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Unintended Health Costs of Gender Equalization *

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Abstract

In 1980, a few years after the democratization process, Spain raised the minimum working age from 14 to 16, while the compulsory education age remained at 14. This reform changed the within-cohort incentives to remain in the educational system. Using a difference-in-differences approach, we analyze the gender asymmetries in mortality generated by this change. The reform, through its effects on education, decreased mortality at ages 14-29 among men (6.4%) and women (8.9%), mainly from the reduction in deaths due to traffic accidents. However, the reform also increased mortality for prime-age women (30-45) by 7%. This last effect is driven by increases in HIV mortality, as well as by diseases related to the nervous and circulatory system. We show that health habits of women deteriorated as a consequence of the reform, but this was not the case for men. The gender differences in the impact of education on smoking and drinking are driven by the gender equalization process that the affected women were experiencing when the reform took place. All in all, these patterns help explain the narrowing age gap in life expectancy between women and men in many developed countries while, at the same time, they provide important policy implications for middle income countries that are undergoing those gender equalization process right now.

JEL Codes: I12, I20, J10

Keywords: minimum working age, education, mortality, gender equalization

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1 Introduction

In general, women have lower mortality rates and 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 1990s to 2017. In 1991 the difference in mortality rates between men and women at age 14 was 0.1 deaths per thousand individuals. However, by 2017, this difference had disappeared. This pattern is even more pronounced at older ages. Whereas in 1991, the difference in mortality rates between men and women at age 30 or 45 was two deaths per 1,000 individuals, in 2017 this difference was only 0.5 per 1,000 individuals.

The evolution of the gender gap in mortality across time and countries indicates that the gap cannot be fully explained by biological reasons[2]. 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, hazardous driving, and occupational risks (Loef and Walach, 2012)[3].

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). The smoking epidemic model describes that men were the first to take up smoking, while women began to smoke later in time. Because of this lag, there is a phase when the proportion of males dying from smoking begins to decline, but the proportion of females continues to rise (Wensink et al., 2020; Lopez et al., 1994; Thun et al., 2012). Preston and Wang (2006) estimate that changes in smoking patterns contributed to around 20% of the declining gender mortality gap. Wensink et al. (2020) show that, in high-income regions, smoking explained up to 50% of sex differences in life expectancy between ages 50 and 85 from 1950 to 2015. They also indicate that the reduction of these gender differences since the 1980s is driven by smoking-attributable mortality, which tended

[1]Source: OECD Health Statistics, 2016.

[2]Though, biologically, women are more likely to suffer from acute illness and nonfatal chronic conditions (arthritis, constipation, thyroid conditions, gall bladder conditions, headaches and migraines) while men are more likely to suffer from life-threatening chronic diseases (coronary heart disease, cancers, cerebrovascular disease, emphysema, liver cirrhosis and kidney disease) (Bird and Rieker, 1999; Case and Paxson, 2005). Hormonal, autoimmune and genetic factors can explain these gender differences (Oksuzyan et al., 2008; Schünemann et al., 2017).

[3]Women also used to eat better[4] and use health care services more often than men(Sindelar, 1982; Schünemann et al., 2017).

to decline in males while increasing in females overall.

The reason why women took up smoking or drinking much later is due to these behaviors being considered taboo for females for a very long time. As women entered the labor force and had access to better economic opportunities, smoking or drinking became acceptable and adopted first by the most successful women as a symbol of independence (Amos and Haglund, 2000). Thus, during the gender equalization process, more successful women (that is, more educated) undertook more unhealthy behaviors, despite their health cost. In the specific case of Spain, in Figure 2 we observe the negative gradient between education and unhealthy behaviors (smoking and drinking) for women born between 1944 and 1964. While less than 40% of women with elementary education consume alcohol, around 70% of women with tertiary education engage in such activities. From another angle, the same can be observed in terms of the percentage of those who have never smoked: 70% of women with elementary education, in contrast with only 30% of women with tertiary education. On the other hand, for men, smoking and drinking don't seem to be correlated with education.

Thus, it would seem that the new roles and statutes for more educated women during the gender equalization process may have had adverse effects on their health relative to men. In other words, the gender equalization process that developed countries experienced during the 1970s could be one important factor contributing to the narrowing of the gender gap in mortality.

In this paper, our aim is to assess whether increases in education during the process of gender equalization and women's greater access to economic opportunities are able to explain, at least in part, the narrowing gender mortality gap. 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 difference-in-differences strategy to identify the reform's within-cohort effects, where our treated and control individuals will differ only in their month of birth.

