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Cristina Bellés-Obrero
Sergi Jiménez-Martín
Judit Vall Castelló

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Cristina Bellés-Obrero[1,4], Sergi Jiménez-Martín[2,5,6] and Judit Vall Castello[3,4]

[1]Department of Economics, University of Mannheim,

[2]Department of Economics, Universitat Pompeu Fabra,

[3]Department of Economics, Universitat de Barcelona & IEB,

[4]Center for Research in Economics and Health, Universitat Pompeu Fabra,

[5]Barcelona GSE

[5]FEDEA, Madrid

Abstract

We examine the gender asymmetries in mortality generated by a Spanish reform raising the legal working age from 14 to 16 in 1980. While the reform, though its effects on education, decreased mortality at ages 14-29 among men (6.3%) and women (8.9%), it increased mortality for prime-age women (30-45) by 6.3%. This last effect is driven by increases in HIV mortality, as well as by diseases of the nervous and circulatory system. All in all, these patterns help explain the narrowing age gap in life expectancy between women and men in Spain.

JEL Codes: I12, I20, J10

Keywords: minimum working age, education, mortality, gender

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1 Introduction

Women generally have a lower mortality rate in each age group and a higher overall life expectancy than men. Although this gender gap was first observed in developed countries, it is now a universal phenomenon. However, the size of the gender gap has not remained constant over time. In OECD countries, for example, the gender gap in life expectancy widened between 1950 and 1970, but subsequently narrowed. While in 1975 women were expected to live 6.2 years longer than men, 30 years later the difference in life expectancy had fallen to 5.2 years.[1] Figure 1 shows the evolution of female and male mortality rates in Spain since the early 90s through to the present day. Whereas in 1991 the difference in mortality rates between men and women aged 30-45 was 40 deaths per thousand individuals, in 2016 this difference was only 20 per thousand individuals.

Gender differences in health behaviors could explain the bulk of the gender gap in life expectancy (Sundberg et al., 2018; Luy and Wegner-Siegmundt, 2014). Originally, men had a higher mortality risk due to smoking, alcohol consumption, substance abuse, and occupational risks (Loef and Walach, 2012). However, changes in gender patterns of smoking and other unhealthy risk factors could partially explain the narrowing of the gender gap in life expectancy over the past decade (Pampel, 2002, 2005). In other words, the gender equalization process that developed countries experienced during the 1970s could lie behind the narrower gender mortality gap.

The effect of education on mortality has been extensively studied in previous literature, but does education have the same effect on survival for both men and women? Gender asymmetries in the health benefits of acquiring further education have been less studied. In principle, we expect to find gender differences when the health benefits of further education are conveyed via the labor market (through higher earnings, higher occupational status, or different exposure to occupational health hazards). If this hypothesis is true, and education does actually benefit men more than women, then the spread and greater access to education could partially explain this narrower gender mortality gap.

This paper's aim, therefore, is to analyze the interaction between gender and education as regards adult mortality at a time of increasing gender equality and women's greater access to economic opportunities. To do so, we resort to a quasi-natural experiment. In 1980, a new Workers Statute (Law 8/1980) was enacted in Spain that increased the minimum legal working age from 14 to 16. Yet the school leaving age remained at 14 until 1990. We use a differences-in-differences strategy to identify the reform's within-cohort effects, where our treated and control individuals will differ

[1] Source: OECD Health Statistics, 2016.

only in their month of birth.

The child labor reform of 1980 encouraged individuals to stay in the educational system according to their year and month of birth. Before the reform, both the school leaving age and the minimum working age were set at 14. This meant that individuals born at the beginning of the year were legally entitled to work before finishing their final year of primary education,[2] while individuals born at the end of the year reached the legal working age only after completing this final year. In 1980, when the legal working age rose to 16, this difference in incentives between those born at the beginning of the year and those born at the end disappeared. We exploit this difference in incentives affecting individuals born at the beginning and end of the year before and after the reform. As no other reform affecting the working or schooling age had been introduced until 1990, we are confident that no other confounding factor is affecting our estimates.[3]

A previous paper by Del Rey et al. (2018) focuses on the education and labor market impacts of the same child labor reform. They show that the reform was effective in their sample not only at providing incentives for treated individuals to finish primary education, but also to remain in the educational system. In particular, they find that the increase in the minimum statutory working age also increased the probability of girls and boys finishing primary education by 1.3 percentage points (10%) and 1.2 percentage points (7.4%), respectively. At the same time, the reform decreased the number of treated girls (boys) not attaining optional secondary education by 1.2 percentage points or 2.7% (1.6 percentage points or 3.2%). These results show that restricting child labor effectively increased the educational attainment of the individuals in question.

This paper further extends the work by Del Rey et al. (2018) by analyzing the reform's effects on long-term mortality rates. We find that the child labor reform reduced the mortality rate among young men (aged 14-29) in the sample by 0.07 per thousand deaths. This corresponds to a 6.3% decrease in their mortality rate at this age. This decrease is entirely driven by a 12.2% decrease in deaths due to external causes. We also show that there is a 14.7% decrease in the mortality rate due to external causes among young women. Surprisingly, we also find that the mortality rate among prime-age (30-45) treated women increased by 0.048 per thousand deaths (or 6.3%). When analyzing this increase in detail, we find that this effect is driven by an increase in the mortality rate due to HIV (11.6%), and diseases of the nervous and circulatory system (8.7%). This last finding

[2]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 turn 14 during the final year of primary education, while those born at the end of the year are still 13 years old.

[3]In 1990, an educational reform increased the school leaving age from 14 to 16. See Felgueroso et al. (2014) for an evaluation of this reform in Spain

could be a result of more educated women engaging in less healthy habits. In fact, we find that women affected by the reform had a higher probability of consuming alcohol and of having taken an HIV test, compared to women not affected by the reform. This unexpected effect is related to the social context in Spain at the time of the reform.

Thus, the contextualization of the reform is crucial for interpreting our results. Spain's Workers Statute was enacted in 1980, just a few years after the end of Franco's dictatorship, which lasted almost 40 years. In 1980, the country's levels of educational attainment, child labor, and women's social development were closer to those of a middle-income country. On the one hand, 16.19% of boys and 12.71% of girls in 1965 (last cohort not affected by the reform) did not complete their compulsory education. On the other, 49.3% of boys and 43.8% of girls in the same cohort did not finish upper secondary education (Del Rey et al., 2018). A large percentage of the Spanish population entered the labor market at a very young age. Before 1980, around 40% (15%) of boys and 30% (10%) of girls were already working by the age of 15 (14). Moreover, health risk factors were peaking during this period; in particular, substance abuse and car accidents were at, or about to reach, a record high.[4] Furthermore, the level of social development for those cohorts born between 1940 and 1960 was substantially different according to gender. During the dictatorship, Spain was a male-dominated society, with women's rights generally ignored or suppressed. This meant that very few women had access to higher education, and women's labor market participation rates were low. For instance, in 1975 only 27.9% (34.5% in 1985) of working-age women in Spain were actually participating in the labor market (World Bank, 2009). The end of the dictatorship raised the level of gender equality and improved women's access to economic opportunities (Philips, 2010). This gender equalization process led to a convergence of health risk factors (e.g., smoking, drinking, taking drugs, and sexual promiscuity) for both men and women. Among the cohorts of women unaffected by the reform, better educated women smoked more than women with less education (Bilal et al., 2015). This inverse gradient for Spanish women was gradually reversed among the cohorts of women born after 1980, when the country's gradient begins to mirror that of more developed countries, with less educated women recording higher smoking rates.

This paper contributes to previous literature in several ways. First, we formally investigate the gender differences in the causal effect of education on adult mortality rates at a time of increasing gender equality and women's greater access to economic opportunities. Most previous literature has either focused solely on men (Van Kippersluis et al., 2011; Cipollone and Rosolia, 2011), or

[4] The literature has shown that AIDS (de Olalla García et al., 1999; Gómez-Redondo and Boe, 2005), drugs and alcohol abuse (Ribes et al., 2004), and fatal traffic injuries (Saiz-Sánchez et al., 1999; Gine, 1992; Puig et al., 1983; Gómez-Redondo and Boe, 2005; Serra et al., 2006) all peaked during the late 1970s and early 1980s, especially for young cohorts.

analyzed reforms that took place before the 1950s, when female labor market participation was very low (Oreopoulos, 2006; Albouy and Lequien, 2009; Clark and Royer, 2013; Lleras-Muney, 2005; Meghir et al., 2018).[5] Three previous papers have reported gender's differential effects on mortality rates. Gathmann et al. (2015) analyze the effect of compulsory schooling reforms in 18 European countries, and find that they differ by gender. In particular, they show that education reduces the mortality rate among men, but not so among women. Palme and Simeonova (2015) analyze a reform that increased the number of compulsory years of education from seven to eight in Sweden. They find that the reform increased not only the probability of being diagnosed with breast cancer in women, but also the probability of dying from the disease. They also point out that a potential mechanism relies on the qualities, behaviors, and risk factors acquired in the process of obtaining more education. Finally, Kemptner et al. (2011) investigate the causal effect of several changes in compulsory schooling laws between 1949 and 1969 in the former West Germany, and find that education has a positive effect on long-term illness among men, but not among women.

Secondly, as far as we know, this is the first paper to investigate the effect of a child labor regulation on adult mortality rates. Previous literature has mainly used changes in compulsory schooling laws as an instrument to identify the causal effect of education on many health outcomes and health behaviors (Oreopoulos, 2006; Clark and Royer, 2013; Lleras-Muney, 2005; Meghir et al., 2018; Albouy and Lequien, 2009; Kemptner et al., 2011). However, even in this extensive literature, there is a lack of consensus on the sign and size of this impact.[6] Other studies have examined the effect of both child labor laws and compulsory schooling laws on short-term outcomes such as educational attainment and child labor (Goldin and Katz, 2011; Lleras-Muney, 2002; Edmonds and Shrestha, 2012)[7].

