account when thinking about the design of

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

[21]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.

[22]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.

these policies.

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,[23] the fraction that are born after more than 37 gestational weeks,[24] 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 2014. 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 43.[25]. It should also be noted that the birth weight is missing from 11 percent of all registered first births. However, as it can be observed from Table A1 that the probability of not having registered birth weight is not affected by the reform.

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 born at the beginning of the year 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. Thus, we will also analyze the effect of the reform over the sex ratio (the probability of having a male first birth). This outcome can be considered a proxy for miscarriage as male births are known to miscarry more often. The medical literature argues that hormones induced by stress increase the probability of spontaneous abortions at an early stage of pregnancy, and these hormones have a larger effect on male than on female fetuses (Hobel et al., 1999; Byrne et al., 1987).

Note that this analysis only examines health at birth 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).

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

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

[25]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 (from 14 and 18), so we are confident that we don't have a selected sample.

Table 5 reports the effects of the reform on the sex ratio and the four infant health outcomes we have just mentioned using the same econometric model as before. First of all, we find that the reform did not have any effect on the sex-ratio. After the reform, treated women did not have more first-born children of a certain gender, which provides further evidence that differential miscarriage is not a possible problem in our setting. Furthermore, we find that the reform has a negative impact on the health of children born to treated women (women born at the beginning of the year).[26] After the reform, the first child of a woman born at the beginning of the year has a 0.223 percentage-point (0.24%) higher probability of being premature (born with less than 37 gestational weeks). This translates into 290 more children of women of the first post-reform cohort (born in 1967) that are premature due to the reform.[27]

Apart from the infant health outcomes, we also find that the reform increased the probability of having a multiple birth in 0.239 percentage points. This might be a consequence of the postponement of the entrance into motherhood. As shown before, we find that the reform increased the incidence of first births after the age of 35, the age when women's fertility begins to drop. Many of these women might start receiving infertility treatments, which have a higher probability of multiple pregnancy. Also, at ages 35 or more, the probability of having multiple births increases, even without fertility treatments.[28] On the other hand, we do not find any effect of the reform on the probability of the first child surviving the first 24 hours.

The reform also caused women born at the beginning of the year to have children that weighted 4.69 grams less, on average, compared to children of women born at the end of the year. While 4.69 grams may not seem like a lot, it has to be taken into account that this is the estimated average impact of the reform. In fact, this result is of similar magnitude as the change in birth weight brought on by several US federal nutrition programs. For instance, Hoynes et al. (2011) determine that the Supplemental Program for Women, Infants and Children in the United States led to an increase in average birth weight of around 2 grams. Similarly, Almond et al. (2011) estimate that the US Food Stamp program increased the average birth weight between 2 and 5 grams. Moreover, we estimate that after the reform, women born at the beginning of the year have a 0.186 percentage points

[26]Results are robust in sign and significance to the substitution of cohort time dummies by linear, quadratic and quartic pre- and post-reform trends, the exclusion of region fixed effects, and the incorporation of the interactions of cohort and region fixed effects.

[27]2,789 if we take into account the 10 consequent cohorts.

[28]Given that the reform affects the probability of having multiple births, we cannot examine the effects of the reform on infant health outcomes excluding multiple births, as the resulting first births will constitute a selected sample. Though, if we restrict the sample to single first births, the effects of the reform over infant health go in the same direction.

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.[29] As the percentage of children born weighing less than 2,500 grams is not very large,[30] this effect implies an increase of 2.7 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 the scarce evidence presented in the extant literature, which finds either a positive impact of maternal education on child health (Currie and Moretti, 2003), or no causal effect (McCrary and Royer, 2011). Thus, in the next subsection, we propose three possible channels through which the child labor reform could have a negative impact on infant health.[31]

3.2.1 Explanatory Mechanisms

The Postponement of First Births

A first channel through which the reform operates is the postponement of the entrance into motherhood. We have shown in Table 3 that the reform increases the probability that women have their first child after the age of 34. Previous medical literature has indicated that having a first birth after the age of 35 could have negative effects on infant health as risk during pregnancy increases after that age. For instance, Jolly et al. (2000) find that advanced maternal age is correlated with an increased likelihood of delivering a small for gestational age baby, which may be related to poorer placental perfusion or transplacental flux of nutrients. Likewise, older women are more likely to deliver preterm.

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

[29] 4,352 in the subsequent 10 generations of women.

[30] Only 6.4% of children from the cohort of women born before 1966 were born weighing less than 2,500 grams.

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

al., 1999; Bennett, 1992; Balayla et al., 2011) has established that children whose mothers are not married or have no father in the birth certificate data have worse health outcomes at the time of delivery. Table 6 shows that the reform significantly increased the probability that first children did not have a registered father by 0.219 percentage points (0.2%) and the probability that the mother is not married by 0.289 percentage points (0.33%). 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. It seems plausible that, if the child labor reform proofs to have increased the educational attainment of treated women, it could also have affected their probability of working or the type of job that they have. Thus, the level of stress of treated women or even their health behaviors could be changed due to the changes in labor market outcomes (through, for example, an income effect). This, in turn, could affect the health of their babies at birth. 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 long-term 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 2013. Table 7 shows that the reform has a positive but not significant impact on the probability of working. We also observe that, after the reform, treated women have a significantly lower probability of being in a low skill job or having a part-time job (about 1 pp in either case) as compared to women born at the end of the year. Both results suggest potentially more demanding jobs which could be linked to potentially higher stress as well as income levels.

Finally, we use data from the Spanish National Health Survey (see the data appendix for more information on this database) to determine whether the labor market impacts of the child labor reform are also translated into differences in health behaviors of treated women after the reform.[32] Table 8 shows that, after the reform, women born at the beginning of the year have a higher probability of smoking regularly and they also smoke more cigarettes per day.[33] Although the reform

[32] Although this survey is available for several years, only the 2006 and 2012 wave reports the individual's month of birth, which is a crucial variable for our identification strategy. Therefore, the results that we report are for the 2006 and 2012 waves and include cohorts from 1961 to 1971.

[33] Results are robust in sign and significance to the substitution of cohort time dummies by linear, quadratic and

did not impact the probability that these women are ex-smokers, we do find that after the reform women born at the beginning of the year have a lower probability of quitting smoking during pregnancy (conditional on being ex-smokers and having kids). These outcomes could directly affect the health of their offspring.

This unexpected result 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. For instance, a recent paper by Bilal et al. (2015) shows a high negative correlation between gender inequality and the female-to-male smoking ratio in Spain from the 1960s to the 2010s.

More importantly, this positive association between education and prevalence of smoking for women cannot be considered an isolated and particular case of Spain at that time. In many countries in the world, the number of smoking women is increasing, even though smoking prevalence among women is still lower than among men. This phenomenon can be attributed to the weakening of the social and cultural constraints that prevented many women from smoking in the past (Mackay and Amos, 2003). In some Eastern European countries and Eastern Mediterranean countries a high smoking prevalence among high educated women compared to low educated women has been established by previous literature (Bosdriesz et al., 2014). This same pattern has been found to hold (Pampel, 2003) in other high-income countries at early stages of the smoking epidemic. Then, the process of gender equalization and the initial adoption of tobacco consumption that was taking place in the early post-Franco era in Spain could explain the positive correlation that we find between smoking prevalence and education among women. Those women affected by the child labor reform had higher education and financial independence that improved their social status and hence an autonomy to emulate their male counterparts' life style.

Thus, we find evidence that the child labor reform has positive impacts on affected women, as they have a lower probability of being in low skill jobs, a lower probability of having a part-time job and they also increase their educational attainment. However, the impact of the reform on their children is negative. The better employment perspectives of women 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 show that part of these negative health outcomes on children can be attributed to the postponement of fertility after the age of 35 and the increase in

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quartic pre- and post-reform trends, the exclusion of region fixed effects, and the incorporation of the interactions of cohort and region fixed effects.

unmarried mothers and children without fathers.

4 Robustness Checks

In this section, we perform several robustness checks of our key results. More specifically, we examine the robustness of the results if we take the cohorts of women from 1964 to 1966 as partially affected by the reform. We also explore the sensitivity of our results to the exclusion of some pre-reform cohorts that could be considered non-compliers and perform some placebo tests in which we change the definition of the timing of the reform (fake reforms). We also examine the influence of father's education on fertility and infant health outcomes by including additional variables that control for the characteristics of the father. Finally, we analyze the robustness of our results when we reduced the sample to include only individuals born during the middle months of the year.