The child labor reform of 1980 encouraged individuals to stay in the educational system depending on 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,[5] while individuals

[5] In the Spanish educational system, all children from the same cohort starte 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.

born at the end of the year reached the legal working age only after completing this final year of education. 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 at 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.[6]

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 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 11% and 7.4%, respectively. At the same time, the reform decreased the number of treated girls (boys) not attaining optional secondary education by 2.7% (3.6%). These results show that restricting child labor effectively increased the educational attainment of the individuals affected.

This paper 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, through its effects on education, reduced the mortality rate among young men (aged 14-29) by 0.07 per 1,000 men in that age bracket. This corresponds to a 6.4% decrease in their mortality rate at this age. This reduction is entirely driven by a 17% decrease in deaths due traffic accidents. We also show that there is a 21% drop in the mortality rate due to traffic accidents among young women. This result is consistent with evidence in the literature suggesting that education increases risk aversion (Jung, 2015), which could have reduced reckless driving behavior. Similarly, previous studies have also shown that increases in the length of compulsory education result in significant reductions in accidents (Lager and Torssander, 2012; Grytten et al., 2020).

Surprisingly, we also find that the mortality rate among prime-age (30-45) treated women increased by 0.052 per 1,000 individuals (or 7%). When analyzing this increase in detail, we find that this effect is driven by an increase in the mortality rate due to HIV or AIDS (13.3%), and diseases related to the nervous and circulatory system (13.2%), in particular, increases in the probability of dying due to hypertensive or ischemic disease, hearth failure and influenza. This last finding could be a result of more educated women engaging in less healthy habits during the gender equalization process. In fact, we find that women affected by the reform had an 11.2 percentage point higher

[6] 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

probability of having been tested for HIV, a 7.9 percentage point higher probability of smoking, a 6.8 percentage point lower probability of never having smoked, an increase by 5 percentage points in the probability of drinking daily, and an 11.5 percentage point lower probability of drinking less than once a month. On the other hand, we do not find any increase in the probability of engaging in these risky behaviors for men. This effect is related to the social context in Spain at the time of the reform.

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 had 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.[7] 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% of working-age women in Spain participated in the labor market, increasing slightly to 34.5% in 1985 (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. At that time, as we can also observe in Figure 2, 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

[7]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.

gender equality and women's greater access to economic opportunities. A large part of previous literature has either focused solely on men (Van Kippersluis et al., 2011; Cipollone and Rosolia, 2011), or 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).[8] Three previous papers have reported the differential effects of gender 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 indicate that a potential mechanism relies on the behaviors acquired and existing risk factors 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 are aware of, this is the first paper to investigate the effect of education on adult mortality using a child labor regulation. 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 with this extensive literature, there is a lack of consensus on the direction and magnitude of the impact.[9] 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)[10]. Child labor reforms differ from compulsory

[8] 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.

[9] 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.

[10] 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.

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 more likely lead to the increase in educational attainment of children whose main motivation to drop out relates to 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 have differing impacts on health and mortality in countries with different levels of development, this paper sheds light on a reform that affected what was considered 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-differences estimator, we do not rely on the assumption that individuals born in different months are equal. The only assumption we are making is that any existing differences between those born at the beginning and at the end of the year remain constant for the cohorts before and after the reform.

The remainder of the paper is organized as follows: Section 2 introduces the reform and the identification strategy; Section 3 presents the effects of the reform on mortality rates and explores the mechanisms that can explain the reported effects on mortality; Section 4 covers the results of vari-

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.

ous robustness checks performed; while Section 5 concludes with a discussion of the main results and their policy implications.

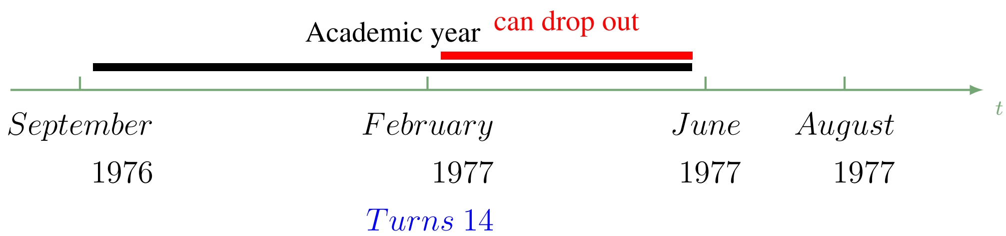
2 Institutional Context and Identification Strategy

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. This meant only individuals born after 1966, and who were 14 or over at the time the reform was passed, were affected by it. For our identification strategy, we compare individuals born before 1966 to those born after.