[5]Oreopoulos (2006) examines two changes in the school leaving age that were enacted in the UK in 1947 and 1957. Clark and Royer (2013) have also explored the UK reform of 1947 and a further reform in 1972. Lleras-Muney (2005) has analyzed two reforms in the US in 1915 and 1939. Meghir et al. (2018) has estimated the one-year increase in the length of compulsory schooling that was enacted in Sweden between 1949 and 1962. Finally, Albouy and Lequien (2009) have analyzed two reforms in France in 1923 and 1953.

[6]On the one hand, Lleras-Muney (2005) for the US, Oreopoulos (2006), for the UK, and Van Kippersluis et al. (2011) for the Netherlands find that educational attainment has a strong positive impact on mortality rates. Nevertheless, Clark and Royer (2013) using two compulsory schooling reforms in the UK, do not find any significant effect of education on such rates. Meghir et al. (2018) and Albouy and Lequien (2009) do not find any causal impact of schooling on mortality rates either in Sweden or in France, respectively.

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

Child labor reforms differ from compulsory schooling reforms in many aspects. For one, the type of individuals affected will be different with each type of reform. Compulsory schooling reforms will force children to stay in the educational system, increasing educational attainment across the board (if correctly applied). A child labor reform, on the other hand, will only act as a subtle incentive to continue studying. This means child labor reforms will be more likely to increase the educational attainment of children whose main motivation to drop out was the need to contribute to the household income by working. Moreover, compulsory schooling reforms tend 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 any improvement in the quality of teaching. A child labor reform typically involves labor market legislation, and thus leads to increases in educational attainment without affecting the educational system in any other way.

Thirdly, this paper contributes to the discussion on the link between education and mortality rates in middle-income countries experiencing a gender equalization process. Previous studies on the causality between education and mortality have largely focused on developed countries (mainly the US (Lleras-Muney, 2005), the UK (Oreopoulos, 2006; Clark and Royer, 2013), the Netherlands (Van Kippersluis et al., 2011), Sweden (Meghir et al., 2018), and France (Albouy and Lequien, 2009). As education could impact differently on health and mortality in countries with different levels of development, this paper sheds light on a reform that affected what was a middle-income country at the time of the reform.

Finally, our identification strategy allows us to estimate the reform's within-cohort effects, where our treated individuals and their control counterparts differ only in their month of birth. Consequently, our identification strategy will be robust to any concurrent social or political events, as these will have the same impact on both our treatment and control groups. Moreover, as we use a difference-in-difference estimator, we do not rely on the assumption that individuals born in different months are equal. The only assumption we are making is that if there are indeed differences between those born at the beginning and at the end of the year, these differences remain constant for the cohorts before and after the reform.

The remainder of the paper is organized as follows. Section 2 introduces the reform we are addressing and the identification strategy. Section 3 presents the reform's effects on mortality rates. Section 4 performs a number of robustness checks, while Section 5 concludes with a discussion of the main results and their policy implications.

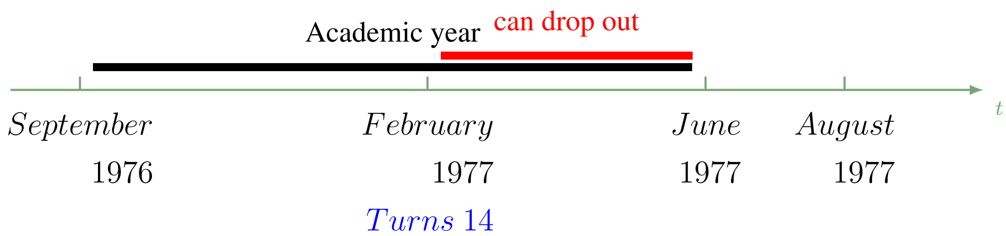
2 Institutional Context and Identification Strategy

In March of 1980, a child labor regulation (Law 8/1980 "Estatuto de los Trabajadores" (ET)) was enacted to raise the minimum legal working age from 14 to 16. We use this exogenous variation in the incentive to stay out of the labor market to build our identification strategy. Only individuals born after 1966, and who were 14 or over at the time the reform was passed, were affected by it. This means we will be comparing individuals born before 1966 to those born after.

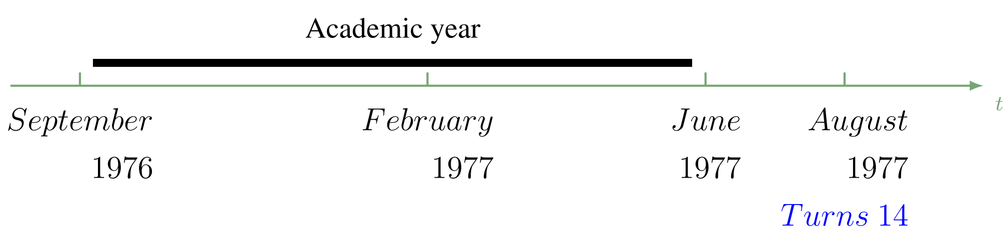
This reform also induced different incentives depending on each individual's birth month. This is because of the Spanish educational system and the compulsory schooling age that was maintained at 14 until 1990. In Spain, all the children in the same cohort start school the same year. This means that some children are six years-old, while others are still five when they start school. Likewise, some children finish their final year of primary school when they are 14, while others are still 13 at the end of the academic year. Before the reform, therefore, individuals born in the first months of the year reached the minimum legal working age (14) before finishing their final year of primary education, and had an incentive to leave school before completing their primary education. However, students born during the last months of the year were not old enough to legally work before completing their primary education. Before the reform, therefore, individuals born at the beginning of the year had fewer incentives to complete their primary education compared to individuals born at the end of the year. The 1980 reform eliminates these differences in incentives. After the reform, all individuals, regardless of their birth month, had the same incentives to finish primary education, as now they could not work until they were 16.

With a view to clarifying the different incentives for remaining in the educational system, the following chart illustrates the choices of two individuals born in the same year, 1963 (pre-reform), during their final year at primary school:

1. An individual born in February 1963:



2. For an individual born in August 1963:



This chart shows that before the reform, the two individuals' incentives to stay in the educational system during the final year of primary education differed depending on whether they were born in the first part of the year (from January to May) or in the last part of the year (from July to December). The reform removed these differential incentives.

2.1 Identification strategy

We use the exogenous change in the incentives introduced by the ET reform to identify the causal effect of a child labor regulation on adult mortality rates. In order to identify the policy's effects, we compare the outcomes among individuals born in the first/last months of the years before and after the introduction of the reform. We will then identify the reform's within-cohort effects. We are aware that this effect is potentially smaller than the between-cohort effect (comparing the entire 1966 cohort with the 1967 cohort). However, our results will be more reliable than the before-after approach, as our estimates will not be affected by any other concurrent events. This is important in our setting, as this reform was approved during a period of significant social change in Spain.

Formally, we consider the following econometric model:

$$Outcome_{jct} = \alpha + \beta_1 Treated + \beta_2 Treated * Post\ Reform_c + BY_c + CY_t + \epsilon_{jct}$$

The outcome of interest is the mortality rate of treated and control individuals (indexed j) of cohort c observed in year t. We construct this outcome using register mortality data obtained from Spain's National Institute of Statistics from 1975 until 2016.[8]. We collapse the individual data at the level of cohort and calendar year for the treated and control groups. We then divide the number of deaths by the number of individuals born in each cohort and treatment (and multiply it by 1,000). $Treated$ is a dummy variable that equals one if the individual is born between March and May, and zero if they are born between August and October.[9]. $Post\ Reform_c$ is also a dummy variable that takes a value of one for the cohort of individuals that turned 14 after the reform, and zero otherwise. We then define the pre-Reform cohorts as those born from 1961 to 1965, and the post-Reform cohorts as those born between 1967 and 1971. We also include cohort (BY_c) and calendar year (CY_t) fixed effects. We cluster the standard errors at cohort level, and we report them in parenthesis. We also perform a wild bootstrap with 1,000 repetitions, and we report the p-values in brackets.

The effect of the reform, after controlling for cohort and calendar time fixed effects, can be identified by the coefficient of the interaction between the post-reform and the treatment dummy variable, β_2. All the results are robust to the substitution of cohort time dummies by linear, quadratic and quartic pre- and post-reform trends.[10]

It is important to note that our analysis omits the cohort born in 1966 because they turned 14 in 1980, the year the reform was introduced. We also exclude migrants, as we do not have information on when they arrived in Spain, so we cannot determine whether they were affected by the reform. As mortality is age-specific, it is important for all the cohorts of individuals being considered (1961-1971) to have ex-ante the same probability of dying during all the years we observe mortality rates (1975-2016). We therefore restrict the sample to include deaths occurring between the ages of 14 and 45. This age restriction allows us to include the same ages for all the cohorts considered, as individuals in the first cohort (1961) are 14 in the first year of the register (1975), and individuals in the last cohort (1971) are 45 in the last year of the register (2016).

With this identification strategy, we are assuming that the reform did not have any effect for the

[8]For more information on this database, please go to the Data Appendix.

[9]Results are robust when we compare individuals born between January and May with individuals born between July and December.

[10]These results are available upon request.

cohort of individuals aged between 14 and 16 when the reform was passed (those individuals born in 1964, 1965 and 1966). In particular, we are assuming that the reform forced all the individuals aged between 14 and 16 to leave their job when the reform was enacted (and could have been working before the reform). This is a major assumption that we will seek to relax in Section 4.1.

As we already have explained in the introduction, Del Rey et al. (2018) show that the reform was effective in improving the educational attainment of affected individuals. Using the same identification strategy, they find that the increase in the minimum statutory working age also increased the probability of girls and boys finishing their primary education by 1.3 percentage points (10%) and 1.2 percentage points (7.4%), respectively. The reform also increased student numbers in post-compulsory education. In particular, it decreased the number of treated girls (boys) not attaining secondary post-compulsory education by 1.2 percentage points or 2.7% (1.6 percentage points or 3.2%). We will now go on to analyze the effects of the reform on the mortality rates of affected individuals.