4.1 Consider the Cohort 1964, 1965 and 1966 as Partially Affected by the Reform

The ET reform was enacted in March of 1980 and, as we have already explained, increased the minimum legal age to work from 14 to 16 years old. Then, all individuals born after 1966 (and more specifically those born after March of 1966) were fully affected by the reform and could not start working until they turned 16 years old. At the same time all those women born in 1963 or before (or more specifically those born before March 1964) were already 16 the year the reform took place and were not affected at all by the reform. However, all women born between March of 1964 and February of 1966 were between 14 and 16 years old when the reform was enacted and could have been partially affected by it.

In our previous specifications we dropped all women born in 1966, who were 14 the year the reform took place, and assume the cohort of women from 1964 and 1965 to not be affected at all by the reform. In this Section we examine if our previous results are robust to the relaxation of these assumptions. Now our post-Reform variable will not longer be a dummy but will reflect the possibility of some women being partially affected by the reform. It is constructed in the following way. All women born after March of 1966 (March included) will be fully affected by the reform and the post-Reform variable will take a value of 1. All women born before February of 1964 will not be affected at all and the variable will take a value equal to 0. For all women born between March of 1964 and February of 1966, the post-Reform variable will take a value between 0 and 1 depending on how many months they had the opportunity of working before the law passed. For instance, a woman born in March of 1964 had to wait one month before they could start working, as they were lacking one month to turn 16 when the reform was passed. Then for these women the post-Reform variable will take a value of 1/24 (as those fully affected had to wait 2 years or 24 months to start working when the reform passed). In the same way, the post-Reform variable will take value of 2/24 for all women born in April of 1964, and so on. Until all women that were born in February of 1966 that were affected by reform for 23 months (the variable takes value of 23/24).

We can observe in Table 9 that the results are extremely robust in sign when this alternative specification is used, thought the results are a bit stronger. Now the estimated delay in age at which women affected by the reform have their fist child is of 25 days instead of 20. Also, 2 women per 1,000 women (instead of 1.63) decided not to have kids after the reform and 3.69 (instead of 3.09) less children (per 1,000 women) were born. In addition the impact over infant health at delivery is very similar in magnitude for the probability of having a premature child and a multiple birth. However, the effect over birth weight is quite stronger. Now we find that that the reform decreased the average birth weight by 7.25 grams (instead of 4.69) and the probability of having a low-birth child increased by 0.259 percentage points (instead of 0.18). Moreover, with this new specification we also find that the reform had a significant negative effect on the probability of surviving during the first 24 hours after delivery (by 0.037 percentage points), while with our original specification this negative effect was smaller in magnitude and not significant.

An alternative assumption is to consider the cohorts of 1964, 1965 and 1966 as potential non-compliers of the law. Then, we can check the sensitivity of our results if we drop sequentially from the analysis these cohorts. The results in Table 10 and 11 indicate that the effects of the reform on fertility and infant health outcomes are unchanged when we exclude these two additional cohorts. Thus, we can also conclude that our results are robust to the exclusion of possible non-compliers.

4.2 Placebos

We also perform several placebo tests in which we use "fake" reform years. In these tests 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.[34] We

[34] 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 start working. Moreover, these cohorts could also still be in the last year of primary schooling if they had to retake a year at school.

use the same econometric specification and treatment status definition as before. We expect a non-significant effect of the interaction term between the post-reform dummy and the treatment dummy.

In Figure 3 and 4 , 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 3 shows that none of the "fake" reforms considered has a significant effect on the age at which women have their first child. Moreover, graphs d) and e) of Figure 3 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 b) and e) in Figure 4 that the "fake" reforms for the 1962, 1963, and 1964 cohorts do not affect the probability of having a first child with more than 37 weeks of gestation or the child's birth weight. The results on multiple births and survival during the first 24 hours are less clear, as the trend difference between the treatment and control groups seems to change for some 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.

4.3 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 effect of the child labor reform on the mother but also by its effects on the child's father. 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.

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 11 shows that the reform increased the age at which fathers had their first child by almost a month, indicating that our instrument is relevant.

Table 12 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 effect of the reform over treated women but also over treated men is affecting the age at which women have their first child. On the contrary,, 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 (except for maturity and multiple births). This last result implies that the mother's characteristics 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.

4.4 Reduced Sample

Previous literature has pointed out that individuals born at the beginning of the year are typically quite different in several dimensions from individuals born at the end of the year (Bound and Jaeger, 2000; Buckles and Hungerman, 2013). Note, however, that this is not a necessary assumption in our identification strategy to correctly estimate the causal effect of the reform. The identifying assumption needed in our approach is that any difference that we observe for women born at the begin and end of the year after the reform (with respect to the differences observed before the reform) is due to the passing of the child labor reform.

Thus, even though the existing differences among women in the pre-reforms cohorts should not affect our results, we try to address the remaining possible doubts by omitting from our sample those individuals born in the first and last two months of each year.[35] Therefore, we analyze the robustness of our main results on fertility and infant health outcomes comparing only women born in months 3, 4 and 5 with women born in months 8, 9 and 10 before and after the reform. Table 13 shows that even with this reduced sample approach our main findings of the effect of the reform on the fertility and infant health outcomes are unchanged.

[35] Another robustness check (available upon request) shows that the impact of the reform is the same for individuals born during January, February, March, April or May. This results confirms that the effect of the reform comes from completing primary school and not from adding months of schooling.

5 Discussion

This study investigates the effect of a child labor regulation on fertility and infant health outcomes 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.

Jiménez-Martín et al. (2015) showed that the reform was enforced and was effective. Those women and men born at the beginning of the year (that had lower educational attainment before the reform) had higher incentives to finish primary education and continue secondary and post-secondary education after the reform.

We find that, as a consequence, the reform prompted a postponement of first births by 21 days, on average. This number is very similar to the results in the majority of the previous literature studying a different type of reform that also increased educational attainment (compulsory schooling laws). 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 2,198 women born between 1967 to 1976 do not become mothers. In turn, this resulted in 4,160 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 decreased the probability of pregnancy during the early thirties while increasing the probability of having late first births (after the age of 34).

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 0.223 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 the child labor law on children's health. The first is the effect of the increase of the age at which treated women get pregnant for the first time. The negative impacts of the child labor law on infant's health could be partly driven by 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.

The second channel operates through changes in maternal marital status. We show that the reform increased the number of unmarried mothers as well as the number of children without fathers. Previous literature has proven that the lack of a father can be detrimental for the health of the baby at the moment of delivery.

The third channel that we propose is changes in labor market prospects and unhealthy habits of affected women. More precisely, we find that the reform decreased the probability of treated women having a low skill job or a part-time 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 child labor reform had positive impacts on women by increasing their educational attainment and improving their labor market prospects, the reform had negative consequences for their children. This effect is driven by the increase in women's age at delivery, the increase of unmarried mothers and children without fathers, and by the increase in women's unhealthy habits. These results have to be considered together with the fact that the child labor reform that we are analyzing took place during the 1980s in Spain. At that time, Spain was still a developing country; 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.