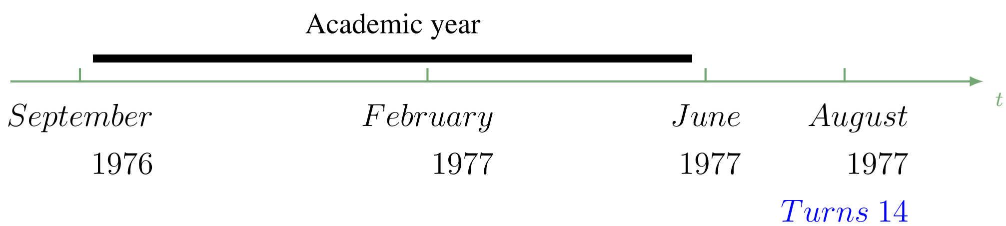
This reform also generated different incentives depending on each individual's birth month due to the norms of the Spanish educational system and the compulsory schooling age that was maintained at 14 until 1990. In Spain, all children from the same cohort start school the same year. This means that some children are six years old, while others are still five when they start school. As a result, 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. Therefore, before the reform, 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. So, 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 eliminated these differences in incentives. All individuals, regardless of their birth month, had the same incentives to finish primary education, since they could not work until they were 16.

In order to illustrate the different incentives for remaining in the educational system, the following chart shows 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 incentive 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 July) or in the last part of the year (from August to December). With the reform, this difference no longer existed.

2.1 Identification strategy

We use the exogenous change in the incentives introduced by the ET reform to identify the causal effect of education 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 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 because this reform was approved during a period of significant social change in Spain.

Formally, we consider the following econometric model:

$$Mortality\ Rate_{myt} = \beta_0 + \beta_1 Treated + \beta_2 Treated * Post\ Reform + \gamma_m + \gamma_y + \gamma_t + \epsilon_{myt}$$

Our main outcome of interest ($Mortality\ Rate_{myt}$) is the mortality rate of individuals born in month m and year y observed in year t (year of death or year of interview, depending on the data used). We construct this outcome using the mortality registries obtained from the Spanish National Institute of Statistics from 1975 until 2016[11]. We collapse the individual data at the level of year of birth, month of birth, and year of death. We then classify the number of deaths by the number of individuals born in each month and year (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[12]. $Post\ Reform$ 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[13]. We also include month of birth (γ_m), year of birth (γ_y) and year of death or year of interview (γ_t) fixed effects. We cluster the standard errors at cohort level, and we report them in parenthesis. We also perform a wild bootstrap with 999 repetitions, and we report the p-values in brackets.

The effect of the reform can be identified by the coefficient of the interaction term 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.[14]

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).

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

[12] Results are mostly robust when we compare individuals born between January and July with individuals born between August and December. Please see Tables 4, 5, A10, A11, A12, and A13.

[13] In Section 4.1, we relax the assumption that the 1964 and 1965 cohorts were not affected at all by the reform.

[14] These results are available upon request.

With this identification strategy, we are assuming that the reform did not have any effect for the 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 jobs when the reform was enacted. This is a major assumption that we will relax in Section 4.1.

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 increased the probability of finishing primary education by 7.6% in the case of men and 11% in the case of women. The reform also increased post-compulsory education. In particular, it decreased the number of treated women (men) not attaining secondary post-compulsory education by 2.7% (3.3%). We then analyze the effects of the reform on the mortality rates of affected individuals.

Del Rey et al. (2018) also reports some significant long-run labor market effects of the reform. They show that working accidents fell for both men and women (although there is no effect on deadly working accidents) as a consequence of the increase in educational attainment. They also provide evidence that the reform increased wages and the probability of working, and that it decreased the probability of working in the low-skilled sector for men. However, they do not find any significant labor market effects for women, perhaps due to the existence of the strong bias against women working outside the home at that time in Spain.

(Bellés-Obrero et al., 2015) find that this same reform decreased marriage and fertility rates and deteriorated infant health outcomes mainly due to the prolongation of age at first birth. However, Bellés-Obrero et al. (2019) show that more educated parents were able to reverse these negative shocks on their children's health at birth through increased parental vigilance in the long run.

3 Effect of the Reform on Mortality

This section explores whether the reform, through its effect on education, had any impact on 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, since the coefficient of the interaction term is not significant, it would indicate that the reform did not affect the mortality rate for the treated group.

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)[15]. The mortality variable we use is age-specific, as the policy may have affected mortality differently among younger and older individuals. Before addressing the regression results, Figure 3 reports the raw data and the predictions from the estimation model for women and men in the treatment and control groups for the 1961-1971 cohorts[16]. 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 between ages 14 and 29 compared to another man or woman born at the end of the same year. However, this difference narrowed after the reform was implemented. On the other hand, as shown in graph (b), the difference in the mortality rates of men aged 30-45 born at the beginning and at the end of the year did 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, but the gap grew for the cohorts affected by the reform.