3 Effect of the Reform on Mortality

This section explores whether the increase in the minimum working age had any impact on long-term mortality. Table 1 shows the results for mortality rates at ages 14-45 for men and women. We can see that the mortality rates before the reform for both men and women born at the beginning of the year are higher than for those born at the end of the year, as can be seen by the positive and significant coefficient of the "Treated" variable. However, it seems that the reform did not significantly reduce the mortality rate for the treated group, as the interaction coefficient is not significant.

In order to explore these results further, we split the mortality rate into a short-term effect (ages 14-29) and a longer-term one (ages 30-45). As mortality is age-specific, the policy may have affected mortality differently among younger and older individuals. Before addressing the regression results, Figure 2 reports the raw data and the predictions from the estimation model for women and men in the treatment and control groups for all cohorts during the 1961–1971 period. Graph a) shows that before the reform, a man or a woman born at the beginning of the year had a significantly higher mortality rate by the age of 30 compared to another man or woman born at the end of the same year. However, this difference is attenuated after the reform has been implemented. On the other hand, the difference in the morality rate after the age of 30 of men born at the beginning and the end of the year does not appear to be affected by the reform. Finally, graph b) also shows that, before the reform, women born in different months of the year had the same mortality rate after the age of 30, while this difference grows after the reform is implemented. More precisely, Column (2) in Table 1 shows that the reform decreased the mortality rate of young treated men (aged 14-29) by 0.07 per 1,000 men. This corresponds to a 6.3% decrease with respect to the pre-reform mean. Column (4) shows that the reform also seems to decrease the mortality rate of young treated women by 8.9%,[11] although the coefficient is marginally nonsignificant (the p-value is 0.116).

When looking at the effects of the reform over the longer run, we see that the reform did not have a significant impact on the mortality rates of affected men aged 30-45, while it significantly increased these rates among prime-age affected women. Column (6) shows that the reform increased the mortality rate of women aged 30-45 by 0.048 per thousand women, or by 6.3% with respect to the pre-reform mean. Thus, the child labor reform reduced mortality rates for young men and women, while it increased the mortality rates of the older group of women. The next section explores the potential reasons behind the unexpected increase in the mortality rates for middle-aged women.

3.1 Explaining the Mortality Effects: Causes of Death

In order to shed some light on the mechanisms explaining the impact the regulation of child labor had on men and women's mortality rates, we explore the different causes of death. We divide mortality rates into ten potential factors: 1) infectious and blood diseases,[12] 2) HIV,[13] 3) tumors,[14], 4) female tumors,[15] 5) endocrine, nutritional and metabolic diseases,[16] 6) diseases of the nervous and circulatory system,[17] 7) diseases of the respiratory system,[18] 8) diseases of the digestive and urinary system,[19] 9) diseases related to pregnancy, delivery and post-partum period, and 10) exter-

[11] Note that the pre-reform mortality rate for young individuals differs greatly between genders. There is a mortality rate of 1.1 per thousand men (aged 14-29) before the reform, while the same rate for women of the same age is 0.39 per thousand women.

[12] This classification includes diseases such as infectious intestinal complaints, tuberculosis, meningococcal disorder, septicemia, and viral hepatitis.

[13] This classification includes HIV and AIDS.

[14] This classification includes malignant tumors located in different parts of the body.

[15] This classification includes malignant tumors of the breast, the cervix, or the ovary.

[16] 16 This classification includes diseases such as mellitus diabetes.

[17] This classification includes diseases such as meningitis, Alzheimer's, chronic rheumatic cardiac disorders, hypertensive complaints, acute myocardial infarction, ischemic complaints of the heart, heart failure, cerebrovascular complaints, atherosclerosis, or disorders of the blood vessels.

[18] This classification includes diseases such as influenza, pneumonia, chronic complaints of the lower respiratory tract, asthma, and respiratory insufficiency.

[19] This classification includes diseases such as stomach ulcer, enteritis, non-infectious colitis, intestinal vascular disorder, cirrhosis, kidney complaints, or disorders of the genital organs.

nal causes of mortality.[20] As in the previous section, we run different models for men and women, and we distinguish between the short-term effects (ages 14-29) and the longer-term effects (ages 30-45).

Table 2 shows the reform's effect on mortality rates for each of the causes of death among men in the 14-29 age group. The reform only affects death due to external causes among young treated men. The reform decreased the mortality rate due to external causes by 0.079 per thousand deaths, or 12.2%, with respect to the pre-reform mean. Thus, the reduction in total mortality rates observed in Table 1 for young treated men is mostly driven by the reduction in external causes of death, which include all types of accidents, suicides or physical violence. Unfortunately, we cannot identify the types of accidents informing this reduction in mortality rates, but this finding is consistent with previous studies pointing to reductions in accidents due to increases in the length of compulsory education (Lager and Torssander, 2012).

Table 3 shows the effect of the child labor reform on the mortality rates of men aged 30-45. Consistent with the findings for total mortality shown in Table 1, the reform does not have any impact on any one of the causes of death among middle-aged treated men. Thus, the results for men show that the child labor reform decreased mortality rates for young treated men by decreasing the deaths due to external causes (e.g., accidents), while it does not have any impact among middle-aged treated men (in any of the causes of death).

When we look at the results of the causes of death among women, Table 4 shows that the only reduction in mortality rates among young treated women is in external causes of death (like men). In particular, regression 10 shows that the reform decreased the mortality rate among women aged 14-29 due to external causes by 0.021 per thousand women, or by 14.7% with respect to the pre-reform mean. The reform's effect on the mortality rate among young treated individuals does not seem to vary by gender.

Finally, Table 5 shows the results for the ten causes of mortality among middle-aged women. We can see that the reform increased the mortality rate due to HIV by 0.011 per thousand treated women, or by 11.6% with respect to the pre-reform mean. Column 6 also indicates that the mortality rate due to diseases of the nervous and circulatory system increased as a consequence of the reform by 0.014 per thousand treated women, or by 8.7%.

[20]This classification includes deaths due to car accidents, accidental falls, drowning, accidents with fire, accidental poisoning, suicide, physical violence, or healthcare complications.

Deaths due to HIV and the circulatory system (chronic rheumatic cardiac diseases, hypertensive diseases, acute myocardial infarction, ischemic diseases of the heart, heart failure, cerebrovascular diseases, atherosclerosis, or diseases of the blood vessels) are greatly affected by unhealthy behaviors, such as drinking, smoking, drug abuse or risky sexual practices (Borzecki et al., 2002; for Disease Control et al., 2010). In order to explore whether unhealthy habits are behind this increase in mortality for middle-aged women, we study the reform's effect on the probability of engaging in unhealthy behaviors. We use the Survey on Health and Sexual Habits conducted by the Spanish National Institute of Statistics in 2003, which contains information on alcohol consumption, drug consumption, and sexual behaviors.

Table 6 reports the reform's effect on unhealthy behavior among women. We observe that the reform increased the probability of treated women consuming alcohol. In particular, and after the reform, treated women have a 4.5 percentage point (27%) higher probability of consuming alcohol daily, and a 10.4 percentage point (30%) higher probability of consuming alcohol more than twice a week, compared to women not affected by the child labor reform. However, we do not find any evidence of an increased probability of the affected women having ever used injectable drugs. We are aware that using injectable drugs is an extreme variable for capturing increases in drug consumption; however, this is the only question regarding drug consumption available in the survey. Neither do we find any effect on the total number of sexual partners reported, used as a proxy for risky sexual behavior. Interestingly, we do find that women affected by the reform have a 9.3 percentage point (37.5%) higher probability of having been tested for HIV, and an 8.4 percentage point (36.2%) higher probability of collecting and knowing the test's result.

Our results therefore show that women affected by the reform had a higher probability of engaging in unhealthy behavior, which may (at least partly) explain the increase in mortality rates due to HIV and circulatory system diseases. In the case of men, none of the health behavior results is significantly altered by the reform.[21]. This is consistent with the fact that the reform does not have any impact on the mortality rate of middle-aged treated men.

The gender differences in the impact of education on risky behaviors are driven by the gender equalization process that the affected women were experiencing when the reform took place. Women in these cohorts were growing up during the early post-Franco era, receiving more education and increasing their participation in the labor market. For these women, access to smoking and its social acceptance were much higher than for previous (pre-reform) cohorts. For instance, a recent paper by Bilal et al. (2015) reports a high negative correlation between gender inequality

[21]The reform's effect on unhealthy behaviors among men can be found in the appendix (Table A4)

and the female-to-male smoking ratio in Spain from the 1960s to the 2010s.

Importantly, this positive association between education and the prevalence of smoking and drinking among women cannot be considered a particular case affecting Spain. In many countries in the world, the number of women smoking and drinking is increasing, even though the rates of smoking and drinking among women are 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 and drinking in the past (Mackay and Amos, 2003). IIn some Eastern European and Eastern Mediterranean countries, a high rate of smoking and drinking among highly educated women, compared to those with little education, has been reported in 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. Our results are therefore more relevant, from a policy perspective, to developing countries, whose educational systems, child labor market participation rates, and women's social development are similar to the levels that Spain was experiencing around 1980.

4 Robustness Checks

This section contains several robustness checks for our key results. First, we examine the robustness of our results when we consider the cohorts of women born in 1964 to 1966 as partially affected by the reform, or as non-compliers. Secondly, we explore the sensitivity of our key results to the inclusion of regional fixed effects, or age fixed effects. Finally, we perform some placebo tests, where we change the timing of the reform and some of the events studied.

4.1 Considering the Cohorts born in 1964, 1965 and 1966 as Partially Affected by the Reform or Potential Non-Compliers

The ET reform we are examining was enacted in March 1980. This means that all individuals born after February 1966 turned 14 after the reform had been passed, and were fully affected by it. Likewise, all the individuals born before March 1964 were 16 years-old when the reform was introduced, and so were completely unaffected by it. Individuals born between March 1966 and February 1966, however, were aged between 14 and 16 when the ET reform was enacted. In the previous analysis, we have assumed that these individuals were unaffected by the reform. In this section we will relax this assumption.