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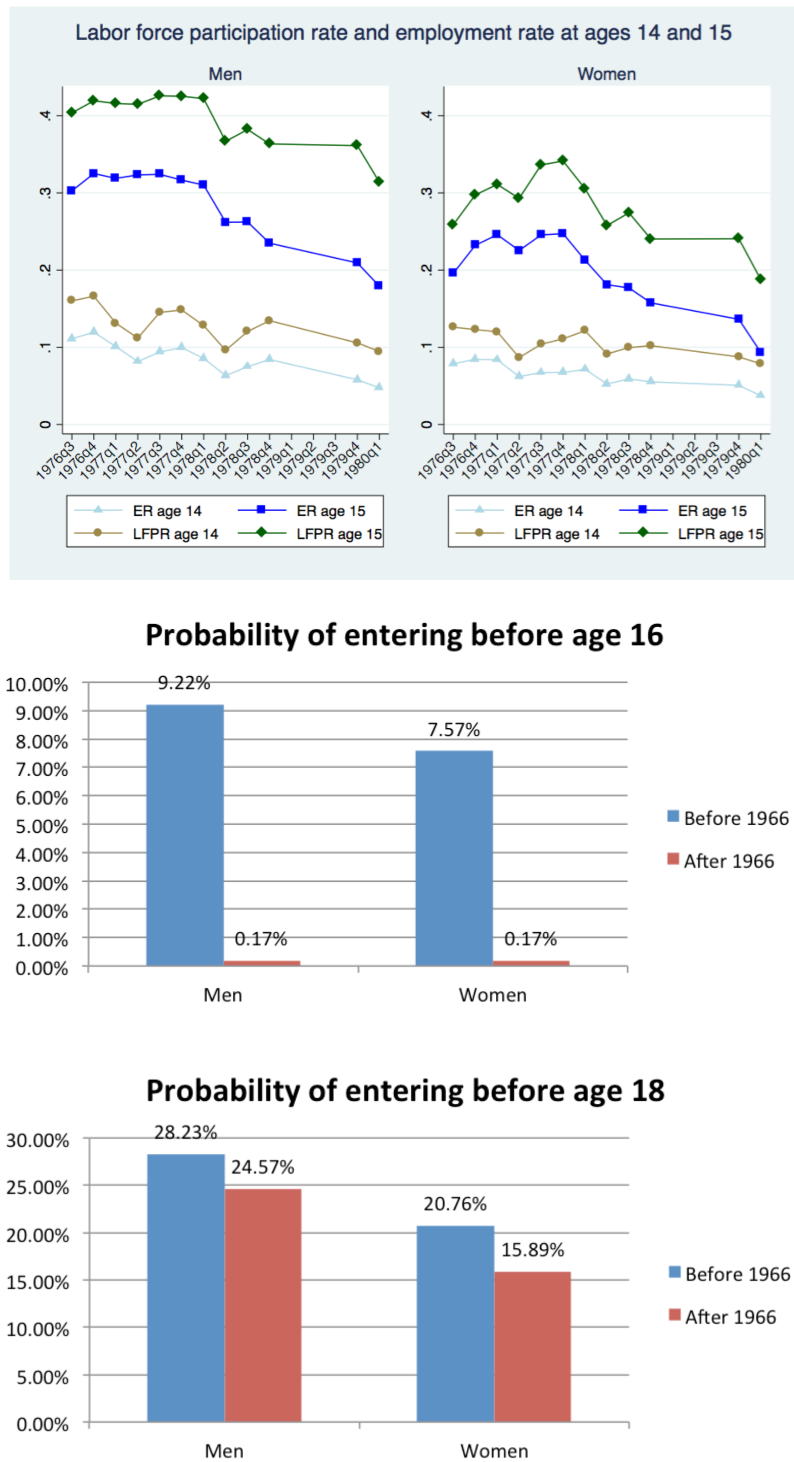
Tables and Figures

Figure 1: LABOR FORCE ATTACHMENT AND THE AGE OF LABOR MARKET ENTRY



Notes: "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).

Table 1: EFFECT OF THE REFORM ON FERTILITY OUTCOMES

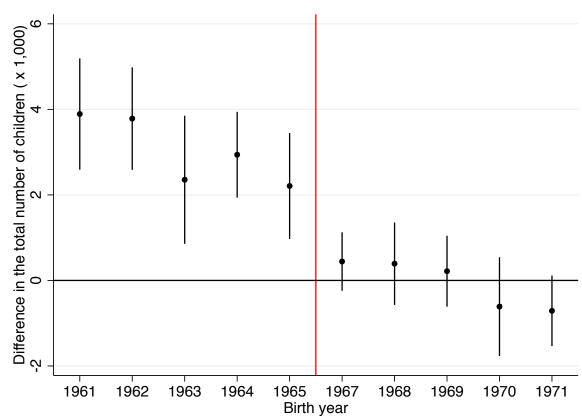
	Age first birth (1)	Perc. women in each cohort become a mother (2)	Number of children per women in each cohort (3)
Treated	-0.080**	1.319**	3.034***
	(0.009)	(0.203)	(0.390)
	[0.023]	[0.037]	[0.004]
Treated* Post Reform	0.058***	-1.627***	-3.079***
	(0.011)	(0.123)	(0.187)
	[0.002]	[0.003]	[0.003]
Observations	2,493,107	9,982	10,026
R^2	0.067	0.345	0.373
BirthYear FE	YES	YES	YES
Calendar FE	NO	YES	YES
Region FE	YES	YES	YES
Mean pre-reform	26.83	29.03	52.55
Std. dev. pre-reform	5.507	23.40	38.23

Notes: The dependent variables are (1) the age of the woman when she 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971.

Figure 2: DIFFERENCES IN FERTILITY OUTCOMES OF WOMEN IN THE SAME COHORT BORN AT THE BEGINNING AND AT END OF THE YEAR

(a) Age at which women had their first birth

(b) Proportion woman that have children (x 1000)



(c) Total number of children per women (x 1000)

Notes: Estimated difference in (a) the age (in years) woman have their first child, (b) probability they have children, and (c) total number of children between women born at the beginning (treated) and at the end of the year (control) of each cohort. Regressions include region and calendar time dummies. The confidence intervals are calculated using standard errors clustered at cohort and region levels. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971.

Table 2: EFFECT OF THE REFORM ON FERTILITY OUTCOMES USING CENSUS DATA

	Prob. of having a child (1)	Total number of children (2)	Prob. of having 3 or more children (3)
Treated	0.003***	0.017**	0.008**
	(0.001)	(0.003)	(0.001)
	[0.003]	[0.053]	[0.053]
Treated*Post Reform	-0.000	-0.005	-0.004
	(0.002)	(0.006)	(0.002)
	[0.884]	[0.497]	[0.128]
Observations	269,392	269,392	269,392
R^2	0.009	0.025	0.016
BirthYear FE	YES	YES	YES
Calendar FE	NO	NO	NO
Region FE	YES	YES	YES
Mean pre-reform	0.819	1.598	0.130
Std. dev. pre-reform	0.385	1.019	0.336

Notes: 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Census 2011, all women from cohorts 1961-1971.

Table 3: EFFECT OF THE REFORM ON THE PROBABILITY OF HAVING THE FIRST BIRTH AT A CERTAIN AGE BRACKET

	Probability of having the first birth					
	Between 14 and 18	Between 19 and 23	Between 24 and 28	Between 29 and 33	Between 34 and 38	Between 39 and 43
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.001*	0.007**	-0.003*	-0.003**	-0.001*	-0.000*
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
	[0.061]	[0.024]	[0.076]	[0.018]	[0.087]	[0.057]
Treated* Post Reform	-0.001	-0.002	0.002	-0.004***	0.003***	0.001***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)
	[0.223]	[0.232]	[0.279]	[0.009]	[0.001]	[0.001]
Observations	2,493,107	2,493,107	2,493,107	2,493,107	2,493,107	2,493,107
R^2	0.010	0.031	0.011	0.023	0.019	0.005
BirthYear FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0690	0.258	0.339	0.226	0.0874	0.0211
Std. dev. pre-reform	0.253	0.437	0.473	0.418	0.282	0.144

Notes: The dependent variables are the probability of having a first child between the ages of (1) 14 and 18, (2) 19 and 23, (3) 24 and 28, (4) 29 and 33, (5) 34 and 38, and (6) 39 and 43. 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971.

Table 4: EFFECT OF THE REFORM ON MARRIAGE

	Age first marriage (1)	Number of first marriages per woman in each cohort (2)	Number marriages per woman in each cohort (3)
Treated	-0.056***	0.444	0.384
	(0.015)	(0.232)	(0.225)
	[0.005]	[0.164]	[0.224]
Treated*Post Reform	0.047*	-1.293**	-1.264**
	(0.023)	(0.217)	(0.211)
	[0.072]	[0.011]	[0.012]
Observations	2,322,360	9,106	9,118
R^2	0.051	0.372	0.387
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	NO	YES	YES
Mean pre-reform	24.81	29.58	30.33
Std. dev. pre-reform	4.807	26.31	26.01

Notes: 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Marriage registries (1976-2012), all women from cohorts 1961-1971.

