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The Unintended Effects of Increasing the Legal Working Age on Family Behaviour *

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Abstract

We use an exogenous variation in the Spanish legal working age to investigate the effect of education on fertility and infant health. The reform introduced in 1980 raised the minimum legal age to work from 14 to 16 years old. We show that the reform increased educational attainment, which led to 1786 more women remaining childless and 3307 less children being born in the 10 generations after the reform. These negative effects operate through a postponement of first births until an age where the catching up effect cannot take place. We show that woman's marriage market is one channel through which education impacts fertility, delaying the age at which women marry for the first time and reducing the likelihood that a woman marries. Even more importantly, this postponement in fertility seems to be also detrimental for the health of their offspring at the moment of delivery. The reform caused 2,789 more children to be born with less than 37 weeks of gestation, 268 died during the first 24 hours of life and 4,352 were born with low birth weight. We are able to document two channels that contribute to the negative effects on infant health: the postponement in age of delivery as well as a higher employment probability of more educated women, which enhances unhealthier behaviors (smoking and drinking).

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1 Introduction

The decreasing fertility rates which is one of the factors that contributes to the ageing of the population is a major concern in many industrialized countries due to the increased pressure on the sustainability of the social security systems. Some researchers have pointed out the role of education in explaining the reduction in fertility rates. If this is the case, the impact of education on fertility should be considered as a non-pecuniary return of education. Moreover, if parents' education improves the health outcomes of their children, there will be intergenerational education spillovers that should be considered, even if they are not captured by the traditional pecuniary returns of education.

An extensive literature has addressed the study of the relation between education and fertility. A negative correlation between these two variables has been clearly established. However, it is still a debate whether this negative correlation constitutes causality. The main concerns regarding the estimation of the effects of education on fertility rates are the presence of unobserved characteristics that could be affecting schooling choices and the decision to have children, as well as potential reverse causality.

In this paper we investigate the effect of education on fertility and infant health. To deal with the potential endogeneity of education, we exploit a reform that introduced exogenous variation in the Spanish legal working age. This strategy is in stark contrast with recent literature that has used, instead, changes in state compulsory schooling laws as a source of exogenous variation on individual schooling choices. For instance, Black et al. (2008) exploited several reforms in compulsory schooling, both in Norway and the US, and found a significant negative effect on the probability of having a child as a teenager. Other papers also found the same postponement effect of childbearing away from the teenage years in Norway (Monstad et al. (2008)), Italy (Fort (2007)), and the UK (Silles (2011);Geruso et al. (2014)).

Nevertheless, the evidence on the impact of education on completed fertility and the probability of remaining childlessness is contradictory. On one hand, Monstad et al. (2008) used an educational reform in Norway as an instrument and found no effect of education on the probability of women remaining childless or having fewer children. Similarly,Fort (2007), Silles (2011) and Geruso et al. (2014) showed that women catch up with the fertility delay in their early twenties. These papers, then, argue that that education has mainly an "incarceration effect", delaying but not reducing fertility. On the other hand, Cygan-Rehm and Maeder (2013) exploited an exogenous variation from a German compulsory schooling reform and found a reduction in completed fertility. They attributed this lack of catching-up effect to the particularly high opportunity costs of childbearing in Germany. León (2006) also used compulsory schooling laws in the US and showed that education causally reduced completed fertility. Finally, Fort et al. (2011) analysed several compulsory schooling reforms in Europe and concluded that education increased completed fertility as well as reduced the incidence of childlessness. They explain these results by arguing that compulsory schooling reforms target women at the lower end of the educational distribution and these women are also more likely to have grown up in larger and poorer families. Thus, for these women, the income effect might outweigh the substitution effect of education.

Education can, furthermore, affect child quality. Some studies have empirically analysed the causal effect of mothers' education on the health of their offspring at the moment of delivery. If education does indeed affect child health outcomes it will also have an impact on productivity and education for these children. In this case, we should consider that the increase in education could have important intergenerational spillovers. Evidence in industrialized countries, however, is also very inconclusive. Behrman and Rosenzweig (2002) found an increase in child quality through the channel of increasing the household budget constraint (higher earnings and marriage market). Currie and Moretti (2003) also showed a positive impact of mothers' education on children health. However they propose a different channel through which education affects infant health: the change in behaviour of the mothers during pregnancy. More explicitly, they found that the increase in maternal education decreases the incidence of smoking during pregnancy. On the other hand, a number of studies have found no causal effect between education of the mother and health of their children McCrary and Royer (2011).

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

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

The majority of this literature has examined only one of these family behaviour outcomes. In this paper, however, we aim to examine the impact of education over all these three family behaviour outcomes at the same time given the important interactions between them. In order to deal with the possible endogeneity of education on fertility, marriage and infant health, we take advantage of a quasi-natural experiment. In 1980 a labour market reform was introduced in Spain, which increased the minimum legal age to work from 14 to 16 years old. Therefore, we exploit this exogenous variation to estimate a difference-in-difference model.

Most of the previous papers on the topic have used changes in the state compulsory schooling laws as an instrument for years of education. Thus, our approach is different at several levels. First, an educational reform that increases the number of compulsory years of education can usually be accompanied by other changes in the educational system. Thus, this issue makes it difficult to disentangle the effect of a simple increase in the number of years of education from the improvement of the quality of education[1]. Secondly, we exploit the interaction of both the minimum age of compulsory education and the minimum legal age to start working to identify the incentives of different individuals. We argue that both age thresholds affect the decision to remain in the educational system and this is why it is important to consider both of them at the same time. Finally, changes in the minimum legal age to work may represent a more efficient and costless way of increasing educational attainment than increases in the number of years

[1] 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.

of compulsory schooling. Thus, it can constitute a potential policy alternative for a number of developing countries.

As opposed to most of the papers in the 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. This type of data has some advantages over census data, which only allows the identification of all children of each woman if they are still living in the same household at the moment of the interview. There are several situations that can alter this observance such as divorce, death of the mother or the emancipation of some of the older children. If the level of education affects the probability that some of these situations happen, then census data will contain selected data that could bias the results.

We use the effect of the 1980 Spanish labor market reform on education, borrowed from Jiménez-Martín et al. (2015), to test the effect of education on fertility and infant health using a reduced-form analysis. We find that the reform significantly increased the women's probability of remaining childless and reduced their completed fertility, which led to 1786 less women having children and 3307 less children being born in the 10 generations after the reform. These negative effects operate through a postponement of first births until an age where the catching up effect cannot take place. We also show that the marriage market is another channel through which education impacts fertility, delaying the age at which women marry for the first time and reducing the likelihood that a woman marries.

Moreover, we show that the postponement in fertility is also detrimental for the health of their offspring at the moment of delivery. The reform caused 2,789 more children to be born with less than 37 weeks of gestation, 268 died during the first 24 hours of life and 4,352 were born with low birth weight. We propose two different channels through which education can be negatively impacting infant health. The first channel 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. This, in turn, will have negative effects on infant health as the risk during pregnancy increases after that age. In addition, we show that the higher employment probability of more educated women enhances unhealthier behaviours (smoking and drinking), which also contributes to the negative effects that we report on infant health.

The remainder of the paper is organized as follow. We first present the institutional context jointly with the identification strategy in Section 2. In Section 3 we analyse the effect of the reform on educational attainment and in section 4we present the effects of the reform on fertility and infant health. Section 5 concludes with the discussion of the main results and their policy implications.

2 Institutional Context

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

Additionally, not all individuals from the same cohort were impacted in the same way by the reform. Before the reform, those students born during the first months of the year reached the minimum legal age to work (14) before finishing the last year of compulsory education[2]. Therefore, they had an incentive to leave school before completing compulsory education. On the other hand, students born during the last months of the year had incentives to finish compulsory education, as they were not able to work before that. Then, before the reform was passed we would expect those individuals born at the beginning of the year to have a lower probability of finishing compulsory schooling than those individuals born at the end of the year.

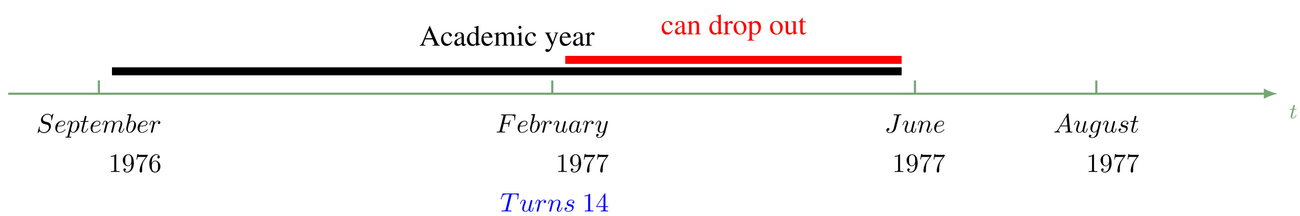
After the reform, this difference in incentives disappears. The reform increased the legal working age to 16 years old while the compulsory schooling age remained at 14 years old. Thus, after the reform, the incentives to drop out from school before finishing primary (compulsory) education for individuals born in the first months of the year disappear and, consequently, all

[2]Note that the Spanish educational system is characterized by the fact that all children from the same cohort start school the same year. Then, children that are born at the beginning of the year start school at an older age (in months) than those children that are born at the end of the year.