First, we consider these cohorts of individuals as partially affected. We use the number of months these individuals had to wait before they could start working. Thus, our post-Reform variable is no longer a dummy, but a continuous variable. The post-Reform variable continues to take a value of 1 for all individuals born in or after March 1966, as they are fully affected by the reform and had to wait for two years to start working. In addition, the variable will take a value of 0 for all individuals born before February 1964, as they are not affected in any way by the reform (they could start working immediately, as they were already 16 years-old). The post-Reform variable will take a value between 0 and 1 for individuals born between March 1964 and February 1966, depending on the number of months they had to wait until they could start working as a result of the ET reform. For example, someone born in March 1964 had to wait for a month before they could start working, as they were only one month away from turning 16 when the reform was passed. The post-Reform variable will thus take a value of 1/24 for these individuals (as those fully affected had to wait two years or 24 months to start working when the reform was passed). In the same way, the post-Reform variable will take the value of 2/24 for all individuals born in April 1964, and so on. We follow this rule through to individuals born in February 1966, who were affected by the reform for 23 months (the variable takes a value of 23/24).

The first regressions in Tables 7, 8, 9, and 10 show that our main results are robust in sign when this alternative specification is used, although some of the coefficients lose their significance. For instance, the reform now decreases the mortality rate among men aged between 14 and 29 by 0.053 per thousand men (instead of 0.069) (see Table A of Table 7)), although the coefficient is not significant. In addition, the effect of the reform on the mortality rate among young women due to external causes has a point estimate that is practically identical to our baseline specification, although it is no longer significant in Table 8. The rest of the key results are closely similar, and maintain the significant level of the main specification.

An alternative assumption is to consider the cohorts of 1964, 1965 and 1966 as potential non-compliers of the law. We can then check the sensitivity of our results by sequentially dropping these cohorts from the analysis. The results in the second and third columns in Tables 7, 8, 9, and 10 indicate that the reform's effects on age and cause of specific mortality rates are unchanged when we exclude these two additional cohorts. We may therefore conclude that our results are robust to the exclusion of possible non-compliers.

4.2 Including Regional or Age Fixed Effects

The previous analysis constructed the mortality rates using data recorded from 1975 to 2016 and collapsing it at the level of cohort and calendar year for the treated and control groups. As a robustness test, we now collapse the data also at regional level to control for the effects on mortality rates that are time-invariant at regional level. We use the same econometric specification as before, except for the inclusion of regional dummies.

The fourth column in Tables 7, 8, 9, and 10 shows that the effects on our key results are very robust to the inclusion of this regional fixed effects.

Finally, the fifth column in Tables 7, 8, 9, and 10 includes age dummies as controls. The results are also very robust to this alternative specification.

4.3 Placebos

We also perform several placebo tests, assuming the reform took place in different years (prior to 1980). We then examine the effect of four "fake" reforms, affecting the cohorts of 1962, 1963, 1964, and 1965, using the same econometric specification and definition of treatment status as before. We do not expect our estimation's interaction term to be significant for any of these years.

In Figure 3 shows the coefficient and the 95% confidence interval of our estimation's interaction term for the different key results. It is important to note that we do not use wild bootstrap to correct the standard errors reported in the graph, but if anything, we expect this correction to increase the standard errors of our estimates. Graph a) in Figure 1 shows the effect that the different "fake" reforms have on the mortality rate among young men (ages 14-29). None of the coefficients of these "fake" reforms is significant at the 95% level. In graph b), we plot the interaction term and 95% confidence interval on the mortality rate of middle-aged women (ages 30-45), and the "fake" reforms appear to be significant at the 95% level solely for the 1962 cohort.

Moreover, graphs c), d), e) and f) in Figure 3 aagain indicate that none of the "fake" reforms has an effect on the mortality rate of young men and women due to external causes of death, or on the mortality rate of middle-aged women due to HIV or diseases of the nervous and circulatory system. We therefore believe that the parallel assumption is fulfilled in our analysis, and there were no differences between the treatment and control for any of the previous years before the reform took place.

5 Discussion

This paper explores the effects of a reform introduced in Spain in 1980 that raised the legal working age from 14 to 16, while the school-leaving age remained at 14. Before the reform, children born in the first months of the year turned 14 and were legally able to work before finishing compulsory education. The 1980 labor market reform eliminated the difference in the alternatives available to individuals born at different times of the year because they would all have obtained compulsory education by the time they reached the legal working age.

We exploit this difference in incentives between treated individuals (born in the first months of the year) and their control counterparts (born in the last months of the year) before and after the reform, to estimate that the reform reduced the mortality rate among young men (aged 14 -29) in the treatment group by 0.07 per thousand deaths (6.3% decrease with respect to the pre-reform mean). This decrease in mortality is entirely driven by a 12.2% decrease in the mortality rate due to external causes. We also show that there is a 14.7% decrease in the mortality rate due to external causes among young treated women. Surprisingly, we also find that the mortality rate of prime-age (30-45) treated women increased by 0.05 every thousand deaths (or 6.3%). When analyzing this increase in detail, we show that this effect is driven by an increase in the mortality rate of HIV (11.6%), and diseases of the nervous and circulatory system (8.7%). With respect to the increase in the mortality rate among prime-age treated women, we show that their health habits also deteriorated, which increased the incidence of habit-related diseases, and ultimately led to higher mortality rates. As we mentioned in the introduction, this effect may be partially explained by the inverse education gradient in smoking rates among women from pre-reform cohorts found by Bilal et al. (2015).

Together, these results help explain the closing of the life expectancy age gap between women and men in Spain, which has narrowed by 1.5 years over the past twenty years. Furthermore, although the literature has typically reported the positive effects of education on health, our results are consistent with a recent strand of literature that reports differential effects of education on mortality by gender (Gathmann et al., 2015; Palme and Simeonova, 2015).

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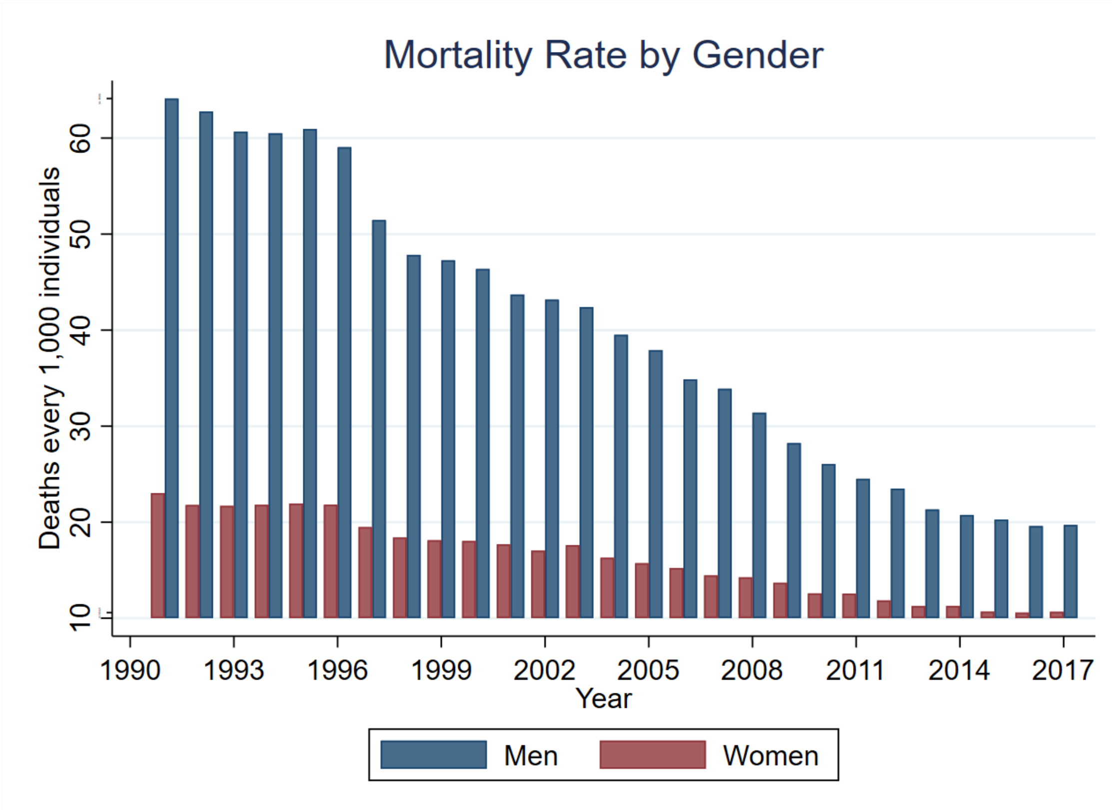
Tables and Figures

Figure 1: EVOLUTION OF THE MORTALITY RATE BY GENDER IN SPAIN



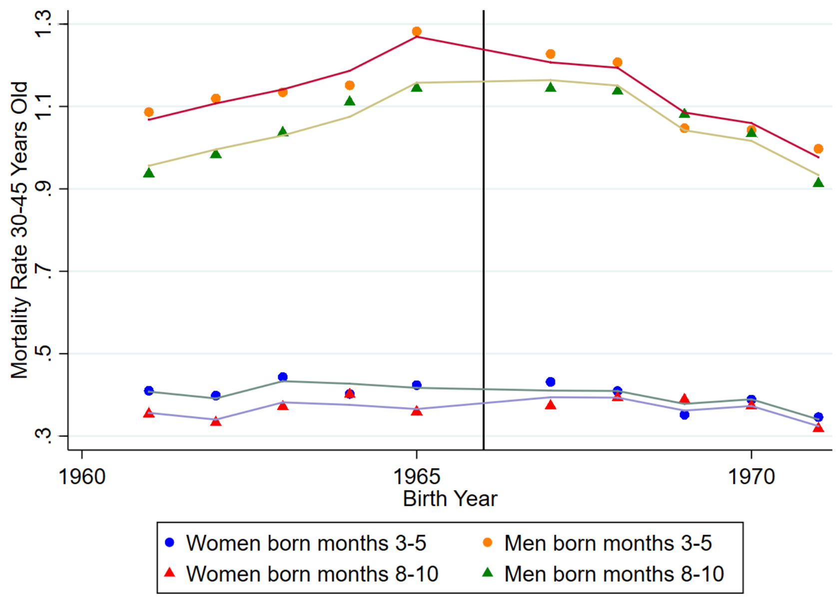
Notes: Number of deaths among men and women aged 14-45 per 1,000 individuals of that age and gender. Source: Mortality registries (1991-2017).