Table 5: EFFECT OF THE REFORM ON INFANT HEALTH OUTCOMES

	Infant health					
	Prob. male	Maturity	Multiple birth	Survival 24h	Weight	Weight less 2,500
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.214	0.062	0.017	0.012	2.016	-0.038
	(0.284)	(0.037)	(0.043)	(0.015)	(1.813)	(0.052)
	[0.563]	[0.157]	[0.827]	[0.502]	[0.252]	[0.427]
Treated* Post Reform	-0.484	-0.223***	0.239**	-0.019	-4.694*	0.186**
	(0.613)	(0.049)	(0.079)	(0.015)	(2.035)	(0.060)
	[0.469]	[0.001]	[0.023]	[0.294]	[0.099]	[0.025]
Observations	2,493,107	2,493,107	2,493,107	2,173,324	1,938,272	1,938,272
R^2	0.000	0.013	0.021	0.000	0.011	0.009
BirthYear FE	YES	YES	YES	YES	YES	YES
CalendarYear FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.904	90.43	2.518	99.77	3218	6.466
Std. dev. pre-reform	0.294	29.42	15.67	4.794	506.1	24.59

Notes: The dependent variables are (1) the probability that the first birth is a boy, (2) the probability of having a first child 37 weeks of gestation, (3) the probability of having multiple births, (4) the probability of having a first child that survives the first 24 hours after delivery, (5) the weight at birth of the woman's first child and, (6) 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971. For birth-weight, only consider the birth registries from 1980-2014 and cohorts of women 1962-1971.

Table 6: EFFECT OF THE REFORM ON MARITAL STATUS OF MOTHERS

	Has father (1)	Mother married (2)
Treated	0.271***	0.315**
	(0.043)	(0.121)
	[0.002]	[0.040]
Treated* Post Reform	-0.219***	-0.289**
	(0.040)	(0.111)
	[0.001]	[0.041]
Observations	2,493,107	2,493,107
R^2	0.029	0.044
BirthYear FE	YES	YES
CalendarYear FE	YES	YES
Region FE	YES	YES
Mean pre-reform	96.93	88.86
Std. dev. pre-reform	17.25	31.46

Notes: 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1980-2014), all women from cohorts 1961-1971.

Table 7: EFFECT OF THE REFORM ON LABOR OUTCOMES OF WOMEN

	Work	Low skill job	Part-time job
	(1)	(2)	(3)
Treated	-0.580	1.053**	0.727**
	(0.324)	(0.378)	(0.319)
	[0.112]	[0.020]	[0.046]
Treated*Post Reform	0.508	-1.010*	-1.260***
	(0.402)	(0.473)	(0.368)
	[0.244]	[0.056]	[0.005]
Observations	151,602	92,352	58,349
R^2	0.035	0.015	0.008
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	YES	YES	YES
Mean pre-reform	59.02	25.03	20.28
Std. dev. pre-reform	49.18	43.32	40.21

Notes: The dependent variables are (1) the probability of working at the time of the survey, (2) the probability of having a low skill job and, (3) the probability of having a part-time 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Spanish Labor Force Survey (2000- 2013), all women from cohorts 1958-1974.

Table 8: EFFECT OF THE REFORM ON BEHAVIORAL AND HEALTH OUTCOMES OF WOMEN

	Smoke/day (1)	Smoke regular (2)	Ex-smoker (3)	Pregnancy as motive for being ex-smoker (4)
Treated	-0.038*	-0.044**	0.006	0.128**
	(0.014)	(0.008)	(0.009)	(0.028)
	[0.079]	[0.016]	[0.550]	[0.021]
Treated*Post Reform	0.067**	0.075*	-0.025	-0.189**
	(0.021)	(0.028)	(0.015)	(0.077)
	[0.024]	[0.060]	[0.133]	[0.031]
Observations	4,209	4,209	4,209	959
R-squared	0.014	0.015	0.015	0.065
Mean pre-reform	0.333	0.365	0.239	0.134
Std. dev. pre-reform	0.472	0.481	0.427	0.341

Notes: 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 quitted 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Spanish National Health Survey (2006 and 2012), all women from cohorts 1961-1971.

Table 9: ROBUSTNESS CHECK: EFFECT OF THE REFORM TAKING COHORTS 1964, 1965, 1966 PARTIALLY AFFECTED

Panel 1	Age first birth (1)	Perc. women in each cohort become a mother (2)	Number of children per women in each cohort (3)	Age first marriage (4)	Number of first marriages per woman in each cohort (5)	Number marriages per woman in each cohort (6)
Treated	-0.094***	1.645	3.465	-0.077**	1.001**	0.945*
	(0.002)	(0.354)	(0.553)	(0.011)	(0.540)	(0.539)
	[0.000]	[0.138]	[0.132]	[0.015]	[0.046]	[0.054]
Post Reform	-0.061**	-9.637***	-16.333***	-0.120*	-10.512**	-10.365**
	(0.012)	(4.875)	(8.360)	(0.028)	(5.786)	(5.771)
	[0.037]	[0.001]	[0.001]	[0.057]	[0.024]	[0.024]
Treated* Post Reform	0.068***	-2.006**	-3.696***	0.068**	-1.761**	-1.729**
	(0.006)	(0.392)	(0.609)	(0.018)	(0.550)	(0.543)
	[0.000]	[0.012]	[0.008]	[0.015]	[0.031]	[0.031]
Observations	2,744,675	17,996	18,228	2,559,613	16,575	16,646
R-squared	0.064	0.595	0.621	0.048	0.594	0.597
Panel 2	Prob. male (7)	Maturity (8)	Multiple Births (9)	Survival 24h (10)	Weight (11)	Weight less 2,500 (12)
Treated	0.038	0.080	-0.003	0.034	5.261	-0.123
	(0.026)	(0.046)	(0.066)	(0.010)	(0.402)	(0.012)
	[0.297]	[0.384]	[0.959]	[0.258]	[0.139]	[0.250]
Post Reform	0.711	-0.124	-0.074	0.138*	13.438*	-0.273*
	(0.417)	(0.132)	(0.218)	(0.022)	(1.432)	(0.062)
	[0.201]	[0.489]	[0.790]	[0.099]	[0.097]	[0.081]
Treated* Post Reform	-0.026	-0.222***	0.213*	-0.037*	-7.259**	0.259**
	(0.082)	(0.056)	(0.088)	(0.010)	(1.104)	(0.030)
	[0.764]	[0.008]	[0.058]	[0.055]	[0.026]	[0.027]
Observations	2,744,675	2,744,675	2,744,675	2,416,721	2,154,563	2,154,563
R-squared	0.000	0.013	0.021	0.000	0.011	0.009

Notes: The dependent variables are (1) the age at which women had their first child, (2) the percentage of women in each cohort that had at least one children (multiplied by 1,000), (3) the total number of children per each cohort (multiplied by 1,000), (4) the age of the women they married for the first time, (5) the percentage of (treated and control) women that married at least one time (multiplied by 1,000), (6) the total number of marriages divided by the total number of women born in each cohort (multiplied by 1,000), (7) the probability that the first birth is a boy, (8) the probability of having a first child 37 weeks of gestation, (9) the probability of having multiple births, (10) the probability of having a first child that survives the first 24 hours after delivery, (11) the weight at birth of the woman's first child and, and (12) the probability that the first child is born with less than 2,500 grams. Regressions include cohort, region dummies and (2, 3, 5, 6, 7-12) 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets.* significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014) and Marriage registries (1976-2012), all women from cohorts 1961-1971.