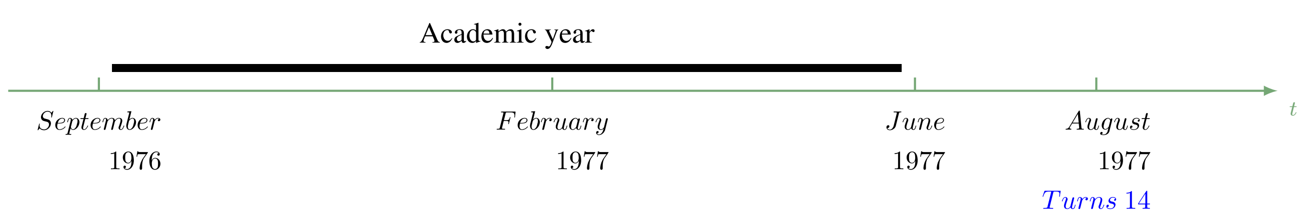
individuals in the same cohort had similar incentives to finish compulsory schooling at age 14, as they could not start working before turning 16 years old.

We can see the timing of the reform in a clearer way in the following chart that represents two individuals of the same cohort of 1963 (before the reform), during their last year of compulsory schooling:

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



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



In this chart, then, we can clearly see that, before the reform that increased the legal age to work was implemented, for two individuals of the same cohort (1963), the incentives to stay in the educational system during the last year of compulsory schooling are different. More specifically, the difference in incentives depends on whether they have been born during the first part of the year (from January to May) or at the last of the year (from August to December).

Thus, we exploit the exogenous change in the incentives to study introduced by the ET reform to identify the causal effect of education on fertility and infant health outcomes. In particular, we exploit the variation of individuals from the same cohort but born at different times of the year, before and after the reform. Thus, we are not exploiting the before-after difference, as the rest of the 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 the one we estimate by using the within cohort comparison and, at the end of the paper, we will provide estimates of this before-after effect. However, in the year that the reform was introduced, 1980, Spain was experiencing a very unsettled period of time. The democratization process in Spain took place in 1979 and during these years very different reforms were held at the same time. For instance divorce was legalized in 1981 and abortion in 1985. Therefore, the cohorts of women born before and after the reform are probably not entirely comparable. Even if we observe a significant change in education attainment and other outcomes after the reform, this change could be due to the influence of other reforms that were taking place at that same moment in time. 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.

3 Effect of the reform on educational attainment

We borrow from Jiménez-Martín et al. (2015) the estimation of the effect of the reform on two educational outcomes. Particularly, we show that the reform affected the incentives of individuals born during the first months of the year to become an early school leaver (leave the educational system before completing primary education) Moreover, we also examine if the reform affected the probability of becoming a dropout (leaving school before completing secondary education).

For these estimations, we use the Spanish Labour Force Survey (LFS) for the years 2000-2013. We cannot use previous waves of the LFS as the information on the month of birth is not reported before the year 2000[3].We also dropped from the sample all individuals that were not born in Spain, as we do not know if they were already in the country at the time of studying primary and secondary education.

The econometric model, then, exploits the fact that before the ET reform of 1980, individuals

[3]In any case, we are only interested in completion rates of primary and secondary education and our youngest cohort (1976) is 26 in 2000. Therefore, we would not be able to use the information of previous years as we need to ensure that individuals are not in primary and/or secondary education at the time of the interview.

from the same cohort had different incentives to become an early school leaver before obtaining the primary education degree. Our hypothesis is that the introduction of the reform would make this difference in incentives disappear and would decrease the probability of being an early school leaver for individuals born at the begin of the year. Thus, a difference in difference approach is used in order to identify the effects of the policy, where we compare individuals born during the first months of the year to individuals born during the last months of the year, for the cohorts that turn 14 years old before and after the reform.

We can enhance the explanatory power of the model if we add time effects to the difference-in-difference equation. The pivotal cohort will be 1966 as this cohort turned 14 in 1980, the year the reform was introduced. We drop the 1966 cohort as we cannot predict the precise effect of the reform for this cohort. Then, we select cohorts born in 1957 to 1965 to be the pre-reform cohorts, and those born between 1967 and 1976, to be the post-reform cohorts. Moreover, we define those individuals born from January to May as the treated group and those individuals born from August to December as the control group. We will also drop those individuals born in June and July, as the effect of the reform is difficult to predict for this middle group (because the academic year in Spain usually finishes at the end of June).

The first educational outcome that we examine is the probability of being an early school leaver, ESL. We classify as ESL all women that are illiterate, have not completed primary school or have been enrolled in labour market integration programs that do not require a primary school degree. The second outcome is the probability of being a dropout, in other words, the probability of not having completed secondary education.

Therefore, for these two outcomes, we apply the following econometric model:

$$\begin{gathered} Outcome_i = \alpha + \beta_1 Treat_i + \beta_2 Postreform_i + \beta_3 Treat_i * Postreform_i \\ + \beta_4 Trend_i + \beta_5 Trend_i^2 + \beta_6 Trend_i^3 + \beta_7 Postref_i * Trend_i + \beta_8 [Postref_i * Trend_i]^2 \\ + \beta_9 [Postref_i * Trend_i]^3 + \beta_{10} RegionFE + \beta_{11} UnemRateEntry_i + \beta_{12} * Gender_i + \epsilon_i \end{gathered}$$

where $Treat_i$ is a dummy variable that equals one if individual i was born between January and May and zero if she was born between August and December. $Postreform_i$ is also a dummy variable that takes value one if individual i was born after the reform and zero otherwise. $Trend_i$ is defined as the difference between the birth year of individual i and the pivotal cohort, 1966. Then, this model includes pre and post reform linear, quadratic and third-order trends, as well as, controls for the regional unemployment rate at age 14. We also include region fixed effects and cluster the standard errors at the region and cohort level. The effect of the reform can be identified by the coefficient of the interaction between the postreform and the treatment dummy variables, β_3.

The estimates reported in Table 1 show the effect of the reform on the probability of being an early school leaver and a dropout for all women (columns 1 and 2). Figure 1 also report the raw data and the predictions from the estimation model for women in the treatment and control groups and for all cohorts 1957-1976.

From Table 1 and Figure 1, we can observe that, before the reform, women born at the beginning of the year had a significant higher probability of being an early school leaver and a dropout. However this difference almost disappeared after the reform is implemented. More precisely, the drop in the probability of being an early school leaver due to the policy implementation corresponds to 0.3 percentage points. This implies that 408 less women of the cohort born in 1976 become early school leavers due to the reform[4]. As the number of ESL in the last pre-reform cohort, 1965, was 3701 for treated women the effect of the reform implies a decrease in ESL by 11
The reform also decreased the probability of being a dropout by 1 percentage points, even if the first stage of secondary education was not compulsory. Only from the cohort born in 1976, 1,361 less women dropped out[5] . As the number of dropouts in 1965 was 66411 for treated women, the reform decreased dropouts by 2

4 Effect of the reform on family behaviour outcomes

As the reform increased the educational attainment of those individuals born during the first months of the year, we use the reform to estimate the effect of education on family behaviour outcomes, using a reduced-form regression. In particular, in this section we analyze the effects

[4]This number increases until 4053 if we take into account the cohorts from 1967 to 1976.
[5]or 13,512 if we consider the cohorts from 1967 to 1976

of the reform on the main family outcomes; fertility and infant health at delivery.

4.1 Effect of the reform on fertility

We study first the impact of the reform on a number of fertility outcomes. In order to test whether women with higher education postpone the entrance into motherhood, as suggested by the literature, we first examine the impact of the reform on the age at which women have their first child. Secondly, we assess whether the increase in educational achievement affected the probability of women to remain childless and the number of children they have. In other words, we want to test if there is a catching-up effect after the delay of motherhood. [6]

For these estimations, we use register data on all birth certificates, from 1975 to 2012, available in the Spanish National Statistics Institute. We select the same pre-reform (1961-1965) and post-reform (1967-1971) cohorts and treatment status. In addition we restrict the sample to all births from women born in Spain and those births that took place when the mother was between the ages of 14 and 41. This age restriction is introduced in order to include the same ages for all the cohorts considered. Therefore, as women of the first cohort we consider, 1961, were 14 in the first year of the register and women of the last cohort, 1971, were 41 in the last year of the register, we restrict to births of women between the ages of 15 and 41.

To examine the effect over the age at which women had their first child, we apply the same econometric model as the one used for educational attainment. For the probability of ending childless and completed fertility, though, we collapse the data by calendar year and cohort for both treatment and control women. 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[7]. Similarly, we define completed fertility as the ratio between the total number of births and the total number of woman born in a certain cohort. For these two outcomes we apply the following econometric model:

[6] We are aware that probability the results of the reform on fertility will be the interaction of the effects of the reform on both men and women, as normally fertility decisions are mutual. However, it is outside the scope of the paper to try to disentangle the difference in those effects.

[7] The results are multiplied by 1000.

$$Outcome_c = \alpha + \beta_1 Treat + \beta_2 Postreform_c + \beta_3 Treat * Postreform_c$$
$$+\beta_4 CohortYearFE + \beta_5 CalendarYearFE + \epsilon_i$$

In this specification, we are substituting the pre and post reform trends by cohort dummies and we are controlling for the year at which the birth took place. Standard errors are clustered at the cohort level. Again, the effect of the reform on treated women will be identified by β_3.

The estimates in Table 2 show the effect of the reform on the age of women when they had their first child. We can observe that, before the reform, women born at the beginning of the year had their first child almost a month before with respect to women born at the end of the year. This gap in age disappears once the reform is introduced.

Table 3 shows that the postponement effect is followed by an increase in the probability of remaining childless as well as a decrease in completed fertility[8]. Thus, the reform implied that 180 more women born in 1967[9] decided to remain childless. This implies a decrease of 0.15% in the number of women that decide to have children due to the reform[10].