Column (2) in Table 1 shows that the reform results in a decrease in the mortality rate of young men in the treated group (aged 14-29) by 0.07 per 1,000 men. This corresponds to a 6.4% decrease with respect to the pre-reform mean. In a similar way, in column (4), we can observe that the reform also seems to decrease the mortality rate of young women in the treated group by 8.9%,[17] although the coefficient is marginally nonsignificant (the p-value is 0.107).

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[18], while it significantly increased these rates among prime-age affected women. In particular, column (6) shows that the reform increased the mortality rate of women aged 30-45 by 0.052 per 1,000 women, or by 7% in comparison with 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 these varying effects in mortality rates for men and women.

[15] In Figure A2 we replicate our main results using different age brackets.

[16] The same graphs but showing men's and women's mortality rate by cause of death can be seen in the Appendix in Figure A1.

[17] Note that the pre-reform mortality rate for young individuals differs greatly between genders. There is a mortality rate of 1.1 per 1,000 men (aged 14-29) before the reform, while the same rate for women of the same age is 0.39 per 1,000 women.

[18] We can reject that the reform increased the mortality rate of men by more than 0.11%.

3.1 Mechanisms: Causes of Death and Behaviors

In this section we aim to shed some light on the mechanisms explaining the impact of the reform, through its effect on education, on men and women's mortality rates. First, we explore the effect of the reform on cause-specific mortality. We consider seven different causes of death (by order of importance): 1) external causes of mortality[19] ; 2) tumors[20]; 3) diseases of the circulatory and respiratory system[21]; 4) infections and parasitic diseases[22]; 5) diseases of the nervous and digestive system[23]; 6) abnormal clinical and laboratory symptoms[24]; and 7) other causes[25]. 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).

Figure 4[26] presents the impact of the reform on men's cause-specific mortality. In Figure 4 (a), we can observe that the reform decreased men's mortality rate due to external causes by 0.079 per 1,000 individuals, or 11.7%, with respect to the pre-reform mean. External causes of mortality include traffic accidents, other types of accidents, suicides, homicides, surgical or medical complications, and other external causes. We can examine which external cause of mortality was actually reduced as a consequence of the reform. Figure 6 (a)[27] shows that the reform reduced mortality due to traffic accidents for young men by 17% in comparison with the pre-reform mean. Thus, the reduction in total mortality observed in Table 1 for young men is completely driven by the reduction in traffic accidents.

When looking at young women's cause-specific mortality, Figure 5 (a)[28] shows that the reform reduced deaths related to external causes by 0.021 per 1,000 women (13.1% with respect to the

[19]This classification includes deaths due to traffic accidents, other accidents (accidental falls, drowning, accidents with fire, or accidental poisoning), suicide, homicide, surgical and medical complications, and other type of external causes of mortality.

[20]This classification includes deaths due to malignant tumors located in different parts of the body.

[21]This classification includes deaths due to chronic rheumatic hearth diseases, ischemic diseases, acute myocardial infarction, heart failure, other hearth diseases, influenza, pneumonia, asthma, respiratory insufficiency, and other respiratory diseases.

[22]This classification includes deaths due to infectious intestinal diseases, tuberculosis, meningococcal disease, viral hepatitis, AIDS and HIV, and other infectious diseases.

[23]This classification includes deaths due to meningitis, Alzheimer's, stomach ulcer, enteritis, non-infectious colitis, and intestinal vascular diseases.

[24]This classification includes deaths due to a hearth attack or other abnormal clinical and laboratory symptoms.

[25]This classification includes deaths due to other causes that have not been mentioned above.

[26]Tables A1 and A2 in the appendix report the corresponding detailed regression results.

[27]Table A5 in the appendix reports the corresponding detailed regression results.

[28]Table A3 in the appendix reports the corresponding detailed regression results.

pre-reform mean)[29]. Similar to the case of young men, Figure 7 (b)[30] indicates that the reform reduced mortality due to traffic accidents for young women by 21%. 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; Grytten et al., 2020). One possible explanation for the reduction in accidents due to increases in education could be changes in risk-taking behaviors. There is some evidence that education can increase risk-aversion (Jung, 2015). If this is the case, an increase in educational attainment could lead to safer driving.
Figure 4 (b) reports the effect of the reform on the cause-specific mortality rate 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 of the causes of death among middle-aged treated men.