Table 10: ROBUSTNESS CHECK: 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	-0.080**	-0.089***	-0.094***
	(0.009)	(0.005)	(0.002)
	[0.023]	[0.002]	[0.003]
Treated* Post Reform	0.058***	0.067***	0.071***
	(0.011)	(0.008)	(0.006)
	[0.002]	[0.002]	[0.003]
Observations	2,493,107	2,238,017	1,974,964
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	NO	NO	NO
	Perc. women in each cohort become a mother		
Treated	1.319**	1.358*	1.382**
	(0.203)	(0.228)	(0.258)
	[0.037]	[0.052]	[0.035]
Treated* Post Reform	-1.627***	-1.599***	-1.532***
	(0.123)	(0.138)	(0.155)
	[0.003]	[0.005]	[0.003]
Observations	9,982	8,983	7,983
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	YES	YES	YES
	Number of children per women in each cohort		
Treated	3.034***	3.182***	3.225***
	(0.390)	(0.445)	(0.497)
	[0.004]	[0.002]	[0.003]
Treated* Post Reform	-3.079***	-3.118**	-3.009***
	(0.187)	(0.234)	(0.281)
	[0.003]	[0.017]	[0.003]
Observations	10,026	9,023	8,018
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	YES	YES	YES

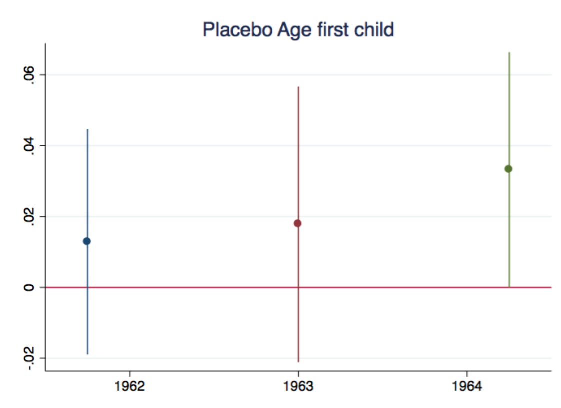
Notes: The dependent variables are (Panel 1) the age at which women had their first child, (Panel 2) the percentage of women in each cohort that had at least one children, and (Panel 3) the total number of children per each cohort. Regressions include cohort, region dummies and (Panel 2 and 3) calendar year. *Treated* are individuals born from January to May, and *control* are those born from July to December. Robust standard errors clustered at cohort level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets.* significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971.

Table 11: ROBUSTNESS CHECK: EFFECT OF THE REFORM ON INFANT HEALTH OUTCOMES EXCLUDING POSSIBLE NONCOMPLIERS

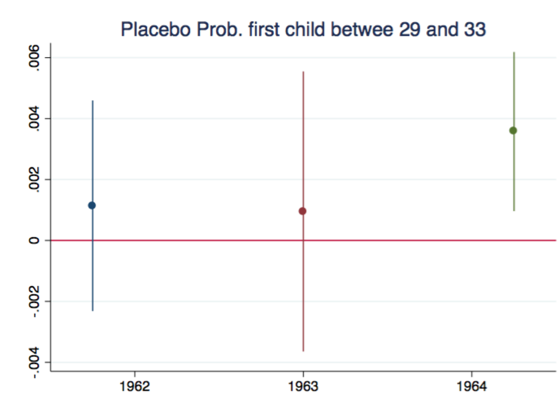
	Drop cohort 1966 (1)	Drop cohorts 1966-65 (2)	Drop cohorts 1966-65-64 (3)
	Infant health: Prob. male		
Treated	0.214	-0.039	-0.240
	(0.284)	(0.201)	(0.117)
	[0.563]	[0.880]	[0.332]
Treated*Post Reform	-0.484	-0.230	-0.027
	(0.613)	(0.580)	(0.557)
	[0.469]	[0.712]	[0.949]
Observations	2,493,107	2,238,017	1,974,964
	Infant health: Maturity		
Treated	0.062	0.083	0.096
	(0.037)	(0.041)	(0.054)
	[0.157]	[0.130]	[0.253]
Treated*Post Reform	-0.223***	-0.239***	-0.246**
	(0.049)	(0.052)	(0.062)
	[0.001]	[0.002]	[0.011]
Observations	2,493,107	2,238,017	1,974,964
	Infant health: Multiple births		
Treated	0.017	0.020	0.011
	(0.043)	(0.054)	(0.072)
	[0.827]	[0.849]	[0.996]
Treated*Post Reform	0.239**	0.234*	0.238
	(0.079)	(0.085)	(0.097)
	[0.023]	[0.060]	[0.121]
Observations	2,493,107	2,238,017	1,974,964
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	YES	YES	YES

Notes: The dependent variables are (Panel 1) the probability of having a first birth boy, (Panel 2) the probability of having a first child with equal or more than 37 weeks of gestation, and (Panel 3) the probability of having a multiple first births. 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets.* significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1971. For birth-weight, only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

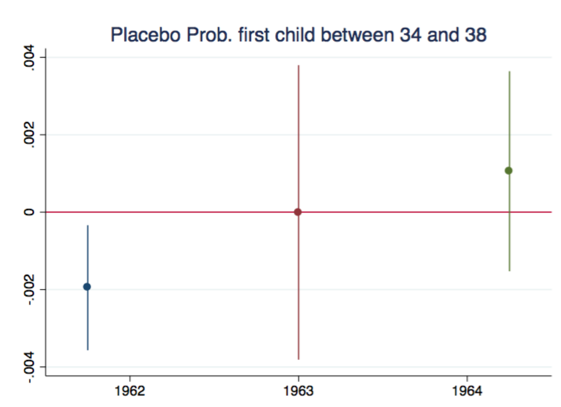
Figure 3: PLACEBOS ON FERTILITY



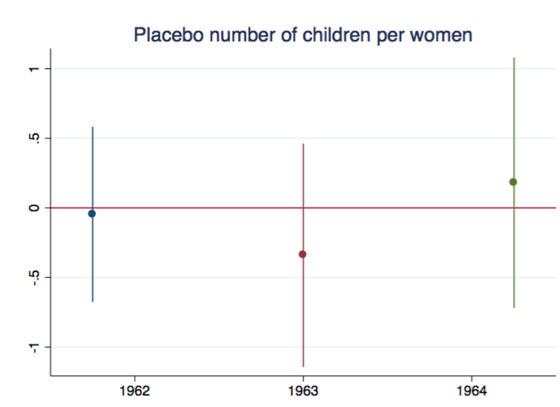
(a) Age at which women had their first birth



(b) Probability of first birth between the age of 29 and 33



(c) Probability of first birth between the age of 34 and 38

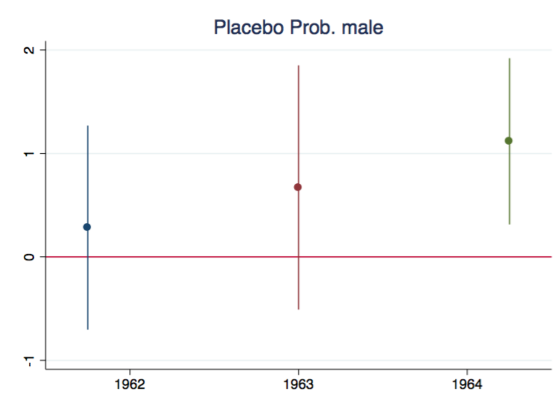


(d) Number of total children

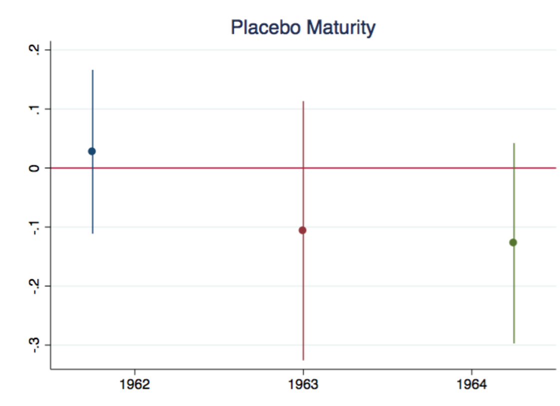
(e) Probability of having children

Notes: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the "fake" reform taking place for the cohorts of 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. Robust standard errors clustered at cohort level. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1965 that had a child for the first time.