Table 1: Effect of the Reform on Age and Gender-specific Mortality Rates

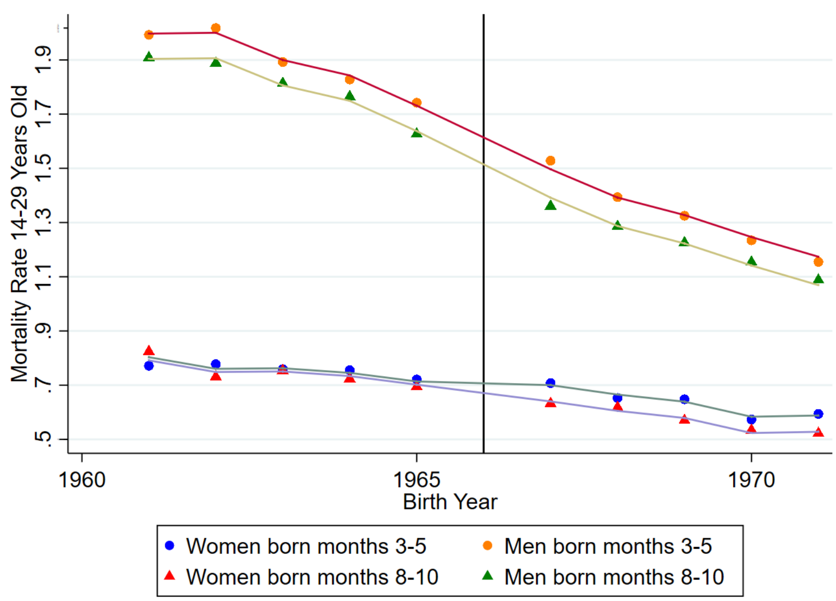
	Mortality					
	Men			Women		
	Aged 14-45 (1)	Under 30 (2)	Over 30 (3)	1 Aged 4-45 (4)	Over 30 (5)	Under 30 (6)
Treated	0.103**	0.112**	0.094**	0.032**	0.051**	0.012
	(0.015)	(0.020)	(0.012)	(0.010)	(0.013)	(0.017)
	[0.020]	[0.031]	[0.026]	[0.017]	[0.044]	[0.542]
Treated* Post Reform	-0.029	-0.069**	0.011	0.007	-0.035	0.048**
	(0.022)	(0.031)	(0.021)	(0.013)	(0.020)	(0.019)
	[0.221]	[0.038]	[0.633]	[0.639]	[0.116]	[0.022]
Observations	640	320	320	640	320	320
R^2	0.894	0.910	0.863	0.807	0.635	0.808
Calendar Year FE	YES	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	1.473	1.099	1.847	0.570	0.390	0.751
Std. dev. pre-reform	0.549	0.471	0.316	0.236	0.118	0.179

Notes: The dependent variables are the mortality rate (number of men/women that died divided by the total number of men/women born in each cohort and treatment) (1) of men between the ages of 14 and 45, (2) of men between the ages of 14 and 29, (3) of men between the ages of 30 and 45, (4) of women between the ages of 14 and 45, (5) of women between the ages of 14 and 29, and (6) of women between the ages of 30 and 45. All dependent variables are multiplied by 1,000. Regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. 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*: Mortality registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Figure 2: Gender-specific Mortality Rates by Cohort



(a) Mortality rate of individuals under 30



(b) Mortality rate of individuals over 30

Notes: The dots represent the average mortality rate of men/women born 1961-1971. The lines are the linear predictions from the regression. *Source*: Mortality registries (1991-2017).

Table 2: Effect of the Reform on the Mortality Rate by Cause of Death among Men aged 14-29

	Mortality rate- Men under 30							
	Infectious & Blood diseases	HIV	Tumors	Endocrine, nutritional & metabolic diseases	Diseases of the nervous & circulatory system	Diseases of the respiratory system	Diseases of the digestive & urinary system	External causes of mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.003	0.013***	-0.001	0.002	0.008**	0.006	0.004	0.081**
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.016)
	[0.414]	[0.012]	[0.976]	[0.335]	[0.028]	[0.205]	[0.341]	[0.025]
Treated* Post Reform	0.005	0.001	0.006	-0.000	0.004	-0.006	-0.002	-0.079***
	(0.004)	(0.011)	(0.007)	(0.002)	(0.003)	(0.005)	(0.004)	(0.020)
	[0.215]	[0.936]	[0.415]	[0.872]	[0.189]	[0.284]	[0.670]	[0.006]
Observations	320	320	320	320	320	320	320	320
R^2	0.541	0.721	0.197	0.131	0.523	0.442	0.437	0.723
Calendar Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0380	0.0701	0.0873	0.00518	0.102	0.0432	0.0265	0.647
Std. dev. pre-reform	0.0551	0.159	0.0311	0.00794	0.0459	0.0323	0.0240	0.281

Notes: The dependent variables are the number of men that died aged between 14 and 29 divided by the total number of men born in each cohort and treatment due to (1) infections and blood diseases, (2) HIV, (3) tumors, (4) endocrine, nutritional and metabolic diseases, (5) diseases of the nervous and circulatory system, (6) diseases of the respiratory system, (7) diseases of the digestive and urinary system, or (8) external causes. All dependent variables are multiplied by 1,000. Regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all men from cohorts 1961-1965 and 1967-1971.

Table 3: Effect of the Reform on the Mortality Rate by Cause of Death among Men aged 30 to 45

	Mortality rate- Men over 30							
	Infectious & Blood diseases	HIV	Tumors	Endocrine, nutritional & metabolic diseases	Diseases of the nervous & circulatory system	Diseases of the respiratory system	Diseases of the digestive & urinary system	External causes of mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.003	0.034**	0.017	0.002	0.005	0.004*	0.005	0.020
	(0.002)	(0.009)	(0.014)	(0.002)	(0.010)	(0.002)	(0.006)	(0.012)
	[0.217]	[0.025]	[0.328]	[0.407]	[0.752]	[0.091]	[0.410]	[0.135]
Treated* Post Reform	-0.004	-0.015	0.009	-0.003	0.009	0.001	0.005	0.006
	(0.003)	(0.011)	(0.016)	(0.002)	(0.013)	(0.007)	(0.008)	(0.014)
	[0.224]	[0.234]	[0.596]	[0.219]	[0.519]	[0.820]	[0.548]	[0.662]
Observations	320	320	320	320	320	320	320	320
R^2	0.353	0.947	0.803	0.318	0.774	0.293	0.680	0.685
Calendar Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0484	0.388	0.301	0.0154	0.256	0.0688	0.133	0.529
Std. dev. pre-reform	0.0237	0.286	0.164	0.0155	0.113	0.0296	0.0640	0.133

Notes: The dependent variables are the number of men that died between the ages of 30 and 45 divided by the total number of men born in each cohort and treatment due to (1) infections and blood diseases, (2) HIV, (3) tumors, (4) endocrine, nutritional and metabolic diseases, (5) diseases of the nervous and circulatory system, (6) diseases of the respiratory system, (7) diseases of the digestive and urinary system, or (8) external causes. All dependent variables are multiplied by 1,000. Regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all men from cohorts 1961-1965 and 1967-1971.

Table 4: Effect of the Reform on the Mortality Rate by Cause of Death among Women aged 14-29

	Mortality rate- Women under 30									
	Infectious & Blood diseases	HIV	Tumors	Feminine Tumors	Endocrine, nutritional & metabolic diseases	Diseases of the nervous & circulatory system	Diseases of the respiratory system	Diseases of the digestive & urinary system	Pregnancy, delivery & post-partum period	External causes of mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.001	0.002	0.002	0.000	-0.001	0.006	0.003	0.004	0.001	0.027**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.005)	(0.002)	(0.002)	(0.001)	(0.007)
	[0.530]	[0.242]	[0.539]	[0.694]	[0.151]	[0.267]	[0.197]	[0.173]	[0.241]	[0.037]
Treated* Post Reform	0.001	0.002	-0.001	0.002	0.000	-0.003	-0.003	-0.002	-0.001	-0.021**
	(0.001)	(0.003)	(0.007)	(0.002)	(0.001)	(0.006)	(0.004)	(0.003)	(0.001)	(0.010)
	[0.619]	[0.462]	[0.922]	[0.319]	[0.962]	[0.634]	[0.485]	[0.492]	[0.273]	[0.048]
Observations	320	320	320	320	320	320	320	320	320	320
R^2	0.334	0.723	0.126	0.283	0.156	0.228	0.129	0.206	0.138	0.381
Calendar Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0200	0.0233	0.0565	0.00722	0.00456	0.0516	0.0193	0.0133	0.00168	0.156
Std. dev. pre-reform	0.0195	0.0551	0.0261	0.0117	0.00747	0.0288	0.0154	0.0136	0.00465	0.0577

Notes: The dependent variables are the number of women that died between the ages of 14 and 29 divided by the total number of women born in each cohort and treatment due to (1) infections and blood diseases, (2) HIV, (3) tumors, (4) female tumors, (5) endocrine, nutritional and metabolic diseases, (6) diseases of the nervous and circulatory system, (7) diseases of the respiratory system, (8) diseases of the digestive and urinary system, (9) pregnancy, delivery and post-partum period, or (10) external causes. All dependent variables are multiplied by 1,000. Regressions include cohort time, and calendar year dummies. Treated are individuals born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all women from cohorts 1961-1965 and 1967-1971.