Finally, Figure 5 (b)[31] shows the results for the seven causes of mortality among women aged 30-45. We can see that the reform increased the mortality rate due to infectious or parasitic diseases by 0.012 per 1,000 treated women, or by 10.8% with respect to the pre-reform mean. Figure 7[32] shows that the increase in the mortality rate due to infectious or parasitic diseases is totally driven by increases in the probability of mortality due to HIV or AIDS. In particular, the reform increased deaths due to HIV or AIDS by 13.3%. Figure 5 (b) also shows that the reform increased the mortality rate due to diseases of the nervous or respiratory system by 0.014 per 1,000 treated women, or by 13.2%. Figure 7 illustrates that this effect is driven by increases in the probability of dying due to hypertensive or ischemic diseases, hearth failure and influenza.

Deaths due to HIV and diseases of the circulatory system are largely influenced 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. For this analysis, we first use the Survey on Health and Sexual Habits[33] conducted by the Spanish National Institute of Statistics in 2003. The survey contains information on drug consumption, and sexual behaviors. We supplement this analysis with information on alcohol and tobacco consumption collected by the Spanish National Health Survey of 2006 and 2012[34].

Table 2 reports the reform's effect on women's consumption of drugs and sexual behaviors. We

[29] We can also observe that the reform decreased mortality due to abnormal clinical and laboratory symptoms. However this effect is very small (one fifth of the effect on external causes) and only significant at the 10% level.

[30] Table A6 in the appendix reports the corresponding detailed regression results.

[31] Table A4 in the appendix reports the corresponding detailed regression results.

[32] Table A7 in the appendix reports the corresponding detailed regression results.

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

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

do not find any evidence that there is 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 pertaining to drug consumption in the survey. Neither do we find any effect on the age at which women had their first sexual relationship, nor the total number of sexual partners reported, which we use as a proxy for risky sexual behavior. Interestingly, we do find that women affected by the reform have a 11.2 percentage point (37.9%) higher probability of having been tested for HIV, and an 10.7 percentage point (38.2%) higher probability of collecting and knowing the test's result. Nevertheless, we do not observe that the reform affected men in any of these dimensions (Table A8).

Table 3 reports that, after the reform, treated women have a 7.9 percentage point (24%) higher probability of smoking and a 6.8 (17%) percentage point lower probability of never having smoked, compared to women not affected by the child labor reform. Moreover, the reform increased the probability of drinking daily by 5 percentage points (48%), and decreased the probability of drinking less than one time a month by 11.5 percentage points (28%). Again, in determining if the reform increased unhealthy behavior for men, we find null effects (Table A9).

Therefore, our results show that women affected by the reform had a higher probability of engaging in riskier 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 seem to have been significantly altered by the reform. This is consistent with the fact that the reform does not have any impact on the mortality rate of middle-aged treated men.

The observed gender differences in the impact of education on risky behaviors are driven by the gender equalization process that 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 and the female-to-male smoking ratio in Spain from the 1960s to the 2010s.

We note that this positive association between education and the prevalence of smoking and drinking among women cannot be considered as particular only for Spain. In many countries, there have been increasing numbers of women smoking and drinking, 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). (Bosdriesz et al., 2014) report a higher rate of smoking and drinking among highly educated women, compared to those with little education in some Eastern European and Eastern Mediterranean countries. This same pattern seems to hold (Pampel, 2003) in other high-income countries at early stages of the smoking epidemic. From a policy perspective, our results are therefore more relevant for developing countries whose educational systems, child labor market participation rates, and women's social development are similar to the levels that Spain had in the 1980s.

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 of death fixed effects. Thirdly, we extend our treatment and control groups to include individuals born in January, February, June and July in the treatment group, and those born in November and December in the control group. Finally, we perform some placebo tests where we change the timing of the reform.

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

The child labor 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 already 16 years old when the reform was introduced, and so were completely unaffected by it. However, individuals born between March 1966 and February 1966 were aged between 14 and 16 when the ET reform was enacted. In our previous analysis, we have assumed that these individuals were unaffected by the reform. In this section, we relax this assumption.

First, we consider these cohorts of individuals as partially affected. Using the number of months these individuals had to wait before they could start working, 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 were fully affected by the reform and had to wait two years to start working. The variable takes a value of 0 for all individuals born before

February 1964. They were not affected in any way by the reform since, having reached 16 years of age, they could already start working. The post-reform variable takes 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 thus takes a value of 1/24 for these individuals (24 being the number of months the individual would have had to wait before being able to work when the reform was passed, or an equivalent of two years). In the same way, the post-reform variable has a 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 4 and 5 show that our main results are robust when this alternative specification is used. The reform decreases the mortality rate among men aged between 14 and 29 by 0.067 per 1,000 men (instead of 0.069) and increases mortality rate among women aged 30-45 by 0.061 per 1,000 women (instead of 0.052).