Moreover, before the reform, 2.2 more children were born per thousand women born at the beginning of the year with respect to women born at the end of the year. This gap is eliminated by the introduction of the reform. This means that 333 less children were born from the cohort of women of 1967[11]. This corresponds to a decrease in 0.15% in the total number of children born, given that women born at the beginning of 1965 had in total 209954 children.

As a robustness check, we investigate these two last outcomes (the probability of remaining childless and completed fertility) using data from the 2011 census. The census of 2011 includes a representative sample of 5% of the population and gives us information about the number of children that women had until 2011[12]. We can observe from Table 4, that the effect of the

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

[9] or 1,786 born between 1967 and 1976

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

[11] or 2,789 children in the subsequent 10 generations affected by the reform

[12] Note that we are considering the same cohorts of women (1961- 1971) and the definition of treatment is also

reform on the probability of having at least one child and completed fertility goes in the same direction as the results found using birth registries. However, the results are not significant. We believe that there are two main reasons that can explain the lack of significance in the coefficients estimated with the 2011 census. First, we are only observing 5% of the population, so the results can be estimated with more noise, and, thus, the standards errors are higher. Second, as the Census does not include information on the year at which women had their children, we cannot control for calendar year effects.

On the other hand, in the birth registries we do not have information on the number of children that each specific woman has. This information is only reported in the census. Therefore, we also examine the effect of the reform on the composition of families. We use the Census information on the total number of children per women. We can see from the last regression of Table 4 that, before the reform, women born at the beginning of the year had a higher probability of having a "large family", defined as having 3 or more children. This gap is, though, reduced by the introduction of the reform.

Therefore, we can conclude that the reform had three main effects on fertility; first, it made some women postpone the entrance into motherhood and this delay was not caught up afterwards[13]. Second, it increased the number of women that remained childless and, third, it decreased the probability of having a large family.

4.1.1 Mechanisms

In this section we try to shed some light on the potential channels that may be preventing the catching-up effect from taking place. The main hypothesis is that education delays entrance into motherhood for a large number of years after which the catching-up effect can no longer take place. To check the validity of this hypothesis we estimate some age-specific probabilities of having the first birth. More precisely, we use the same econometric model as before but now

the same. Furthermore, in 2011, the last cohort we are considering, 1971, had 41 years old. This is the same age constraint than the one that we we had when using the birth registries.

[13]We also evaluated this result using the "Encuesta de fecundidad" of 2006 available from "Centro de Investigaciones Sociológicas". This is a questionnaire that is given to 10.000 women that are more than 15 years old in 2006. Here, we also have information on the total number of children that women from cohorts 1964 to 1968 had in 2006. The number of observations, however, is very small (around 600 women) so that although results go in the same direction, they are not significant.

the different outcomes are: the probability of having the first birth before turning 18 years old, between 18 and 21 years old, between 25 and 30 years and after 35 years old.[14]

Table 5 reveals no significant differences between treatment and control, before and after the reform on the probability of having the first child during the teenage years. Therefore and, in stark contrast from the findings of the previous literature, we find that the reform did not postpone the incidence of first birth away from the teenage years. Thus, we can rule out the "incarceration effect"[15] as the main channel through which this reform affected fertility.

On the other hand, the reform did affect the probability of having the first child between the age of 18 and 21 as well as with more than 35 years old. Before the reform, women born at the beginning of the year had a higher probability of having their first child between the ages of 18 and 21 years and a lower probability of having it after the age of 35, when compared with women born at the end of the year. This gap was reduced due to the introduction of the reform. This effect of the reform can clearly be seen in Figure 2. Therefore, we show that the reform decreased the probability of pregnancy during the early twenties while, at the same time, increased the probability of having late first births.

Thus, even if the postponement of one month on average seemed a small effect, the increase in the incidence of first births after the age of 35 is not. As it is well-know from the medical literature, after the age of 35 women fertility decreases and the catch-up may no longer be possible for some of these 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. Thus, as an additional potential factor that may help us explain the effects of the reform on fertility, we analyze whether the reform had any impact on marriage outcomes. First, we study whether increased education induced women to postpone the age at which they marry for the first time. Next, we examine if this postponement reduces the number of first and total marriages in a permanent way.

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

[15] We define incarceration effect as a delay in fertility for the amount of time that women stay longer at school.

For this analysis, we use register data on all marriage certificates from 1976 to 2012. As before, we select the cohorts of 1961 to 1965 to be the pre-reform cohorts, and the cohorts of 1967 to 1971 as the post-reform cohorts. We restrict the sample to all marriages that took place when the woman was aged between 15 and 41. The definition of treatment and control is the same as before (treated women are born from January to May and control women are born from August to December). Finally, we drop same-sex marriages due to their late acceptance in the definition of marriage.

For the first outcome, the age at which woman in our sample had their first marriage, we use the same econometric model applied for the analysis of age at first birth. For the analysis of the impact of the reform on the number of total marriages, we collapse the data at the cohort and calendar year level for treatment and control and divide them by the total number of women born in a certain cohort. Similarly, to calculate the probability of ending single, we divide the total number of first marriages by the total number of women born at a certain cohort. We use the same econometric model as the one used when analyzing total fertility.

Table 6 shows the postponement of marriage due to the reform. 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 eliminated by the introduction of the reform.

Table 7 points out 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. We can observe that, after the reform, more than 1 in every thousand women born at the beginning of the year decided not to get married. Moreover, we observe almost the same reduction in the total number of marriages[16].

Summing up, we can conclude that the reform did postpone first marriages and consequently the time at which women decided to have children. Moreover, we find that the postponement in fertility is not away from the teenage years, as the majority of previous literature has found, as our results show that the reform did not affect the probability of women having the first child

[16] However, we should take these two results with caution again 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 the age of 41.

before the age of 18. We find that the reform increased the incidence of first births after the age of 35, age at which women start being less fertile, resulting in a reduction in completed fertility rates [17].

4.2 Effect of the reform on infant health at delivery

In this section we focus on the potential long-term impacts of the reform. More precisely, we study whether the reform affected the health of children born to women affected by the reform. We measure children's health at the moment of delivery. This represents a very important issue because if we find evidence that the increase in parental education has an impact on the health of their offspring, we can argue that education has intergenerational externalities that should be taken into account when calculating the returns to education.

In this analysis, we use the same register data as the one used for fertility. The measures of babies' health that we evaluate are: birth-weight (in grams), the fraction of babies that are born with less than 2,500 grams, the fraction that are born with more than 37 gestational weeks and the fraction that die within the first 24 hours of life. For birth-weight the data is only available from 1980 to 2012. Thus, when analysing this outcome, 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 age of 18 and 41.[18]

Note that we are restricting the analysis to the study of infant health of the woman's first child. The reason for this restriction is that a poor health of the first birth can influence the decision of having a second child, as pointed out by Wolpin (1997).

Table 8 reports the effects of the reform on the above-mentioned infant health outcomes using the same econometric model as before. We can observe from these tables that the reform has a negative impact on the health of the children of women born at the beginning of the year. After the reform, the first children of a woman born at the beginning of the year have a 0.213 percentage point higher probability of being premature (born with less than 37 gestational weeks). This

[17] Fertility rates decreases for women with age, specially after the age of 35. For instance, Leridon (2004) show that the probability of having a conception after 1 year of trying decreases from 75% at age 30 to 66% at age 35.

[18] Note that we already showed in Table 5 that the reform did not have an effect on the probability of women having the first child before age 18

translates into 290 more children of women born in 1967 that are premature due to the reform [19]. Given that the number of children that are born premature in each cohort of women is not that large[20], having 290 more children prematures translates into an increase of 2.7%.

The reform also has a significant impact on the probability of the first children dying during the first 24 hours. Women born at the beginning of the year have a 0.023 percentage point higher probability of having a first child that dies within the first 24 hours. This means that women born in 1967 have 28 more children that died during the first 24 hours due to the reform [21]. As only 312 of the children born from women of the cohort of 1965 died during the first 24 hours, 28 more children implies a 7% increase in the number of children that did not survive the first 24 hours due to the reform.

Finally, the reform caused women born at the beginning of the year to have children that weighted 4.4 grams less, on average, compared to children of women born at the end of the year. Figure 3presents graphical evidence of the effect of the reform on children's weight. 4.4 grams does not seem a very high magnitude but we should take into account that this is the estimated average impact of the reform. In fact, when we estimate the impact of the reform on the probability of having a first child with low birth weight (2,500 grams), we can observe that, after the reform, women born at the beginning of the year have a 0.38 percentage points higher probability of having a first child with low weight. In absolute numbers, this implies that 453 more children are born with low weight from the cohort of women born in 1967 [22]. As the percentage of children that are born with weight lower than 2,500 is not that large[23], 453 more children implies an increase of 6% in the number of children with low weight due to the reform. We believe that these numbers constitute an important impact of the reform as the negative effects of low birth weight on long-run outcomes, such as labour 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).

[19] 2,789 if we take into account the 10 consequent cohorts.

[20] Note that only 10499 children were born premature from those women born at the beginning of the year of 1965.

[21] If we scale this results to the 10 generations of women affected by the reform we estimate that 269 more children died in the first 24 hours.

[22] 4,352 in the subsequent ten generations of women.

[23] Only 7474 children from the cohort of women born in 1965 were born with less than 2,500 grams of weight

In order to reconcile our findings with the conflicting previous evidence in the literature, which mainly finds a positive impact of mother's education on child's health. For that, we propose two different channels through which the reform could have a negative impact on infant health.