Table 5: Effect of the Reform on the Mortality Rate by Cause of Death among Women aged 30-45

	Mortality rate- Women over 30									
	Infectious & Blood diseases	HIV	Tumors	Female Tumors	Endocrine, nutritional & metabolic diseases	Diseases of the nervous & circulatory system	Diseases of the respiratory system	Diseases of the digestive & urinary system	Pregnancy, delivery & post-partum period	External causes of mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.003	-0.009**	0.013	0.007	0.000	-0.005	-0.003	-0.005	-0.000	0.002
	(0.002)	(0.005)	(0.009)	(0.007)	(0.001)	(0.003)	(0.005)	(0.006)	(0.001)	(0.005)
	[0.229]	[0.020]	[0.224]	[0.373]	[0.756]	[0.159]	[0.460]	[0.444]	[0.906]	[0.740]
Treated* Post Reform	-0.001	0.011*	-0.006	0.004	0.001	0.014***	0.007	0.007	-0.002	0.013
	(0.004)	(0.006)	(0.012)	(0.009)	(0.002)	(0.003)	(0.005)	(0.007)	(0.001)	(0.009)
	[0.876]	[0.084]	[0.598]	[0.700]	[0.596]	[0.005]	[0.212]	[0.365]	[0.248]	[0.175]
Observations	320	320	320	320	320	320	320	320	320	320
R^2	0.192	0.834	0.758	0.779	0.152	0.465	0.207	0.359	0.118	0.232
Calendar Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0184	0.0945	0.177	0.125	0.00883	0.100	0.0258	0.0460	0.00197	0.113
Std. dev. pre-reform	0.0136	0.0791	0.0901	0.0762	0.0101	0.0439	0.0189	0.0288	0.00471	0.0385

Notes: The dependent variables are the number of women that died between the ages of 30 and 45 divided by the total number of women born in each cohort and treatment due to (1) infections and blood diseases, (2) HIV, (3) tumors, (4) female tumors, (5) endocrine, nutritional and metabolic diseases, (6) diseases of the nervous and circulatory system, (7) diseases of the respiratory system, (8) diseases of the digestive and urinary system, (9) pregnancy, delivery and post-partum period, or (10) external causes. All dependent variables are multiplied by 1,000. Regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all women from cohorts 1961-1965 and 1967-1971.

Table 6: Effect of the Reform on the Health Habits of Women

	Consumption of alcohol daily		Consumption of alcohol > twice per week		Injectable drugs		Total number of sexual partners		HIV tested		Knows the results of HIV test	
	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	-0.006	-0.002	-0.038	-0.022	-0.006	-0.011*	0.114	0.101	-0.044	-0.053**	-0.050	-0.051*
	(0.008)	(0.008)	(0.041)	(0.037)	(0.004)	(0.006)	(0.085)	(0.072)	(0.027)	(0.023)	(0.031)	(0.026)
	[0.325]	[0.718]	[0.469]	[0.564]	[0.212]	[0.136]	[0.333]	[0.272]	[0.205]	[0.083]	[0.188]	[0.064]
Treated* Post Reform	0.045**	0.041**	0.104*	0.085*	-0.016	-0.008	-0.010	-0.034	0.059	0.093*	0.058	0.084*
	(0.016)	(0.015)	(0.050)	(0.045)	(0.017)	(0.016)	(0.112)	(0.105)	(0.043)	(0.043)	(0.046)	(0.044)
	[0.024]	[0.023]	[0.080]	[0.118]	[0.399]	[0.672]	[0.919]	[0.749]	[0.206]	[0.053]	[0.243]	[0.067]
Observations	925	1,115	925	1,115	925	1,115	898	1,082	925	1,115	925	1,115
R^2	0.038	0.039	0.048	0.042	0.037	0.029	0.048	0.043	0.057	0.075	0.057	0.076
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.169	0.169	0.341	0.341	0.0236	0.0236	2.491	2.491	0.248	0.248	0.232	0.232
Std. dev. pre-reform	0.375	0.375	0.474	0.474	0.152	0.152	1.782	1.782	0.432	0.432	0.422	0.422

Notes: The dependent variables are (1-2) the probability of consuming alcohol daily, (3-4) the probability of consuming alcohol more than twice a week, (5-6) the probability of ever using injectable drugs, (7-8) the total number of sexual partners, (9-10) the probability of having ever tested for HIV, and (11-12) the probability of knowing the results of the HIV test. Regressions 1, 3, 5, 7, 9, and 11 take into account the cohort of women born from 1961 to 1971, while regressions 2, 4, 6, 8, 10 and 12 include the cohorts of women born from 1960 to 1972. Regressions include cohort time, and regional dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Survey on Health and Sexual Habits (2003), all women from cohorts 1960-1965 and 1967-1972.

Table 7: Robustness Check: Mortality Rate of Men and Women under 30

	Mortality rate - Men under 30				
	1964-66 Partially affected	Eliminate 1965-66	1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	0.103	0.105*	0.128***	0.115**	0.112**
	(0.024)	(0.024)	(0.015)	(0.020)	(0.021)
	[0.228]	[0.056]	[0.003]	[0.025]	[0.022]
Treated* Post Reform	-0.053	-0.062	-0.084**	-0.072**	-0.069**
	(0.030)	(0.034)	(0.028)	(0.029)	(0.032)
	[0.176]	[0.111]	[0.050]	[0.040]	[0.037]
Post Reform	0.229*				
	(0.123)				
	[0.068]				
Observations	480	288	256	5,440	320
R^2	0.875	0.909	0.904	0.467	0.940
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	1.050	1.070	1.050	1.133	1.099
Std. dev. pre-reform	0.446	0.455	0.446	0.902	0.471

	Mortality rate- Women under 30				
	1964-66 Partially affected	Eliminate 1965-66	1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	0.049	0.048**	0.065	0.052**	0.051***
	(0.014)	(0.016)	(0.004)	(0.013)	(0.014)
	[0.230]	[0.050]	[0.156]	[0.042]	[0.000]
Treated* Post Reform	-0.032	-0.032	-0.048**	-0.036*	-0.035
	(0.018)	(0.023)	(0.016)	(0.019)	(0.021)
	[0.183]	[0.185]	[0.043]	[0.100]	[0.115]
Post Reform	0.113*				
	(0.061)				
	[0.090]				
Observations	480	288	256	5,440	320
R^2	0.824	0.631	0.637	0.163	
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.385	0.389	0.385	0.402	0.390
Std. dev. pre-reform	0.119	0.115	0.119	0.481	0.118

Notes: The dependent variables are the mortality rate (number of men/women that died divided by the total number of men/women born in each cohort and treatment) of (Table A) men between the ages of 14 and 29, and (Table B) women between the ages of 14 and 29. All dependent variables are multiplied by 1,000. Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66, (4) include regional FE, and (5) include age FE. All regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Table 8: Robustness Check: Mortality Rate among Men and Women under 30 due to External Causes

	Death rate- Men, less 30, External Cause				
	1964-66 Partially affected	Eliminate 1965-66	Eliminate 1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	0.083	0.080**	0.098	0.083**	0.081***
	(0.021)	(0.020)	(0.015)	(0.015)	(0.016)
	[0.227]	[0.048]	[0.113]	[0.033]	[0.000]
Treated* Post Reform	-0.066*	-0.078**	-0.095**	-0.080***	-0.079***
	(0.026)	(0.024)	(0.020)	(0.019)	(0.021)
	[0.099]	[0.013]	[0.011]	[0.006]	[0.005]
Post Reform	0.1561				
	(0.115)				
	[0.177]				
Observations	480	288	256	5,440	320
R^2	0.778	0.712	0.712	0.336	0.906
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.650	0.626	0.614	0.702	0.647
Std. dev. pre-reform	0.276	0.268	0.261	0.674 0.281	

	Death rate- Women, less 30, External Cause				
	1964-66 Partially affected	Eliminate 1965-66	Eliminate 1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	0.024	0.025*	0.030	0.027**	0.027***
	(0.009)	(0.008)	(0.009)	(0.006)	(0.007)
	[0.229]	[0.052]	[0.148]	[0.043]	[0.000]
Treated* Post Reform	-0.019	-0.019*	-0.024**	-0.021**	-0.021**
	(0.010)	(0.011)	(0.012)	(0.009)	(0.010)
	[0.111]	[0.099]	[0.043]	[0.039]	[0.047]
Post Reform	0.161				
	(0.115)				
	[0.177]				
Observations	480	288	256	5,440	320
R^2	0.669	0.367	0.382	0.096	0.605
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.156	0.158	0.156	0.172	0.156
Std. dev. pre-reform	0.0565	0.0600	0.0565	0.316	0.0577

Notes: The dependent variables are the mortality rate (number of men/women that died divided by the total number of men/women born in each cohort and treatment) of (Table A) men between the ages of 14 and 29 due to external causes, and (Table B) women between the ages of 14 and 29 due to external causes. All dependent variables are multiplied by 1,000. Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66, (4) include regional FE, and (5) include age FE. All regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Table 9: Robustness Check: Mortality Rate among Women over 30

	Mortality rate- Women over 30				
	1964-66 Partially affected	Eliminate 1965-66	1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	0.004	0.008	-0.000	0.014	0.012
	(0.022)	(0.021)	(0.027)	(0.016)	(0.017)
	[0.676]	[0.814]	[0.957]	[0.5428]	[0.530]
Treated* Post Reform	0.052**	0.052**	0.060**	0.046**	0.048**
	(0.022)	(0.023)	(0.028)	(0.018)	(0.020)
	[0.032]	[0.037]	[0.050]	[0.031]	[0.021]
Post Reform	0.045				
	(0.084)				
	[0.649]				
Observations	480	288	256	5,440	320
R^2	0.895	0.811	0.813	0.272	0.841
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.770	0.762	0.770	0.753	0.751
Std. dev. pre-reform	0.175	0.179	0.175	0.678	0.179

Notes: The dependent variable is the mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) of women between the ages of 30 and 45 multiplied by 1,000. Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66, (4) include regional FE, and (5) include age FE. All regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. Robust standard errors clustered at cohort level in parentheses, and the p-value of the wild bootstrap with 1,000 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Mortality registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Table 10: Robustness Check: Mortality Rate among Women over 30 due to HIV or Diseases of the Nervous and Circulatory System