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 4 and 5 indicate that the reform's effects on age and cause of specific mortality rates are very similar 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 of Death Fixed Effects

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

The fourth column in Tables 4 and 5 shows that the effects on our key results are very robust to the inclusion of regional fixed effects.

Finally, the fifth column in Tables 4 and 5 includes age at death dummies as controls. We observe similar patterns in the results using this alternative specification.

4.3 Extended Treatment and Control Status

Individuals born at the beginning of the year are typically different from individuals born at the end of the year in several dimensions (Bound and Jaeger, 2000; Buckles and Hungerman, 2013). Note, however, that we do not need this assumption for our identification strategy to be valid. Our identification strategy only requires that any existing differences between individuals born at the beginning and at the end of the year for the cohorts born before the reform stay constant for the cohorts after the reform, which may be considered a weak assumption. For this reason, we exclude individuals that are potentially more different: those born in January, February, November and December in our baseline results. We also drop individuals born in June and July because these months coincide with the end of the academic year, and these individuals were potentially not affected by the change in incentives.

Thus in this section we estimate the same baseline model but with a broader definition of our treatment and control groups. We now include individuals born from January to July in the treatment group, and those born from August to December in the control group. The sixth column in Tables 4 and 5 shows that even with this expanded sample our main findings are unchanged. Thus, we believe that this robustness check provides some suggestive evidence that any differences in individuals born at the beginning and end of the year stay constant for the cohorts affected and not affected by the reform.

4.4 Placebos

We also perform several placebo tests, assuming the reform took place in different years (prior to 1980). For this, we extend our main database to include individuals born between 1951 and 1965, excluding all cohorts that were affected by the reform. We then examine the effect of ten "fake" reforms on the cohorts from 1954 to 1963, using the same econometric specification and definition of treatment status as before. We do not perform a placebo test for the cohorts from 1964 and 1965 because, as previously discussed in Section 4.1, these cohorts could be potentially affected by the reform. We do not expect our estimation's interaction term to be significant for any of these years.

Figure 8 shows the coefficient and the 95% confidence interval of our estimation's interaction term for the results on mortality for men under 30 and mortality for women over 30. Graph (a) in Figure 8 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. We also plot the “real” effect of the reform in red so we can observe that the “real” effect is not only significant at the 5% level but also the coefficient is larger than the coefficients from the “fake” reforms. In graph (b), we plot the interaction term and 95% confidence interval on the mortality rate of middle-aged women (ages 30-45), and we see once more that the “fake” reforms effects are not significant. Moreover, Figure 9 also shows that none of the “fake” reforms have an effect on the mortality rates of young men and women due to external causes of death, nor on the mortality rates of middle-aged women due to infectious and parasitic diseases, or from diseases of the circulatory or respiratory system.

From these results, we find that the parallel assumption is fulfilled in our analysis, that there are no differences between the treatment and control in any of the previous years before the reform took place.

5 Conclusion

In this paper, we have examined the gender differences in the effect of education on mortality during a period of gender equalization, employing a quasi-natural experiment based on a new Workers Statute law enacted that increased the minimum legal working age from 14 to 16 in 1980 in Spain. Since the school leaving age remained at 14, this reform encouraged individuals to stay in the educational system depending on their year and month of birth. Before the reform, individuals born at the beginning of the year were legally entitled to work before finishing their final year of primary education, while individuals born at the end of the year reached the legal working age only after completing this final year of education. With the child labor reform, this difference in incentives disappeared. Thus, we have used a difference-in-differences strategy to identify the reform’s within-cohort effects.

Taking from Del Rey et al. (2018) that the reform increased the educational attainment for both men and women, when looking at the reform’s effects on long-term mortality rates, we have found that the reform reduced the mortality rate among young men (aged 14-29) by 6.4%. This decrease is entirely driven by a 17% decrease in deaths due traffic accidents. We have also shown that there is a 21% decrease in the mortality rate due to traffic accidents among young women. These results support previous findings in the literature showing that education increases risk aversion (Jung, 2015) as well as with previous studies pointing towards reductions in accidents due to increases in the length of compulsory education (Lager and Torssander, 2012; Grytten et al., 2020).