4.2.1 Mechanisms

A first channel through which the reform operates is the combination of the age effect and the education effect. On the one hand, we have shown that the reform postpones the entrance into motherhood. We label this effect as the ”age effect”. This postponement increases the probability that women have their first child after the age of 35. This, in turn, will have negative effects on infant health as the risk during pregnancy increases after that age. On the other hand, we have also shown that the reform increased the educational attainment of those women born during the first months of the year. We label this effect as the ”education effect”. We can hypothesize, following the previous literature, that education improves infant health through an increase in household's income and/or an enhancement of healthier behaviours during pregnancy. These two effects go in opposite directions. Therefore, the fact that the reform has a negative effect on infant health, could be driven by a bigger ”age effect” as compared to the ”education effect”.

In order to analyze the “importance” of this first channel in explaining the reported negative effects on infant health, we perform a Heckman selection model for weeks of gestation and birth-weight. In this model, the outcome equation is the same than the one we used to determine the impact of the reform on maturity, weight or survival of the first 24 hours. The selection equation is an age-specific regression for the probability of having the first child at each age bracket. We can then interpret this model as the effect of the reform on children's health, conditional on having the first child at a certain age. If the ”age effect” is the main channel through which the reform is affecting infant health, we expect to find no effects or positive effects of the reform in the outcome regression for this Heckman selection model. Precisely, Table 9 and Table 10 show that, once we control for the age at which women have their first child, the reform has a significant positive effect on weeks of gestation as well as on birth-weight of the woman's child. Thus, once we control for the age at which women have their children, the reform has a positive effect on infant health as a result of the increase in educational attainment. Now, this coincides with the findings reported in the rest of the literature [24].

[24] We also investigate if there is selection in the children that are actually born. It could be the case that before

A second channel through which the reform could be affecting infant health is the labour market. If the reform increased, not only the educational attainment of women, but also the probability of working, it would be more likely that these women work during pregnancy, which could be affecting the health of their children at the time of delivery. Moreover, the income effect of working could be translated into a higher probability of these women engaging in more healthy or unhealthy behaviours such as alcohol consumption or smoking that could ultimate affect their children's health. In fact, previous literature has demonstrated the association between increased educations and the prevalence of unhealthy behaviors (specially smoking) among Spanish women in a clear process of convergence towards men attitudes (see Pampel (2003), and Schiaffino A and JM (2002), for the Spanish case).

Thus, in order to analyse the impact of the reform on labour market outcomes, we use the "Muestra Continua de Vidas Laborales". This database contains administrative data from the Spanish Social Security and includes employment, unemployment and contributory benefits spells of 4% of all the individuals that had a relationship with the Social Security at some point during 2006-2012. We construct information on employment and wage for each quarter from 2007 to 2012. We collapse the individual level data by quarter, cohort and treatment status.

We are interested on the effects of the reform on short-run and long-run labour market outcomes. We consider two main outcomes: the probability of working in each quarter and the first time in which the individual entered the labour market.

We use the same econometric model as before but with cohort fixed effects instead of trends. The results can be seen in the first two columns of Table 11. We can observe that, before the reform, women born at the beginning of the year entered the labour marked 178 days before

the reform, those women with less education incurred in more unhealthy behaviors during pregnancy which could lead to more foetus deaths. Then, the children that we observe from the women that were born at the beginning of the year, would be precisely those that come from the "better" mothers.

In order to check this alternative channel, we used register data on late fetal deaths, which reports all natural abortions that took place when the foetus has at least 6 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 foetus death of more than 6 months of gestation. However, medical research indicates that the greatest risk of suffering a natural abortion is observed during the first 3 months of gestation. Therefore, we cannot completely rule out the selection hypothesis with these results. We can only conclude that there does not exist selection in those cases where the foetus survives until the sixth month of gestation.

women born at the end of the year (on average). This gap is reduced once the reform is introduced. This is consistent with the first results on the effects of the reform on educational attainment as, before the reform, treated women have a higher probability of being both an early school leaver as well as a dropout. Thus, if those women are no longer in the educational system, (at least some of them) are probably entering the labour market at an earlier date. This short-run effect of the reform in the labour market can potentially also have an impact on some of the family formation decisions that we have reported in this paper.

However, this administrative data does not show any significant impact of the reform on long-term labour market outcomes, although the sign points to the right direction. That can be due to the fact that we only have information from 2007 to 2012, which is a very special period in the Spanish economy due to the strong economic crisis experienced. Thus, maybe the lack of significance in our estimates of the probability of working in the long-term is due to the fact that during these years unemployment increased in an unprecedented way in Spain [25] . In order to check if this lack of significance is driven by the effects of the economic crisis, we also analyse the same long-term effect using the Labour Force Suvey. In the LFSwe have labour market information from 2000 to 2013.

In the third and fourth columns of Table 11 we can see that, in fact, with the LFS, the reform shows a positive and significant impact on the probability of working. Before the reform, women born at the beginning of the year have a lower probability of being employed at the time of the survey compared to women born at the end of the year. This difference is reduced once the reform is introduced. We also observe that, before the reform, treated women have a higher probability of being inactive compared with women born at the end of the year.

Thus, we conclude that, before the reform, less educated women (born at the begin of the year) entered the labour market at younger ages than those women with higher education (born at the end of the year). In the long run, however, these women born at the beginning of the year have a lower probability of being employed than women born at the end of the year. This gap is reduced after the reform is introduced due to the increase in the educational level of women born at the begin of the year.

[25] Note that the unemployment rate increased from 8.23 in 2007 to 24.79 in 2012

Finally, in order to analyse if the income effect of having started working at a younger age affected the probability of engaging in healthier of unhealthier behaviours, we use data from the Spanish National Health Survey. Although this survey is available for several years only the wave of 2006 reports the month of birth of the individuals, which is a crucial variable for our identification strategy. Therefore, the results that we report are for the 2006 wave. The cohorts that we include are those from 1956 to 1976.

In Table 12 we can observe that, after the reform, women born at the beginning of the year drink more alcohol per day. They also have a higher probability of smoking regularly and smoke a higher quantity of cigarettes a day. More importantly, it seems that, after the reform, those women born at the beginning of the year have a lower probability of quitting smoking during pregnancy. These outcomes will have direct effects on the health of their offsprings. Moreover, we can see that these changes in behaviour have evident consequences on the women's health as, after the reform, treated women have a higher probability of suffering high blood pressure and bronchitis.

Thus, we have presented some evidence that the reform have positive impacts on women, as they have a higher probability of working and higher educational attainment. However, the impact of the reform on their children is negative. The higher probabilities of working also increased their probability of enhancing in unhealthy behaviours that resulted in a more dangerous pregnancy and worst health outcomes for their first child at the moment of delivery. As we have shown, part of this negative health outcomes on children are also attributable to the postponement of fertility of more educated women after the age of 35.

4.3 Robustness checks

4.3.1 Placebos

In this section, we perform several placebo test where we use ”fake” reform years taking only those cohorts of women not affected by the “real” reform (the reform in 1980).Thus, we will examine the effect of three ”fake” reforms affecting the cohorts of 1961, 1962 and 1963 [26]. As

[26]We cannot do the placebo for the cohorts of 1964 and 1965 because they might be partially influenced by the reform. These two cohorts were between 15 and 16 years old when the reform was introduced. Thus, if they

before, we define those individuals born from January to May as the treated group and those individuals born from August to December as the control group. We also drop those individuals born in June and July, as we did for our previous analysis. We use the same econometric specification as before. We expect a non-significant effect of the interaction term between the post reform dummy and the treatment dummy.

In Figure 7 and 8 we plotted the estimates of the interaction term and the 95%confidence interval for the different fertility and infant health outcomes. In graph a) of Figure 7 we can observe that none of the ”fake” reforms considered have a significant effect on the age at which women decide to have their first child. However, for the probability of having the first child between 18 and 22 years old, it seems that there is a change in the trend differences between the treatment and control groups for the cohorts of 1963 and 1964. Still, we find no effect of the reform for any of the cohorts for the probability of having a first child with more than 35 years old.. Moreover, in graphs d) and e) of Figure 7, we again observe that there is no effect of any of the ”fake” reforms on the probability of having a child or in the total number of children that each woman decides has.

We perform the same analysis for all the outcomes of infant health. We see from graphs a), b) and d) that the ”fake” reforms for cohorts 1962, 1963 and 1964 have no effect on the probability of having a first child with more than 37 weeks of gestation, the probability of surviving during the first 24 hours or the probability of weighting more than 2,500 grams at birth. Nevertheless, the results on birth-weight are less clear as it seems to be a change in the trend difference between the treatment and control groups for the cohort of 1964.

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

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were not working at that moment, the reform would have prevented them from starting working. Moreover, these cohorts could also still be in the last year of compulsory schooling if they had to retake a year at school.

4.3.2 Pre-Post analysis of the reform

As explained before, our identification strategy has focused on comparing individuals in the same cohort before and after the implementation of the policy. Thus, we have are not relied only on the before-after difference given that the reform took place in a very unsettle period of time in Spain[27]. Given that fact, it is reasonable to suspect that women born before and after the reform are not entirely comparable. We are following, instead, a much more conservative strategy comparing women within the same cohorts and only exploiting a difference of months of education. 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..

We can observe in Figure 6, that the reform decreased the probability of having the first child before the age of 18 by 0.19 percentage points as well as the probability of having the first child between the age of 25 and 30 by 1.2 percentage points [28]. However, the probability of having a first child after the age of 35 increased by 0.13 percentage points. This means that only for the cohort of women born in 1966, 433 women delayed their first birth until the age of 35, and this number raises to 4,392 if we consider the cohorts of 1966-1976.