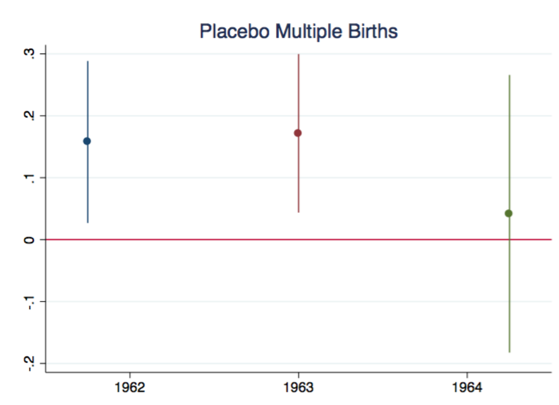
Figure 4: PLACEBOS ON INFANT HEALTH



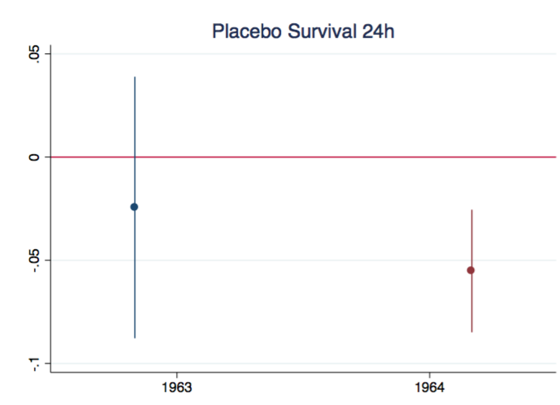
(a) Prob. first birth boy



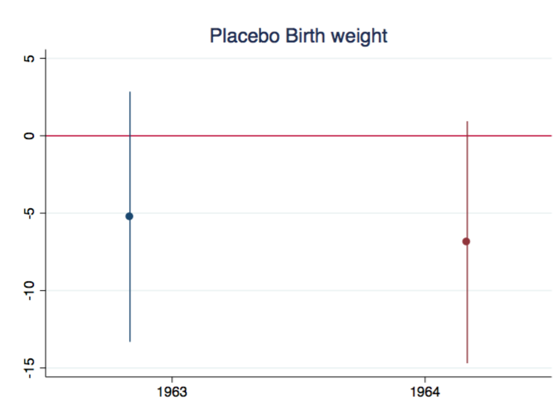
(b) Maturity of first child



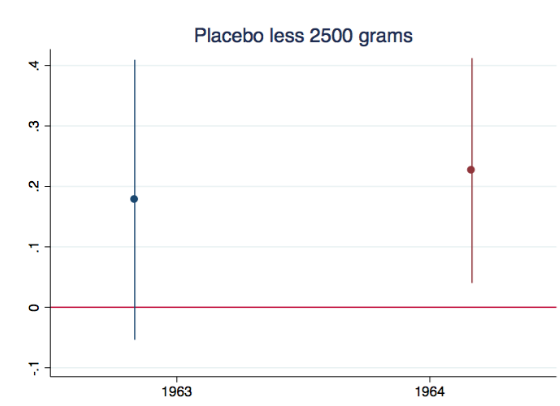
(c) Multiple first births



(d) Survival first 24 hours



(e) Birth-weight of first child



(f) Probability of weighting less 2,500 grams

Notes: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the "fake" reform taking place for the cohorts of 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. Robust standard errors clustered at cohort level. *Source*: Birth registries (1975-2014), all women from cohorts 1961-1965 that had a child for the first time.

Table 12: Effect of the Reform on Fertility and Infant Health Outcomes Controlling for the Father

			Infant Health					
	Age men first birhts (1)	Age women first births (2)	Prob. male (3)	Maturity (4)	Multiple births (5)	Survival 24h (6)	Weight (7)	Weight less 2,500 (8)
Treated	-0.075*	0.067***	0.064	-0.171	-0.042	0.013	2.874	-0.051
	(0.015)	(0.011)	(0.353)	(0.060)	(0.049)	(0.017)	(2.302)	(0.077)
	[0.059]	[0.003]	[0.890]	[0.104]	[0.428]	[0.502]	[0.345]	[0.576]
Treated* Post Reform	0.084**	0.023*	-0.459	-0.170**	0.259**	-0.019	-5.079*	0.161*
	(0.026)	(0.012)	(0.623)	(0.049)	(0.079)	(0.015)	(2.079)	(0.077)
	[0.011]	[0.091]	[0.495]	[0.014]	[0.017]	[0.314]	[0.091]	[0.087]
Aver. age men		0.897***	-1.115	-1.471***	-0.262**	0.012	2.011	-0.177
		(0.022)	(1.112)	(0.342)	(0.115)	(0.032)	(7.252)	(0.277)
Perc. fathers		-15.423***	49.180	33.124*	-10.116	-1.330	392.286	-4.417
		(1.353)	(57.352)	(16.894)	(6.082)	(1.665)	(289.494)	(11.358)
Observations	2,250,019	2,493,107	2,493,107	2,493,107	2,493,107	2,173,324	1,938,272	1,938,272
R^2	0.057	0.068	0.000	0.013	0.021	0.000	0.011	0.007
BirthYear FE	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES
CalendarYear FE	NO	NO	YES	YES	YES	YES	YES	YES
Mean pre-reform	29.70	26.83	289.4	90.43	2.518	99.77	3218	7.425
Std. dev. pre-reform	5.359	5.507	299.8	29.42	15.67	4.794	506.1	26.22

Notes: The dependent variables are (1)the age of the men when they had their first child, (2) the age of the women when they had their first child, (3) the probability of having a first birth male, (4) the probability of having a first child with 37 weeks or more of gestation, (5) the probability of having multiple first births, (6) the probability of having a first child that survives the first 24 hours after delivery, (7) birth-weight at the time of delivery, and (8) the probability that the first child is born with less than 2,500 grams. Regressions (1-8) include cohort, (3-8) calendar time and (1-8) 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all individuals from cohorts 1961-1971. For birthweight and survival 24h, we only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

Table 13: EFFECT OF THE REFORM ON FERTILITY AND INFANT HEALTH OUTCOMES WITH A REDUCED SAMPLE

	Fertility		
	Age first birth	Perc. women in each cohort become a mother	Number of children per women in each cohort
	(1)	(2)	(3)
Treated	-0.086***	0.838***	2.342***
	(0.015)	(0.266)	(0.525)
	[0.001]	[0.001]	[0.001]
Treated* Post Reform	0.068**	-1.253***	-2.706***
	(0.021)	(0.252)	(0.477)
	[0.007]	[0.001]	[0.001]
Observations	1,383,799	9,883	9,939
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	NO	YES	YES
	Infant health		
	Prob. male	Maturity	Multiple birth
	(4)	(5)	(6)
Treated	0.260	0.047	0.031
	(0.451)	(0.038)	(0.069)
	[0.529]	[0.283]	[0.725]
Treated* Post Reform	-0.836	-0.199***	0.197*
	(0.747)	(0.050)	(0.102)
	[0.311]	[0.005]	[0.087]
Observations	1,383,799	1,383,799	1,383,799
	Survival 24h	Weight	Weight less 2500
	(7)	(8)	(9)
Treated	0.018	1.571	-0.033
	(0.025)	(1.437)	(0.082)
	[0.502]	[0.388]	[0.654]
Treated* Post Reform	-0.023	-3.866**	0.176
	(0.027)	(1.524)	(0.101)
	[0.435]	[0.029]	[0.162]
Observations	1,207,828	1,077,377	1,077,377
BirthYear FE	YES	YES	YES
Region FE	YES	YES	YES
CalendarYear FE	YES	YES	YES

Notes: 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), (4) the probability that the first birth is a boy (multiplied by 100), (5) the probability of having a first child 37 weeks of gestation (multiplied by 100), (6) the probability of having multiple births (multiplied by 100), (7) the probability of having a first child that survives the first 24 hours after delivery (multiplied by 100), (8) the weight at birth of the woman's first child, and (9) the probability that the first child is born with less than 2,500 grams (multiplied by 100). Regressions (1-9) include cohort, (2-9) calendar time and (1-9) 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 level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth registries (1975-2014), all individuals from cohorts 1961-1971. For birthweight and survival 24h, we only consider the birth registries from 1980-2011 and cohorts of women 1962-1971.

Appendix

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.[36] 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 A2 shows that the reform decreased the probability of having the first child between the age of 24 and 28 by 2.6 percentage points and the probability of having the first child between the age of 29 and 33 by 0.9 percentage points.[37] However, the reform increased the probability of having the first child between the age of 34 and 38 and between the age of 39 and 43 by 1.3 and 0.1 percentage points respectively.