Peculiarly, however, we have also found that the mortality rate among prime-age (30-45) women in the treated group increased by 7%. On deeper analysis, the effect is driven by an increase in the mortality rate due to HIV or AIDS (13.3%), and diseases of the nervous and circulatory system (13.2%). This may be pointing to an interesting phenomenon that more educated women may be engaging in less healthy habits during the gender equalization process. In fact, our study has indicated that women affected by the reform have a higher probability of drinking, smoking, and having been tested for HIV. Conversely, we have not seen any increase in the probability of engaging in these risky behaviors for men.

This effect must be taken within the social context in Spain at the time of the reform, just a few years after the end of Franco's dictatorship that lasted almost 40 years. During this time, the country's levels of educational attainment, child labor, and women's social development were closer to those of a middle-income country. It must be noted that during the dictatorship, Spain was a male-dominated society. The end of the dictatorship raised the level of gender equality and improved women's access to economic opportunities (Philips, 2010). This gender equalization process then led to a convergence of health risk factors between men and women.

Taken 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. At the same time, our results are also informative for other middle income countries that are undergoing those gender equalization process right now or will do so in the near future. Thus, some protective policies for women in those countries should be designed in order to avoid the deterioration of their health habits and the preventable increase in their mortality rates. In this way, women in those countries can benefit from the gender equalization process at minimal costs. 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).

Compliance with Ethical Standards:

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- *Conflict of Interest*: The authors declare that they have no conflict of interest.

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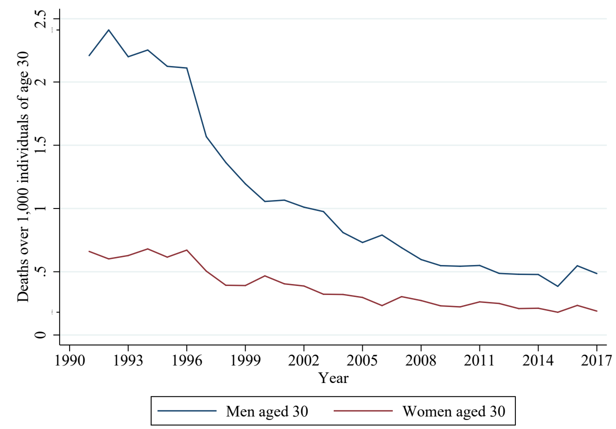
Tables and Figures

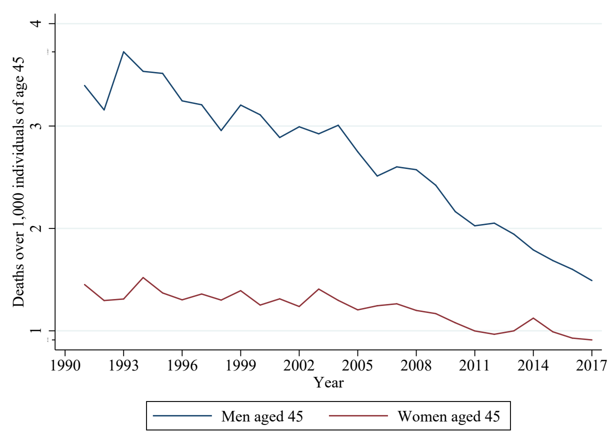
Figure 1: Evolution of the Mortality Rate by Gender in Spain

(a) Mortality rate of individuals at age 14

(b) Mortality rate of individuals at age 30



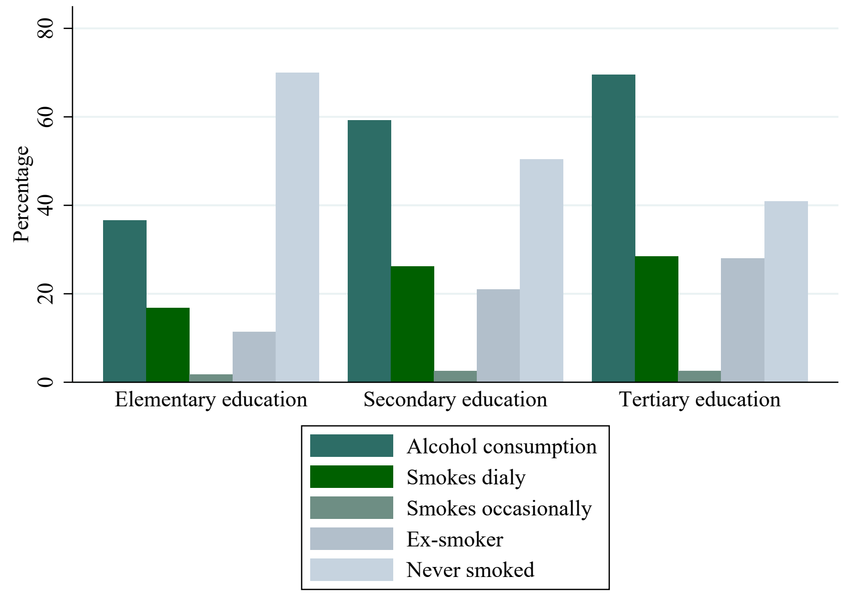
(c) Mortality rate of individuals at age 45