This postponement of birth is also accompanied by a decrease in the percentage of women that decided to become a mother by 1.24 for every thousand women and by a decrease in 1.418 children for every thousand women. We can see these effect in the first two graphs of Figure 6. Thus, we estimate that 412 less women born in 1966 had children (469 less children were born). This estimate increases to 4,735 less mothers and 5,380 less children born as a result of the reform for the subsequent ten cohorts of women.

Finally, we also find that there is a negative impact of the reform on infant health. The probability of having a premature child increased by 0.386 percentage points as well as the probability of having a child with low birth weight (less than 2,500 grams) raises by 0.39 percentage points, as we can observe in the last two graph of Figure 6. This means that around 1,090 children born from women of the 1966 cohort and 11,573 children born from women of the 1966-1976 co-

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

[28] In this estimation we select cohorts of 1961 to 1965 to be the pre-reform cohorts, and cohorts 1966 to 1971, the post-reform cohorts. We do not drop the cohort of 1966 in this analysis, as this cohort of 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.

horts were premature and had low birth weight.

5 Discussion

This work investigates the effect of education on fertility and infant health at the time of delivery. We exploit a reform implemented in Spain in 1980 that increased the minimum legal working age from 14 to 16 years old. Before the reform, students born at the beginning or the end of the year had different incentives for finishing compulsory schooling than individuals born at the end of the year. The introduction of the reform abolished these different incentives between individuals of the same cohort born at different times of the year. Thus, we exploit the variation within cohort, following a difference-in-difference approach where we compare individuals born at the beginning and end of the year, before and after the reform.

We find that, though education, the reform prompted a postponement of the first births by almost a month, on average. This number is very similar to the results in the majority of previous literature. However, our results show that this postponement is not followed by a catching-up effect as the reform increased woman's probability of ending her fertile lifecycle without any children, and reduced her completed fertility. As a matter of fact, the reform made 180 women born in 1967 and 1,786 from the cohorts of 1967 to 1976 decide not to become a mother. In turn, this resulted in 333 less children born from the 1967 cohort and 3,307 from the 1967-1976 cohorts.

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 where the catching up effect does not take place. In fact, we show that the reform decreased the probability of pregnancy during the early twenties while, at the same time, increased the probability of having late first births (after the age of 35). The marriage market is a second channel that proofs to contribute to the postponement of first births. We find that the reform increased the age at which woman 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. In fact, 152 less women born in 1967 got married or 1,510 if we consider women from the cohorts 1967 to 1976.

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 with less than 37 gestational weeks by 0.213 percentage points. This implies that women born in 1967 had 290 more children with less than 37 weeks of gestation. The amount of premature children increased by 2,789 in the subsequent 10 generations of women. Moreover, these mothers also had a higher probability of having low weight babies after the reform and the babies had a lower probability of survival to the first 24 hours. This last effect implies that 269 more children died during the first 24 hours from mothers born between 1967 and 1976.

We propose two different channels through which this detrimental effect of education on children's health are taking place. The first channel we explore is the effect of age on pregnancy. When controlling for the age at which women had their first child, the reform has a positive, rather than negative, effect on infant health,. This is consistent with the positive impact of education on babies' health found in previous literature. Thus, as more educated mothers have their first child at an older age, their pregnancy is more risky and they have children with worse health indicators. This age effect dominates over the positive effect of education on babies' health. The second channel that we propose for which education has a negative effect on children's health is through changes in labour market prospects and unhealthy habits. More precisely, we find that the reform increased employment of more educated women and, as a consequence, increased their unhealthy habits, as smoking and drinking alcohol. Thus, the fact that, after the reform, more educated women had better labour market outcomes has a negative impact on pregnancy through the enhancement of unhealthy behaviours. More precisely, we find that the probability of quitting smoking during pregnancy is reduced for women born at the beginning of the year after the reform.

Therefore, we conclude that, even though the reform had positive impacts on women, as they accumulate more education and have a higher probability of working, the impact of the reform on their children is negative. This effect is driven both by the increase in women's age at the moment of delivery and by increasing their unhealthy habits. Thus, as women's education has an impact on the health of their offsprings, we can argue that education has intergenerational externalities that should be taken into account when calculating the returns to (increased) education.

6 Tables and Figures

6.1 Educational Outcomes

Table 1: Effect of the reform on the probability of being an early school leaver and a dropout for women

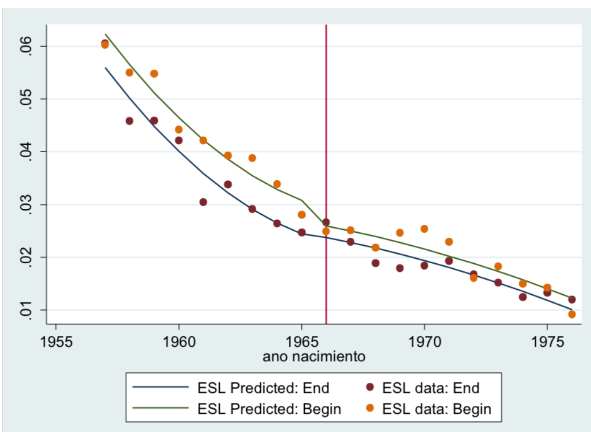
	ELS women (1)	Dropout women (2)
Treated	0.006***	0.012***
	(0.002)	(0.004)
Post Reform	0.005	0.007
	(0.009)	(0.018)
Treated*Post Reform	-0.003*	-0.010*
	(0.002)	(0.005)
Trend	0.001 -0.008	
	(0.006)	(0.011)
Post Reform* Trend	0.024***	0.007
	(0.008)	(0.015)
Trend^2	-0.000	0.002
	(0.001)	(0.002)
Post Reform*Trend^2	-0.005***	-0.006*
	(0.002)	(0.004)
Constant	0.165***	0.469***
	(0.016)	(0.030)
Observations	209,462	209,462
R-squared	0.027	0.074
BirthYear FE	NO	NO
CalendarYear FE	YES	YES
Region FE	YES	YES

Note: The dependent variables are: the probability that (1) a woman is an early school leaver or (2) a dropout. We include as early school leaver all women that are illiterate, have not completed primary school or have been enrolled in labour market integration programs that do not require a primary school degree. We define as dropout all women that have not completed secondary education. Regressions include pre and post reform linear and quadratic trends, calendar time and region dummies. Treated are individuals born from January to May and control are those born from July to December. The results are robust in sign and significance to the inclusion of quartic pre and post reform trends or the substitution of trends by cohort time dummies. Robust standard errors clustered at cohort and region level in parentheses.

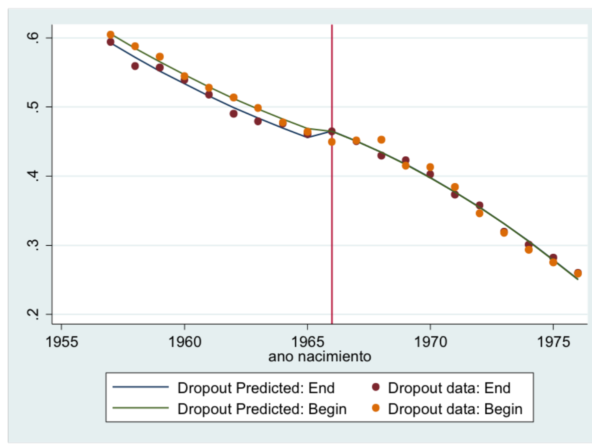
Source: Spanish Labour Force Survey, EPA, (2000- 2013), all individuals from cohorts 1957-1975

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

Figure 1: Probability of being an early school leaver and a dropout for women



(a) ELS



(b) Dropout

Note: The predictions are from a regression of the probability of (treated and non-treated) women of being (a) an early school leaver and (b) a dropout . Treated are individuals born from January to May and control are those born from July to December.

Source: Spanish Labour Force Survey, EPA, (2000- 2013), all individuals from cohorts 1957-1975

6.2 Fertility

Table 2: Effect of the reform on the age at which women had their first birth

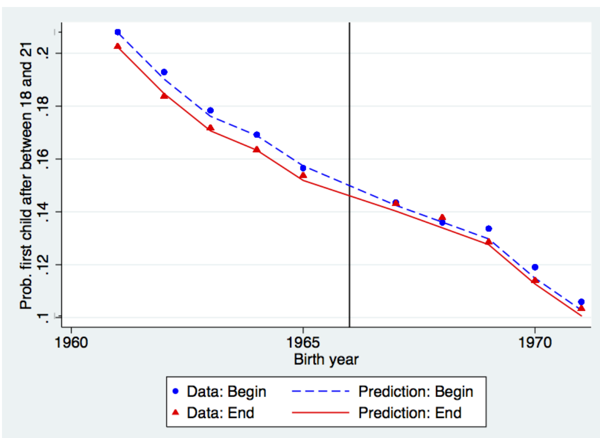
	Age women	
	(1)	(2)
Treated	-0.0823***	-0.0822***
	(0.0105)	(0.0105)
Post Reform	0.0719	3.226***
	(0.227)	(0.0847)
Treated*Post Reform	0.0553***	0.0551***
	(0.0158))	(0.0158)
Trend	0.181	
	(0.216)	
Post Reform*Trend	0.0304	
	(0.318)	
Trend^2	-0.0410	
	(0.0889)	
Post Reform*Trend^2	0.0802	
	(0.125)	
Trend^3	-0.00432	
	(0.0107)	
Post Reform*Trend^3	0.00218	
	(0.0145)	
Constant	26.47***	25.08***
	(0.153)	(0.0939)
Observations	2,469,113	2,469,113
R-squared	0.067	0.067
BirthYear FE	NO	YES
CalendarYear FE	NO	NO
Region FE	YES	YES