This postponement of motherhood is accompanied by a decrease in the number of women that become a mother by 0.16 for every 1,000 women and by a decrease of 0.28 children for every 1,000 women. The first two graphs of Figure A3 illustrate these effects. Finally, we also find that the reform has a negative impact on infant health. The probability of having a premature child increases by 0.36 percentage points, and the probability of having a multiple first births rises by 0.16 percentage points, as seen in the last graphs of Figure A3.

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.

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

[37] 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 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.

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 occupation they have.

Therefore, the main variables used are the following and their descriptive statistics can be found in Table A2 :

- **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.

Birth Statistics

This database contains administrative data from birth certificates for the universe of children born in Spain between 1975 and 2014. 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 18,602,664 births. We, then, restrict the sample to births of Spanish women born between 1961 and 1971 that had 14 to 43 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 4,520,086 births or 2,493,107 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 A3:

- **Age women when first child**: Age of the women when they had their first child.
- **First birth between certain ages**: A dummy variable that is equal to one if the woman has her first child between that ages, and zero otherwise.
- **Prob. male**: A dummy variable that is equal to one if the first child of the woman is a male, and zero otherwise (multiplied by 100).
- **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 (multiplied by 100).
- **Survive 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 (multiplied by 100).
- **Birth Weight**: Weight at birth of the woman's first birth (multiplied by 100).
- **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 (multiplied by 100).

- **Number of first births by cohort and treatment**: 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 total births by cohort and treatment**: Total number of births for a cohort of women divided by the total number of women of that cohort multiplied by 1000.

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 A4:

- **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.

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 A5:

- **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.

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.

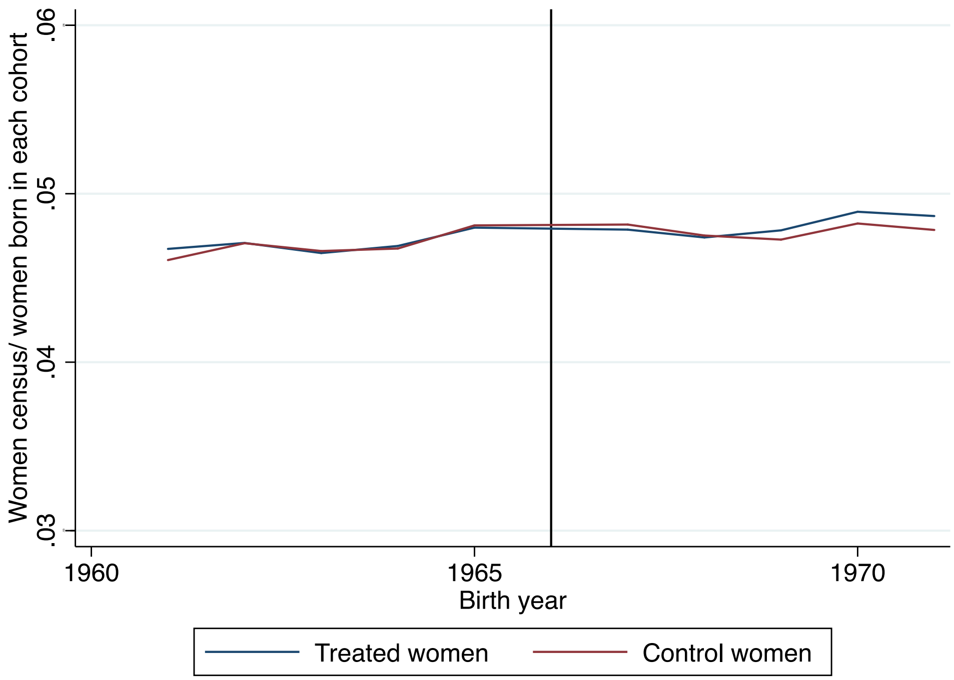
The raw microdata contains 29,478 individuals. We, then, restrict the sample to Spanish women born between 1961 and 1971. We also drop women born in 1966 and who therefore turned 14 the year the reform took place (1980) and those born in July. Thus, finally we observe a total of 3,151 women in our sample.

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 A6:

- **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.

Appendix Tables and Figures

Figure A1: Proportion of Women Represented in the Census by Cohort and Treatment Status



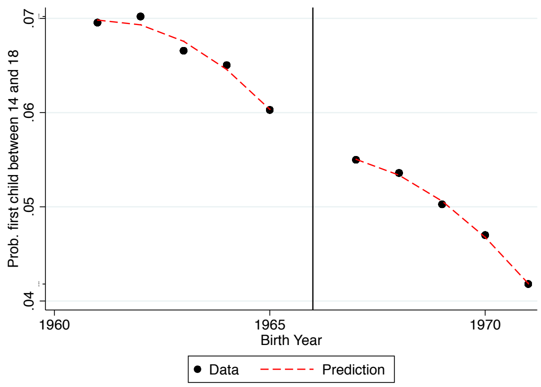
Notes: The ratio is the proportion of treated and control women within each cohort that are represented in the Census of 2001. *Treated* are women born from January to May, and *control* are those born from July to December. *Source*: Census 2001 and Birth Statistics, all women from cohorts 1961-1971.

Table A1: MISSING BIRTH WEIGHT

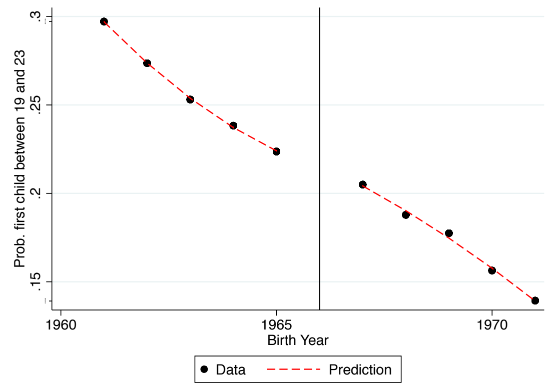
	No birth weight (1)
Treated	-0.000
	(0.000)
	[0.482]
Treated* Post Reform	0.001
	(0.001)
	[0.154]
Observations	2,173,325
R-squared	0.067
BirthYear FE	YES
CalendarYear FE	YES
Region FE	YES

Notes: The dependent variables is the probability that the first birth has not registered birth weight. The regression includes 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 the cohort level in parentheses and the p-value of the wild bootstrap with 1000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Birth Statistics (1980-2014), all women from cohorts 1962-1971.

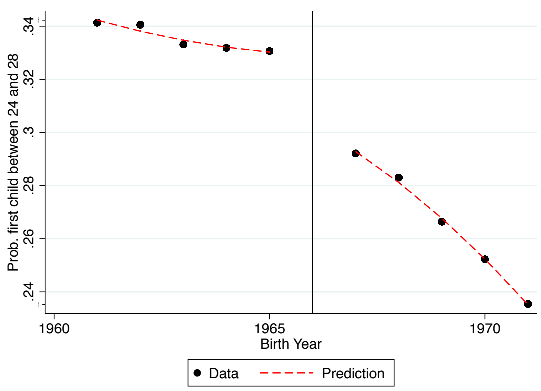
Figure A2: PROBABILITY OF HAVING THE FIRST CHILD AT A CERTAIN AGE BRACKET



(a) Between the age of 14 and 18



(b) Between the age of 19 and 23



(c) Between the age of 24 and 28

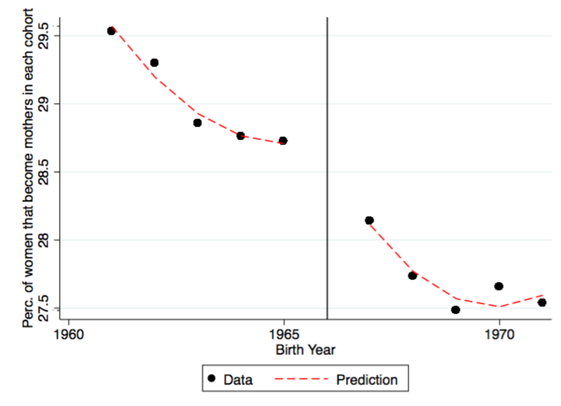
(d) Between the age of 29 and 33

(e) Between the age of 34 and 38

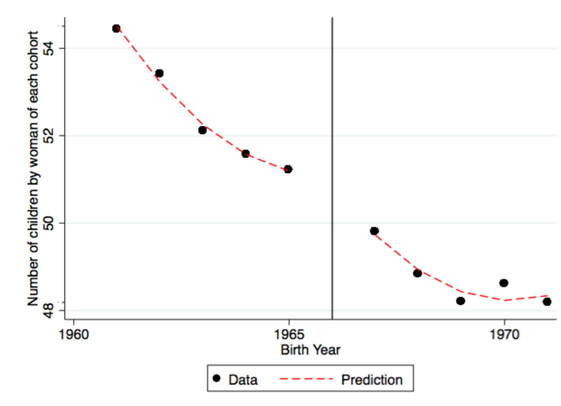
(f) Between the age of 39 and 43

Notes: The predictions are from a regression (with linear and quadratic trends) of the probability of women of having the first child (a) between the age of 14 and 18, (b) between the age of 19 and 23, (c) between the age of 24 and 28, (d) between the age of 29 and 33, (e) between the age of 34 and 38, and (f) between the age of 39 and 43. 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 (1975-2014), all women from cohorts 1961-1971 that had a child for the first time.