	Mortality rate- Women over 30, HIV				
	1964-66 Partially affected	Eliminate 1965-66	1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	-0.010	-0.010*	-0.014	-0.008***	-0.009**
	(0.006)	(0.005)	(0.006)	(0.004)	(0.005)
	[0.229]	[0.068]	[0.207]	[0.001]	[0.036]
Treated* Post Reform	0.012*	0.015**	0.019**	0.010	0.011*
	(0.006)	(0.008)	(0.008)	(0.006)	(0.006)
	[0.090]	[0.037]	[0.027]	[0.111]	[0.083]
Post Reform	-0.020				
	(0.021)				
	[0.421]				
Observations	480	273	241	5,440	320
R^2	0.691	0.846	0.835	0.229	0.841
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.101	0.0972	0.101	0.0947	0.0945
Std. dev. pre-reform	0.0838	0.0827	0.0838	0.236	0.0791

	Mortality rate- Women over 30, Diseases of the nervous & circulatory system				
	1964-66 Partially affected	Eliminate 1965-66	1964-66	Including Region FE	Including Age FE
	(1)	(2)	(3)	(4)	(5)
Treated	-0.006	-0.004	-0.007	-0.004	-0.005
	(0.003)	(0.003)	(0.003)	(0.003) (0.003)	
	[0.274]	[0.349]	[0.191]	[0.124]	[0.149]
Treated* Post Reform	0.011**	0.013**	0.016**	0.013***	0.014***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
	[0.013]	[0.013]	[0.011]	[0.005]	[0.004]
Post Reform	-0.010				
	(0.013)				
	[0.519]				
Observations	480	288	256	5,440	320
R^2	0.631	0.484	0.488	0.068	0.505
Calendar Year FE	YES	YES	YES	YES	YES
Cohort Year FE	YES	YES	YES	YES	YES
Region FE	NO	NO	NO	YES	NO
Age FE	NO	NO	NO	NO	YES
Mean pre-reform	0.101	0.101	0.101	0.103	0.100
Std. dev. pre-reform	0.0453	0.0446	0.0453	0.223	0.0439

Notes: The dependent variables are women's mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 30 and 45 due to (Table A) HIV, and (Table B) diseases of the nervous and circulatory system. All dependent variables are multiplied by 1,000. Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66, (4) include regional FE, and (5) include age FE. All regressions include cohort time, and calendar year dummies. Treated individuals are those born from March to May, and the control are those born from August to October. 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*: Mortality registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

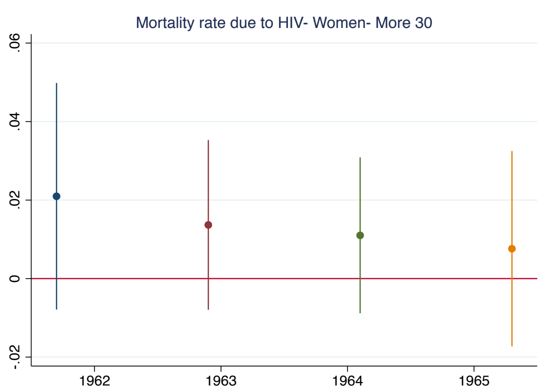
Figure 3: PLACEBOS

(a) Mortality rate for men under 30

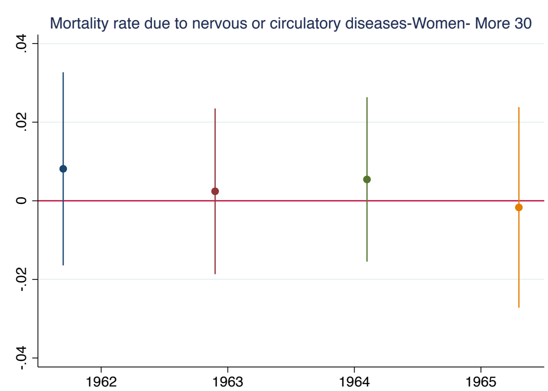
(b) Mortality rate for women over 30

(c) Mortality rate for men under 30 due to external causes

(d) Mortality rate for women under 30 due to external causes



(e) Mortality rate for women over 30 due to HIV



(f) Mortality rate for women over 30 due to diseases of the nervous/circulatory system

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, 1964 and 1965. We only consider cohorts not affected by the real reform: 1961-1965. Treated individuals are those born from March to May, and the control are those born from August to October. Source: Mortality registries (1975-2016), all men and women from the 1961-1965 cohorts.

6 Data Appendix

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

Mortality Statistics

This database contains administrative data from death certificates for the universe of individuals who died in Spain between 1975 and 2016. These data have been obtained from the Spanish National Institute of Statistics. The death certificate is completed by the doctor who certifies the death in the part relating to personal data and the cause of death. The Civil Registry in which the death is registered completes the data related to the recording and the declarant or relatives, and the data on the deceased's residence, nationality and profession. In the case of deaths that occur in special circumstances and in which a court intervenes, the information is completed by the court.

The raw microdata contain 14,540,881 deaths. We then restrict the sample to births of Spanish individuals born between 1961 and 1971 and aged 14-45 at the time of death. We also discard individuals born in 1966, and who therefore turned 14 the year the reform took place (1980), and those individuals born in January, February, June, July, November, and December. Thus, we finally have a total of 107,761 deaths in our sample.

Here we define the main dependent variables used throughout the paper, and whose descriptive statistics can be found in Table A1, A2 and A3:

- **Mortality rate of men/women aged 14 -45**. We first collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), and year of death (1975-2016). We obtain 640 cells. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the result by 1,000.

- **Mortality rate of men/women aged 14-29**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), and year of death (1975-2000). We obtain 320 cells. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the result by 1,000.

- **Mortality rate of men/women aged 30-45**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), and year of death (1991-2016). We obtain 320 cells. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the result by 1,000.

- **Mortality rate of men/women aged 14-45 by cause of death**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), year of death (1975-2016), and cause of death (ten categories for men, eight categories for women). We obtain 6,400 cells for women, and 4,960 for men. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the result by 1,000.

- **Mortality rate of men/women aged 14-29 by cause of death**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), year of death (1975-2000), and cause of death (ten categories for men, eight categories for women). We obtain 3,200 cells for women, and 2,480 for men. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the result by 1,000.

- **Mortality rate of men/women aged 30-45 by cause of death**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), treatment (treated and control), year of death (1991-2016), and cause of death (ten categories for men, eight categories for women). We obtain 3,200 cells for women, and 2,480 for men. We then divide the number of deaths by the number of individuals born in each cohort and treatment. Finally, we multiply the results by 1,000.

We examine ten different causes of death:

- **Infections and blood diseases**, including infectious intestinal diseases, tuberculosis, meningococcal disease, septicemia, and viral hepatitis.

- **HIV and AIDS**

- **Tumors**, including malignant tumors located in different parts of the body.

- **Female tumors**, including malignant tumors of the breast, cervix, and ovary.

- **Endocrine, nutritional and metabolic diseases**, including mellitus diabetes and similar.

- **Diseases of the nervous and circulatory system**, including meningitis, Alzheimer's, chronic rheumatic cardiac diseases, hypertensive diseases, acute myocardial infarction, ischemic diseases of the heart, heart failure, cerebrovascular diseases, atherosclerosis, and diseases of the blood vessels.

- **Diseases of the respiratory system**, including influenza, pneumonia, chronic diseases of the lower respiratory tract, asthma, and respiratory insufficiency.
- **Diseases of the digestive and urinary system**, including stomach ulcer, enteritis, non-infectious colitis, intestinal vascular disease, cirrhosis, kidney diseases, and diseases of the genital organs.
- **Diseases related to pregnancy, delivery and post-partum period**.
- **External causes of mortality**, including deaths due to road accidents, accidental falls, drowning, accidents with fire, accidental poisoning, suicide, physical violence, and health-care complications.

Survey on Health and Sexual Habits

The Health and Sexual Habits Survey was conducted by the Spanish National Institute of Statistics in 2003. The objective was to obtain data on the frequency of sexual conduct related to the risk of HIV infection, on the prevention measures adopted by the population in a new sexual relationship, and on people's opinions and attitudes toward HIV/AIDS infection, their transmission mechanisms, and the measures for preventing them.

The initial sample consisted of approximately 13,600 individuals within the 18-49 age group distributed in 1,700 census sections. We restrict the sample to Spanish individuals born between 1960-1965 and 1967-1972, and those individuals born in January, February, June, July, November, and December. Thus, our final sample consists of 2,044 individuals.

Here we define the dependent variables used in Section 3.1, whose descriptive statistics can be found in Table A4:

- **Alcohol consumption alcohol, daily**: A dummy variable that is equal to one if the individual drinks daily, and zero otherwise.
- **Alcohol consumption alcohol, $>$ twice per week**: A dummy variable that is equal to one if the individual drinks at least twice a week, and zero otherwise.
- **Injectable drugs**: A dummy variable that is equal to one if the individual has ever used injectable drugs, and zero otherwise.
- **Total number of sexual partners**: Total number of sexual partners that the individual has had until this moment.

- **HIV tested**: A dummy variable that is equal to one if the individual has ever been tested for HIV, and zero otherwise.
- **Knows the results of HIV test**: A dummy variable that is equal to one if the individual has collected the results of the HIV test, and zero otherwise.

Appendix Tables and Figures

Table A1: Descriptive Mortality Statistics

	Treatment group					Control group				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Mortality rate of men aged 14-45	320	1.37	0.48	0.30	2.69	320	1.28	0.48	0.22	2.53
Mortality rate of women aged 14-45	320	0.55	0.22	0.14	1.25	320	0.51	0.22	0.11	1.39
Mortality rate of men aged 4-29	160	1.13	0.43	0.30	2.14	160	1.05	0.45	0.22	1.96
Mortality rate of men aged 30-45	160	1.61	0.41	0.85	2.69	160	1.51	0.39	0.79	2.53
Mortality rate of women aged 14-29	160	0.40	0.11	0.14	0.68	160	0.37	0.11	0.11	0.67
Mortality rate of women aged 30-45	160	0.70	0.21	0.27	1.25	160	0.66	0.21	0.25	1.39

Source: Mortality registries (1975-2016), all Spanish men and women from the 1961-1971 cohorts, except the 1966 cohort. Treated individuals are those born from March to May, and the control are those born from August to October.