Note: The dependent variable is the age of the women at the moment they had their first child. Regressions include region fixed effects and (1) pre and post reform linear, quadratic and quartic trends and (2) cohort time dummies. Treated are individuals born from January to May and control are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

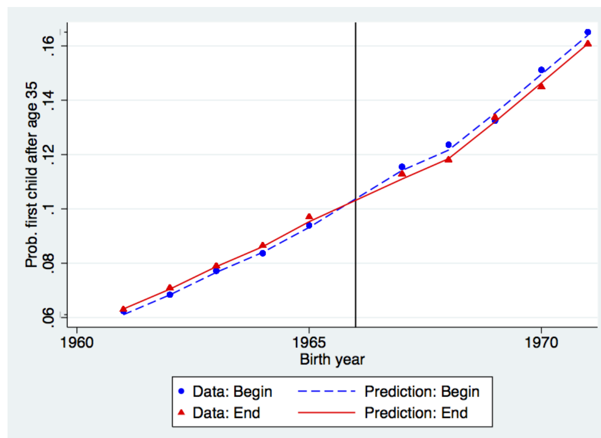
Source: Birth registries (1975-2012), all women from cohorts 1961-1971.

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

Figure 2: Probability of having a first child at a certain age bracket



(a) Between the age of 18 and 21



(b) After the age of 35

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

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

Table 3: Effect of the reform on the probability of remaining childless and the number of total children

	Perc. women in each cohort become a mother	Number of children per women in each cohort
Treated	0.955***	2.478***
	(0.224)	(0.410)
Post Reform	-5.085	-11.36
	(5.743)	(10.00)
Treated*Post Reform	-1.322***	-2.448***
	(0.136)	(0.193)
Constant	-0.392*	-0.185
	(0.185)	(0.323)
Observations	560	540
R-squared	0.227	0.245
BirthYear FE	YES	YES
CalendarYear FE	YES	YES
Region FE	NO	NO

Note: The dependent variables are: (1) the percentage of (treated and control) women that had at least one child and (2) the total number of children divided by the total number of women born in each cohort (multiplied by 1000). Regressions include cohort time, 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.

Source: Birth registries (1975-2012), all women from cohorts 1961-1971, and the total number of women born in each cohort from birth registries. The data from the birth registries is collapsed by cohort year and calendar year for treated and control women.

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

Table 4: Effect of the reform on the probability of remaining childless, number of total children and probability of having at least 3 children

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

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

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

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

Table 5: Effect of the reform on the probability of having the first birth at a certain age bracket

	First birth before 18	First birth between 18 to 21	First birth between 25 and 30	First birth more than 35
Treated	-0.000379	0.005435***	-0.00477***	-0.00204***
	(0.000334)	(0.000741)	(0.000919)	(0.000467)
Post Reform	-0.0157***	-0.1010***	-0.0428***	0.0979***
	(0.00163)	(0.00167)	(0.010464)	(0.00453)
Treated*Post Reform	1.96e-05	-0.00279***	-2.84e-05	0.00494***
	(0.000517)	(0.001046)	(0.00124)	(0.000825)
Constant	0.0553***	0.2394***	0.320***	0.0346***
	(0.00143)	(0.03021)	(0.00520)	(0.00333)
Observations	2,469,113	2,469,113	2,469,113	2,469,113
R-squared	0.006	0.024	0.007	0.017
BirthYear FE	YES	YES	YES	YES
CalendarYear FE	NO	NO	NO	NO
Region FE	YES	YES	YES	YES

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

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

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

6.2.1 Marriage

Table 6: Effect of the reform on the age at which women marry for the first time

	Age women	
	(1)	(2)
Treated	-0.0564***	-0.0563***
	(0.0104)	(0.0104)
Post Reform	0.0902	2.613***
	(0.170)	(0.0631)
Treated*Post Reform	0.0469***	0.0468***
	(0.0153)	(0.0153)
Trend	0.0806	
	(0.171)	
Post Reform*Trend	0.0314	
	(0.242)	
Trend^2	-0.0492	
	(0.0692)	
Post Reform*Trend^2	0.104	
	(0.0965)	
Trend^3	-0.00416	
	(0.00819)	
Post Reform*Trend^3	2.88e-05	
	(0.0113)	
Constant	24.81***	23.70***
	(0.123)	(0.0570)
Observations	2,322,360	2,322,360
R-squared	0.050	0.050
BirthYear FE	NO	YES
CohortYear FE	NO	NO
Region FE	YES	YES

Note: The dependent variable is the age of the women at the moment they married for the first time. Regressions include (1) pre and post reform linear, quadratic and quartic trends and (2) cohort time dummies. Treated are individuals born from January to May and control are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

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

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

Table 7: Effect of the reform on the probability of remaining single and the number of total marriages

	Number of first marriages per woman in each cohort	Number marriages per woman in each cohort
Treated	0.322	0.257
	(0.263)	(0.255)
Post Reform	3.622	4.798
	(7.647)	(7.437)
Treated*Post Reform	-1.118***	-1.075***
	(0.236)	(0.231)
Constant	2.972***	3.021***
	(0.199)	(0.193)
Observations	540	540
R-squared	0.363	0.380
BirthYear FE	YES	YES
CalendarYear FE	YES	YES
Region FE	NO	NO

Note: The dependent variables are: (1) the percentage of (treated and control) women that married at least one time and (2) the total number of marriages divided by the total number of women born in each cohort (multiplied by 1000). 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.

Source: Marriage registries (1976-2012), all women from cohorts 1961-1971, and the total number of women born in each cohort from birth registries. The data from the marriage registries is collapsed by cohort year and calendar year for treated and control women.

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

6.3 Infant Health

Table 8: Effect of the reform on some infant health outcomes

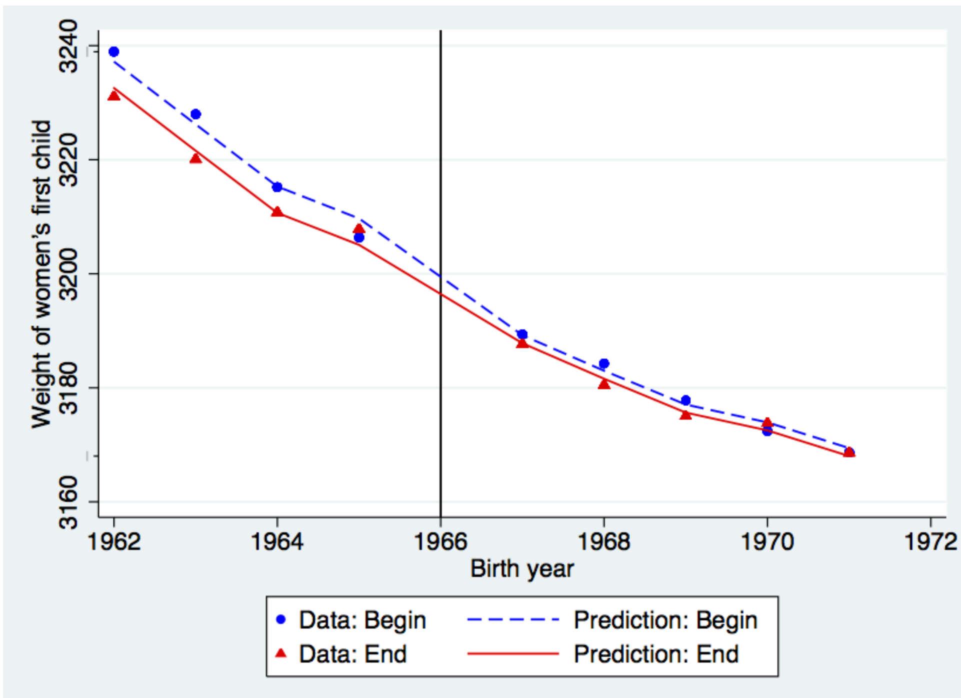
	Infant health			
	Maturity	Survival 24h	Weight	Weight less 2,500
	(1)	(2)	(3)	(4)
Treated	0.000654	0.000158	2.007	-0.000404
	(0.000631)	(0.000106)	(1.348)	(0.000611)
Post Reformf	0.00405	6.62e-05	-10.27**	-0.00382**
	(0.00684)	(0.000180)	(4.629)	(0.00159)
Treated*Post Reform	-0.00213***	-0.000236*	-4.446**	0.00173**
	(0.000771)	(0.000126)	(1.752)	(0.000871)
Constant	0.923***	0.999***	3,304***	0.0505***
	(0.0234)	(0.000407)	(10.12)	(0.00530)
Observations	2,469,113	2,445,589	1,916,854	1,916,854
R-squared	0.013	0.001	0.010	0.008
BirthYear FE	YES	YES	YES	YES
CalendarYear FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES

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

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

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

Figure 3: Birth weight of the first child



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

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

6.3.1 Channel 1

Table 9: Heckman selection model with birth-weight

	Weight if less 21 (1)	Weight if between 25/30 (2)	Weight if more 35 (3)
Treated	-16.67**	-3.698	-25.65**
	(7.190)	(6.458)	(10.80)
Post Reform	326.4***	-64.51	372.7*
	(121.1)	(91.63)	(200.7)
Treated*Post Reform	8.215*	-4.460	35.96*
	(4.944)	(3.954)	(19.52)
Lambda	-1,387***	563.2	1,321
	(384.1)	(839.9)	(829.5)
Constant	5,089***	2,631***	305.0
	(492.9)	(898.3)	(1,682)
Observations	256,577	625,773	233,546
R-squared	0.007	0.005	0.003
BirthYear FE	YES	YES	YES
CalendarYear FE	YES	YES	YES
Region FE	YES	YES	YES