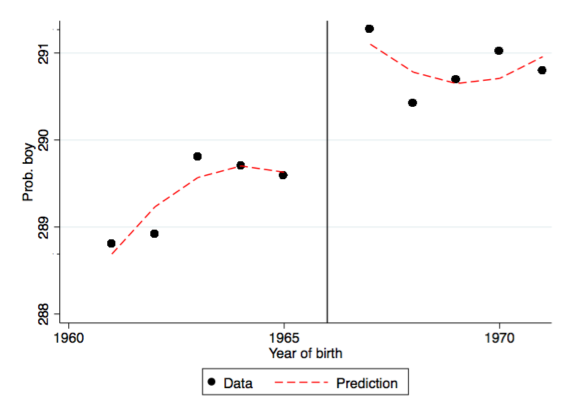
Figure A3: Impact of the Reform on Completed Fertility and Infant Health



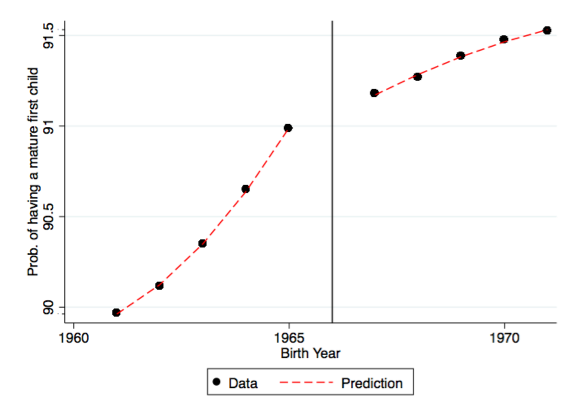
(a) Catching-up



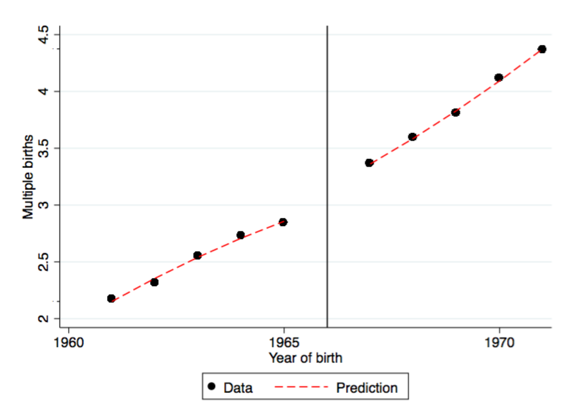
(b) Completed fertility



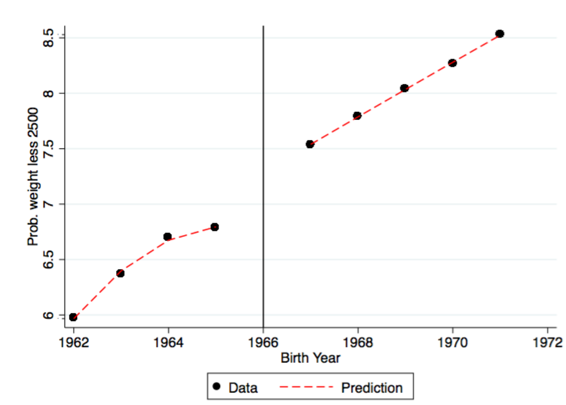
(c) Prob. first birth male



(d) Maturity



(e) Multiple births



(f) Birth weight less 2,500 grams

Notes: 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 birth boy, (d) probability of having a first child with less than 37 gestational weeks, (e) probability of having multiple births, and (f) probability of having a first child that weight less than 2,500 grams. 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 (1975-2014), all women from cohorts 1961-1971 that had a child for the first time.

Table A2: 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: Primary school or more	69924	0.97	0.17	0	1	81678	0.97	0.16	0	1
Women: Secondary school or more	69924	0.86	0.35	0	1	81678	0.87	0.34	0	1
Men: Primary school or more	72261	0.97	0.17	0	1	84282	0.97	0.16	0	1
Men: Secondary school or more	72261	0.86	0.35	0	1	84282	0.87	0.34	0	1
Women: Work	69924	60.67	48.85	0	100	81678	61.13	48.75	0	100
Women: High skill job	42425	25.86	43.79	0	100	49927	25.53	43.61	0	100
Women: Low skill job	42425	22.55	41.79	0	100	49927	21.89	41.35	0	100
Women: Part time job	26796	22.16	41.54	0	100	31553	21.92	41.37	0	100

Source: Spanish Labour Force Survey (2000-2013), for Spanish women and men from cohorts 1958-1974, except the cohort of 1966.

Table A3: 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	1141639	27.74	5.75	14	43	1351468	27.79	5.73	14	43
First birth between 14 to 18	1141639	0.06	0.24	0	1	1351468	0.06	0.24	0	1
First birth between 19 to 23	1141639	0.22	0.41	0	1	1351468	0.21	0.41	0	1
First birth between 24 to 28	1141639	0.30	0.46	0	1	1351468	0.31	0.46	0	1
First birth between 29 and 33	1141639	0.27	0.44	0	1	1351468	0.27	0.44	0	1
First birth between 34 and 38	1141639	0.12	0.32	0	1	1351468	0.12	0.32	0	1
First birth between 39 and 43	1141639	0.03	0.17	0	1	1351468	0.03	0.17	0	1
Prob. male	1141639	29.04	29.83	0	600	1351468	29.16	29.84	0	100
Maturity	1141639	90.82	28.87	0	100	1351468	90.95	28.69	0	100
Survive 24h	1128457	99.80	4.51	0	100	1341126	99.80	4.46	0	100
Birth Weight	991899	3200.20	513.64	500	6500	1185891	3196.75	513.11	500	6400
Weight less 2500	991899	7.12	25.71	0	100	1185891	7.26	25.95	0	100
Number of first births by cohort and treatment	10	114163.90	3378.28	109450	120988.00	10	135146.80	3791.28	131197.00	142065
Number of total births by cohort and treatment	10	207696.50	9677.02	193158.00	222244.00	10	244312.10	9931.51	231494.00	258551

Source: Birth Statistics (1975-2014), all births of Spanish women from cohorts 1961-1971, except the cohort of 1966.

Table A4: 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	1	160290	0.80	0.40	0	1
Number of children by woman	136170	1.53	1.02	0	17	160290	1.52	1.01	0	18
Percentage of woman with more than 3 children	136170	0.12	0.32	0	1	160290	0.11	0.31	0	1

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

Table A5: 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	25.5	4.9	15	41	1260357	25.5	4.9	15	41
Number of first marriages by cohort and treatment	10	106200.4	3325.5	100871	112261	10	126035.7	3951.7	121942	132402
Number of total marriages by cohort and treatment	10	109216.6	3246.6	104271	115196	10	129750.7	3927.3	125034	135854

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

Table A6: 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	1	1706	0.32	0.47	0	1
Smoke regular	1445	0.35	0.48	0	1	1706	0.35	0.48	0	1
Ex-smoker	1445	0.22	0.42	0	1	1706	0.23	0.42	0	1
Pregnancy as motive for being ex-smoker	1445	0.05	0.22	0	1	1706	0.04	0.20	0	1

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