Table A2: Descriptive Mortality Statistics for Individuals aged 14-29

	Treatment group					Control group				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Mortality rate of men aged 14-29 due to infectious and blood diseases	160	0.03	0.04	0.00	0.25	160	0.03	0.04	0.00	0.34
Mortality rate of men aged 14-29 due to HIV	160	0.09	0.16	0.00	0.72	160	0.07	0.13	0.00	0.56
Mortality rate of men aged 14-29 due to tumors	160	0.08	0.03	0.00	0.16	160	0.08	0.03	0.02	0.19
Mortality rate of men aged 14-29 due to endocrine, nutritional and metabolic diseases	160	0.01	0.01	0.00	0.04	160	0.00	0.01	0.00	0.02
Mortality rate of men aged 14-29 due to diseases of the nervous and circulatory system	160	0.10	0.04	0.01	0.22	160	0.09	0.04	0.02	0.22
Mortality rate of men aged 14-29 due to diseases of the respiratory system	160	0.04	0.03	0.00	0.18	160	0.04	0.03	0.00	0.16
Mortality rate of men aged 14-29 due to diseases of the digestive and urinary system	160	0.02	0.02	0.00	0.10	160	0.02	0.02	0.00	0.09
Mortality rate of men aged 14-29 due to external causes	160	0.69	0.27	0.12	1.38	160	0.65	0.31	0.07	1.34
Mortality rate of women aged 14-29 due to infectious and blood diseases	160	0.02	0.02	0.00	0.12	160	0.02	0.02	0.00	0.09
Mortality rate of women aged 14-29 due to HIV	160	0.03	0.06	0.00	0.26	160	0.03	0.05	0.00	0.26
Mortality rate of women aged 14-29 due to tumors	160	0.06	0.03	0.00	0.13	160	0.05	0.03	0.00	0.11
Mortality rate of women aged 14-29 due to female tumors	160	0.01	0.01	0.00	0.05	160	0.01	0.01	0.00	0.05
Mortality rate of women aged 14-29 due to endocrine, nutritional and metabolic diseases	160	0.00	0.01	0.00	0.02	160	0.00	0.01	0.00	0.04
Mortality rate of women aged 14-29 due to diseases of the nervous and circulatory system	160	0.05	0.03	0.00	0.16	160	0.05	0.03	0.00	0.12
Mortality rate of women aged 14-29 due to diseases of the respiratory system	160	0.02	0.01	0.00	0.06	160	0.02	0.01	0.00	0.08
Mortality rate of women aged 14-29 due to diseases of the digestive and urinary system	160	0.01	0.01	0.00	0.05	160	0.01	0.01	0.00	0.05
Mortality rate of women aged 14-29 due to pregnancy, delivery and post-partum period	160	0.00	0.01	0.00	0.02	160	0.00	0.00	0.00	0.01
Mortality rate of women aged 14-29 due to external causes	160	0.17	0.06	0.02	0.35	160	0.15	0.06	0.02	0.35

Source: Mortality registries (1975-2000), all Spanish men and women from the 1961-1971 cohorts, except the 1966 cohort. Treated individuals are those born from March to May, and the control are those born from August to October.

Table A3: Descriptive Mortality Statistics for Individuals aged 30-45

	Treatment group					Control group				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Mortality rate of men aged 30-45 due to infectious and blood diseases	160	0.04	0.02	0.00	0.10	160	0.04	0.03	0.00	0.11
Mortality rate of men aged 30-45 due to HIV	160	0.26	0.26	0.01	1.25	160	0.23	0.24	0.01	1.07
Mortality rate of men aged 30-45 due to tumors	160	0.27	0.16	0.06	0.70	160	0.25	0.15	0.06	0.72
Mortality rate of men aged 30-45 due to endocrine, nutritional and metabolic diseases	160	0.01	0.01	0.00	0.07	160	0.01	0.01	0.00	0.06
Mortality rate of men aged 30-45 due to diseases of the nervous and circulatory system	160	0.24	0.12	0.06	0.57	160	0.23	0.10	0.07	0.48
Mortality rate of men aged 30-45 due to to diseases of the respiratory system	160	0.07	0.03	0.01	0.18	160	0.06	0.03	0.00	0.19
Mortality rate of men aged 30-45 due to diseases of the digestive and urinary system	160	0.11	0.07	0.01	0.38	160	0.10	0.06	0.01	0.29
Mortality rate of men aged 30-45 due to external causes	160	0.50	0.13	0.26	0.91	160	0.48	0.13	0.19	1.03
Mortality rate of women aged 30-45 due to infectious and blood diseases	160	0.02	0.01	0.00	0.06	160	0.01	0.01	0.00	0.06
Mortality rate of women aged 30-45 due to HIV	160	0.06	0.06	0.00	0.34	160	0.07	0.07	0.00	0.36
Mortality rate of women aged 30-45 due to tumors	160	0.17	0.09	0.02	0.41	160	0.16	0.08	0.03	0.43
Mortality rate of women aged 30-45 due to female tumors	160	0.13	0.08	0.00	0.35	160	0.12	0.08	0.01	0.36
Mortality rate of women aged 30-45 due to endocrine, nutritional and metabolic diseases	160	0.01	0.01	0.00	0.04	160	0.01	0.01	0.00	0.05
Mortality rate of women aged 30-45 due to diseases of the nervous and circulatory system	160	0.09	0.04	0.01	0.24	160	0.09	0.04	0.01	0.23
Mortality rate of women aged 30-45 due to to diseases of the respiratory system	160	0.02	0.02	0.00	0.11	160	0.02	0.02	0.00	0.08
Mortality rate of women aged 30-45 due to diseases of the digestive and urinary system	160	0.04	0.03	0.00	0.12	160	0.04	0.03	0.00	0.15
Mortality rate of women aged 30-45 due to pregnancy, delivery and post-partum period	160	0.00	0.00	0.00	0.02	160	0.00	0.01	0.00	0.02
Mortality rate of women aged 30-45 due to external causes	160	0.11	0.04	0.02	0.21	160	0.10	0.04	0.02	0.20

Source: Mortality registries (1991-2016), all Spanish men and women from the 1961-1971 cohorts, except the 1966 cohort. Treated individuals are those born from March to May, and the control are those born from August to October.

Table A4: Descriptive Statistics of the Survey on Health and Sexual Habits

	Treatment group					Control group				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Consumption of alcohol, daily	1041	0.14	0.35	0.00	1.00	1003	0.14	0.35	0.00	1.00
Consumption of alcohol, $>$ twice per week	1041	0.31	0.46	0.00	1.00	1003	0.33	0.47	0.00	1.00
Injectable drugs	1041	0.02	0.14	0.00	1.00	1003	0.02	0.16	0.00	1.00
Total number of sexual partners	1018	2.57	1.73	1.00	9.00	981	2.52	1.71	1.00	9.00
HIV tested	1041	0.29	0.45	0.00	1.00	1003	0.28	0.45	0.00	1.00
Knows the results of HIV test	1041	0.27	0.45	0.00	1.00	1003	0.27	0.44	0.00	1.00

Source: Survey on Health and Sexual Habits (2003), all Spanish men and women from the 1960-1972 cohorts, except the 1966 cohort. Treated individuals are those born from March to May, and the control are those born from August to October.

Table A5: Effect of the Reform on Men's Health Habits

	Consumption alcohol daily		Consumption alcohol more 2 week		Injectable drugs		Total number sexual partners		VIH test ever		Knows the results of VIH test	
	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72	1961-71	1960-72
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	-0.012	-0.002	-0.086	-0.084	0.032**	0.024	0.037	0.047	0.036*	-0.005	0.021	-0.016
	(0.029)	(0.026)	(0.051)	(0.042)	(0.012)	(0.013)	(0.088)	(0.078)	(0.013)	(0.041)	(0.009)	(0.037)
	[0.581]	[0.955]	[0.231]	[0.128]	[0.039]	[0.180]	[0.671]	[0.564]	[0.075]	[0.940]	[0.161]	[0.937]
Treated* Post Reform	-0.011	-0.028	0.072	0.087	-0.041	-0.037*	-0.140	-0.153	0.035	0.028	0.039	0.036
	(0.051)	(0.043)	(0.063)	(0.056)	(0.021)	(0.020)	(0.136)	(0.116)	(0.031)	(0.067)	(0.029)	(0.060)
	[0.833]	[0.523]	[0.264]	[0.134]	[0.103]	[0.079]	[0.334]	[0.223]	[0.309]	[0.709]	[0.169]	[0.566]
Observations	767	929	767	929	767	929	755	917	767	929	767	929
R^2	0.053	0.047	0.059	0.052	0.031	0.027	0.052	0.044	0.041	0.031	0.038	0.031
Cohort Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.169	0.169	0.341	0.341	0.0236	0.0236	2.491	2.491	0.248	0.248	0.232	0.232
Std. dev. pre-reform	0.375	0.375	0.474	0.474	0.152	0.152	1.782	1.782	0.432	0.432	0.422	0.422

Notes: The dependent variables are (1-2) the probability of consuming alcohol daily, (3-4) the probability of consuming alcohol more than twice a week, (5-6) the probability of ever having used injectable drugs, (7-8) the total number of sexual partners, (9-10) the probability of having ever tested for HIV, and (11-12) the probability of knowing the results of the HIV test. Regressions 1, 3, 5, 7, 9, and 11 take into account the cohorts of women born from 1961 to 1971, while regressions 2, 4, 6, 8, 10 and 12 include the cohorts of women born from 1960 to 1972. Regressions include cohort time, and regional dummies. Treated individuals are those born from March to May, and the control are those born from August to October. 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*: Survey on Health and Sexual Habits (2003), all men from cohorts 1960-1965 and 1967-1972.