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

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

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

Table 10: Heckman selection model with gestation weeks

	Weeks if less 21	Weeks if between 25/30	Weeks if more 35
	(1)	(2)	(3)
Treated	-0.195**	-0.120**	-0.0668*
	(0.0904)	(0.0531)	(0.0378)
Post Reform	4.079**	-1.689**	0.832
	(1.787)	(0.844)	(0.604)
Treated*Post Reform	0.0794**	-0.0721**	0.0443
	(0.0390)	(0.0318)	(0.0656)
Lambda	-13.64**	16.47**	2.768
	(5.536)	(7.674)	(2.802)
Constant	54.98***	21.34**	33.42***
	(7.134)	(8.202)	(5.710)
Observations	185,976	543,411	213,665
R-squared	0.022	0.005	0.003
BirthYear FE	YES	YES	YES
CalendarYear FE	YES	YES	YES
Region FE	YES	YES	YES

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

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

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

6.3.2 Channel 2

Table 11: Effect of the reform on the long and short run labour outcomes of women using the MCLV and EPA

	Work (MCLV) (1)	Labour market entry (MCLV) (2)	Work (EPA) (3)	Inactivity (EPA) (4)
Treated	-0.269	-178.610***	-1.036***	0.976**
	(0.205)	(20.072)	(0.303)	(0.400)
Post Reform	1.688*	-355.196*	4.779***	-7.806***
	(0.932)	(183.435)	(0.226)	(0.221)
Treated*Post Reform	0.075	72.297***	0.991**	-0.674
	(0.282)	(22.957)	(0.447)	(0.456)
Constant	76.822***	8,327.970***	62.617***	31.978***
	(1.115)	(196.226)	(1.161)	(1.175)
Observations	19,040	19,040	2,242	2,242
R-squared	0.778	0.761	0.423	0.603
BirthYear FE	YES	YES	NO	NO
CalendarTrimester FE	YES	YES	YES	YES
Region FE	YES	YES	NO	NO

Note: The dependent variables are: the probability of working at the time of the survey (1) using the MCVL or (4) the EPA (both multiplied by 100) , (2) the age (in days) at which they entered into the labour market using the MCVL and (3) the age at which the women were incorporated to the labour market. Regressions include cohort, quarter 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 (EPA) and at cohort and region level (MCVL) in parentheses.

Source: Muestra Continua de Vidas Laborales (MCVL) (2007-2012) and the Spanish Labour Force Survey (EPA) (2000- 2013), all women from cohorts 1957-1975.

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

Table 12: Some behavioural and health outcomes of women

	High blood preassure (1)	Bronchitis (2)	Smoke/day (3)	Smoke regular (4)	Alcohol/day (5)	Pregnancy as motive for being ex-smoker (6)
Treated	-0.00985	-0.0175**	-0.0313*	-0.0317*	-0.0178	0.0583*
	(0.0118)	(0.00676)	(0.0164)	(0.0164)	(0.0133)	(0.0334)
Post Reform	-0.143***	-0.0438**	0.00565	0.0218	-0.133***	-0.147
	(0.0206)	(0.0179)	(0.0464)	(0.0436)	(0.0245)	(0.235)
Treated*Post Reform	0.0238*	0.0294***	0.0576**	0.0511**	0.0287*	-1.077*
	(0.0140)	(0.00889)	(0.0239)	(0.0238)	(0.0168)	(0.607)
Lambda						10.69
						(6.817)
Constant	0.138***	0.0613***	0.340***	0.364***	0.200***	-16.16
	(0.0220)	(0.0186)	(0.0436)	(0.0396)	(0.0203)	(10.35)
Observations	5,468	5,468	5,468	5,468	5,468	1,192
R-squared	0.034	0.016	0.015	0.014	0.036	0.054
BirthYear FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES

Note: The dependent variables are: (1) the probability of having high blood pressure the last 12 months, (2) the probability of suffering bronchitis, (3) probability of smoking at least one cigarette a day (4) probability of smoking regularly, (5) probability of drinking at least one alcoholic drinks per day, (6) the probability of having quitted smoking during pregnancy, conditional on being an ex-smoker (Heckman selection model). The regression include cohort and region dummies. Treated are individuals born from January to May and control are those born from July to December. Robust standard errors clustered at cohort and region level in parentheses.

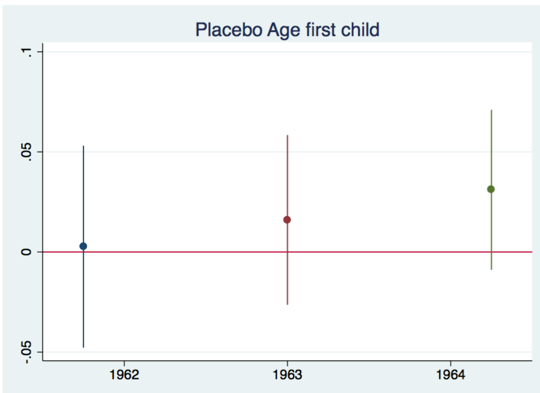
Source: Encuesta Nacional de Salud 2006, all women from cohorts 1956-1976.

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

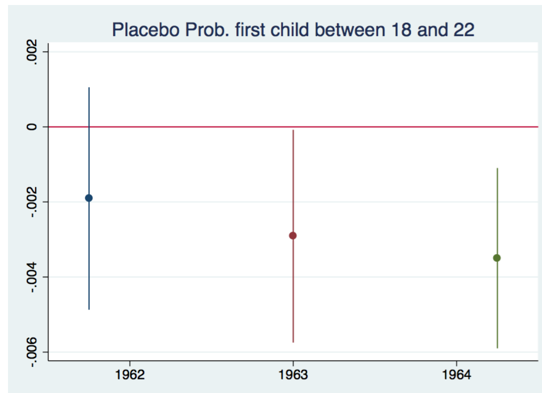
6.4 Robustness checks

6.4.1 Placebos

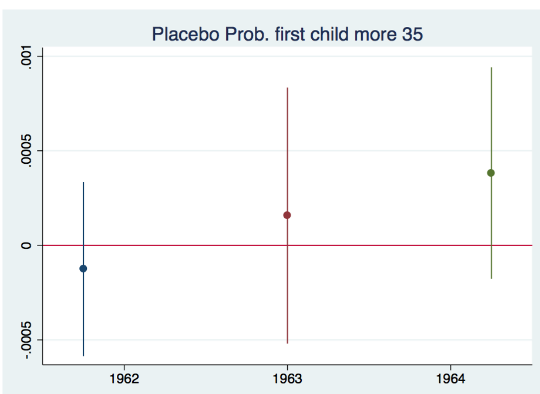
Figure 4: Placebos on Fertility



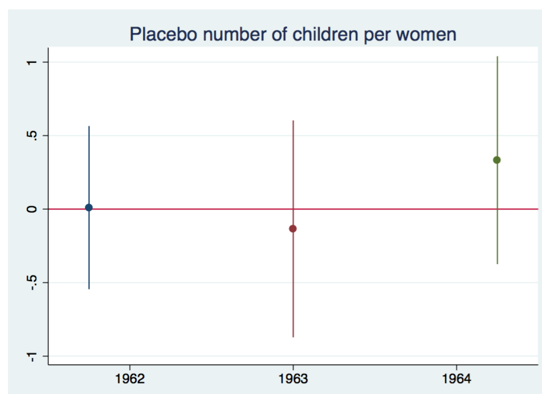
(a) Age at which women had their first birth



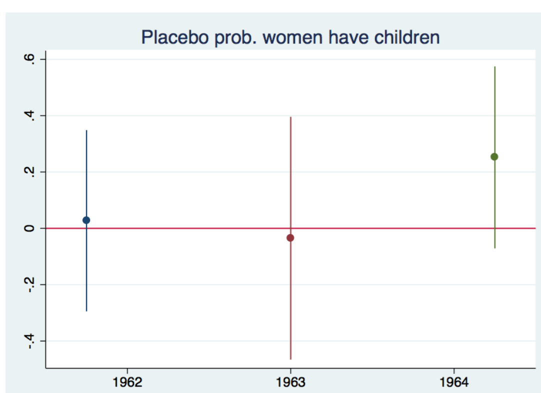
(b) Probability of first birth between the age of 18 and 21



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



(d) Number of total children

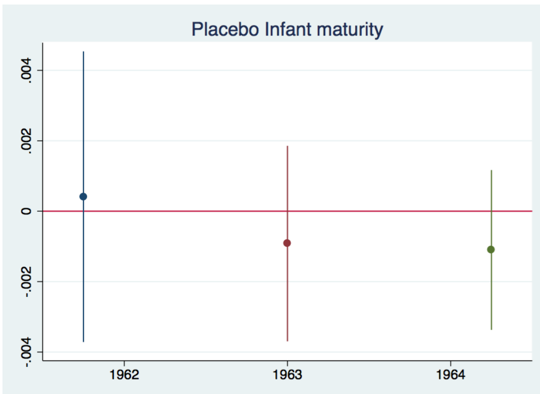


(e) Probability of having children

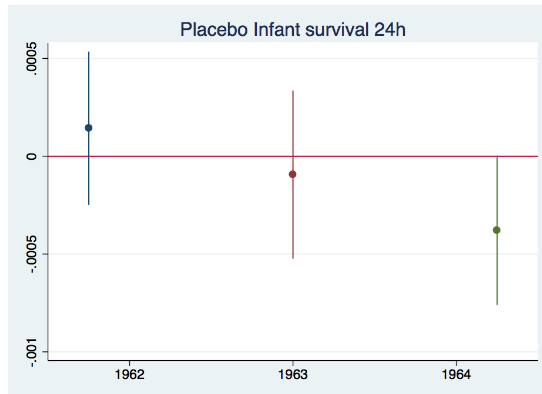
Note: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the "fake" reform taking place in the cohorts of 1962, 1963 and 1964. We only consider cohorts not affected by the real reform: 1961-1965. The treatment is defined as those women born from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

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

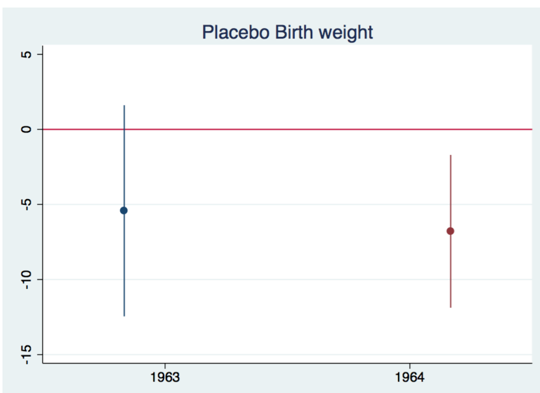
Figure 6: Placebos on Infant Health



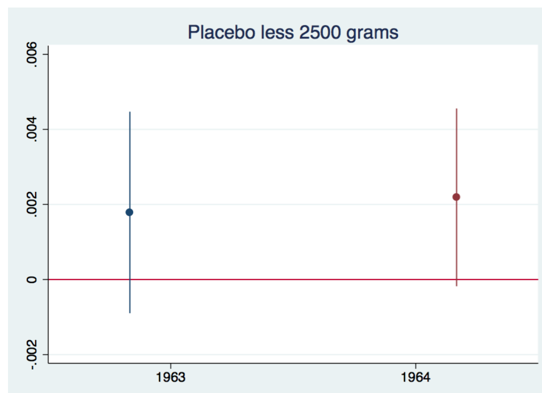
(a) Maturity of first child



(b) Survival first 24 hours



(c) Birth-weight of first child



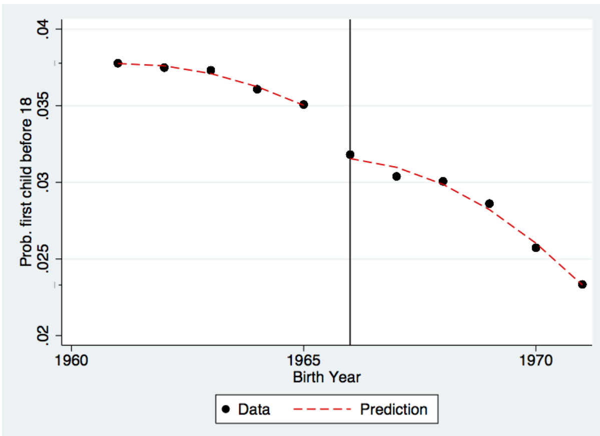
(d) Probability of weighting less 2,500 grams

Note: We report the point estimates and the 95% confidence interval of the interaction term of the treatment and the ”fake” reform taking place in 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 August to December.

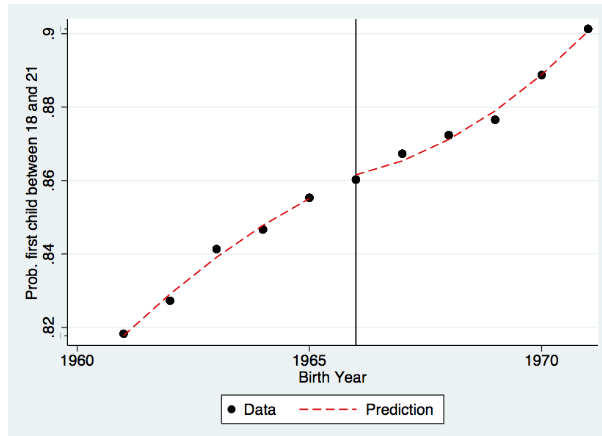
Source: Birth registries (1975-2012), all women from cohorts 1961-1965 that had a child for the first time.

6.4.2 Pre-Post analysis of the reform

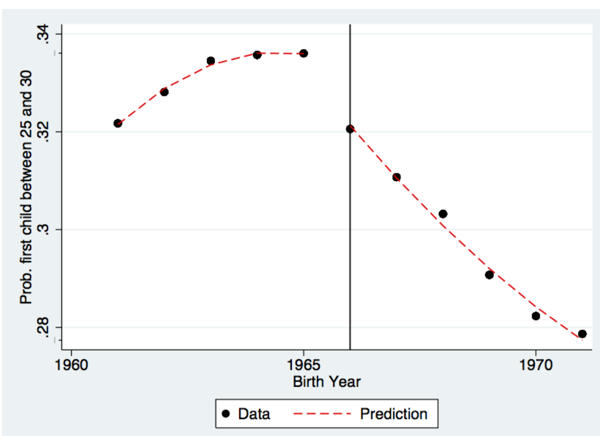
Figure 7: Probability of having the first child at a certain age bracket



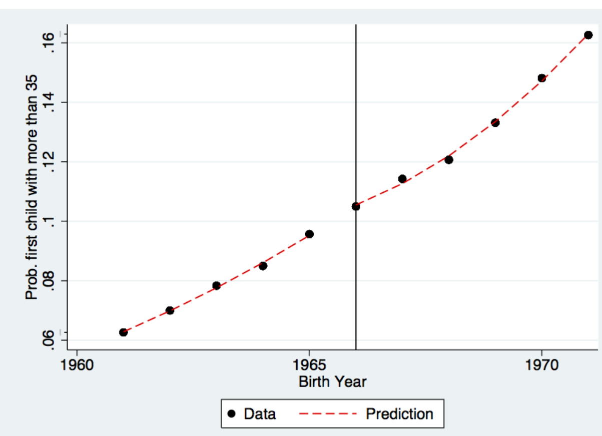
(a) Before the age of 18



(b) Between the age of 18 and 21



(c) Between the age of 25 and 30

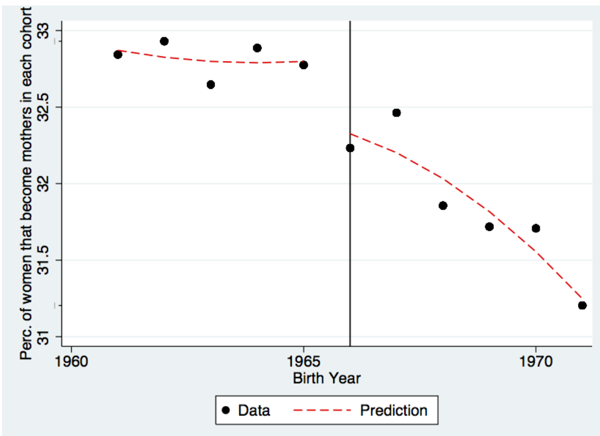


(d) After the age of 35

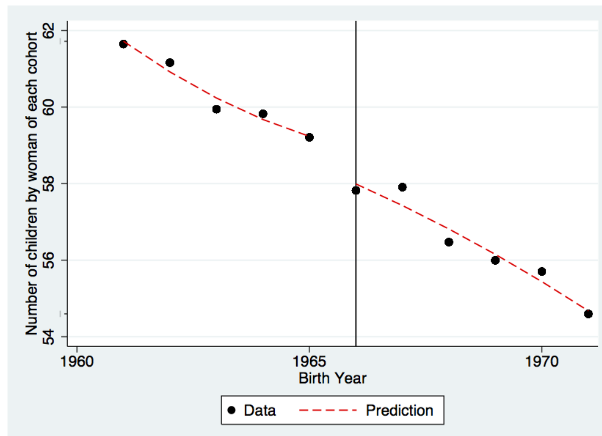
Note: The predictions are from a regression (with linear and quadratic trends) of the probability of women of having the first child (a) before the age of 18 (b) between the age of 18 and 21, (c) between the age of 25 and 30 and (d) after the age of 35. We consider the cohorts from 1961 to 1965 to be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

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

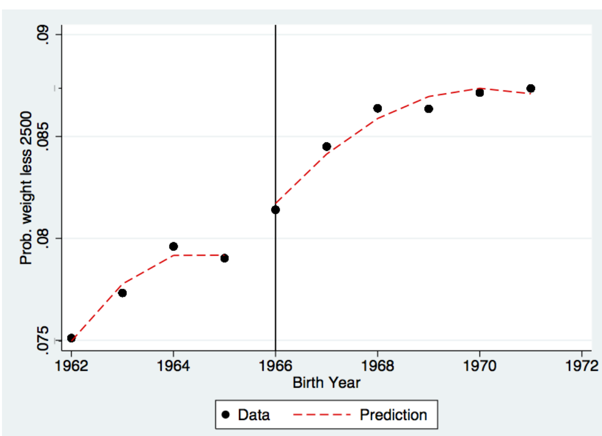
Figure 8: Impact of the reform on completed fertility and infant health



(a) Catching-up



(b) Completed fertility



(c) Low Birth Weight

(d) Maturity

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

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

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