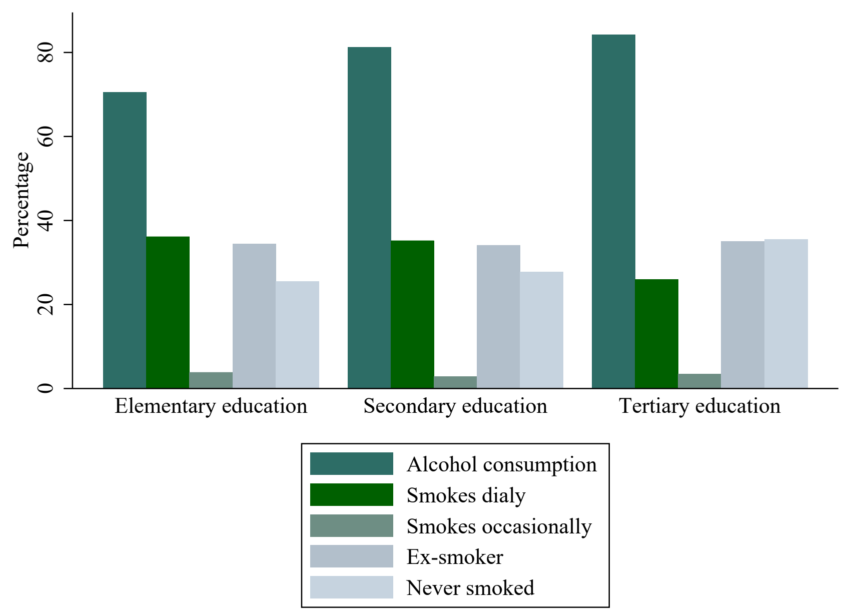
Notes: These figures show the evolution of the mortality rate at age 14, 30 and 45 for men and women for the period 1990 to 2017. The figures graph the number of deaths of men and women at age 14, 30 and 45 per 1,000 individuals of that age and gender. Source: Mortality Registries (1991-2017).

Figure 2: Smoking and Drinking by Gender and Education in Spain

(a) Tobacco and alcohol consumption of women born 1944-1964



(b) Tobacco and alcohol consumption of men born 1944-1964



Notes: These figures show the tobacco and alcohol consumption of women and men born between 1944 and 1964 observed in 2009 by level of education. Source: European Survey of Health in Spain (2009).

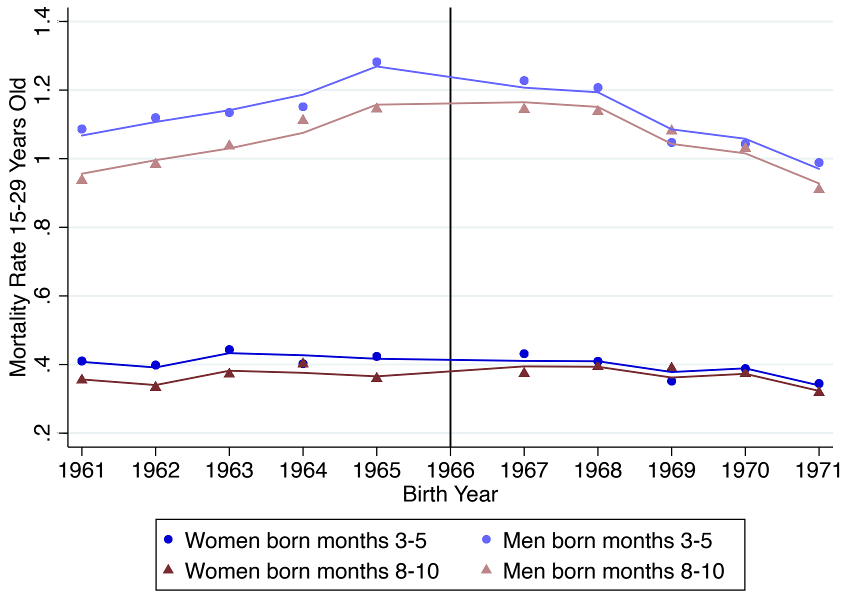
Table 1: Effect of the Reform on Age and Gender-specific Mortality Rates

	Mortality Rate					
	Men			Women		
	Aged 14-45	Under 30	Over 30	Aged 14-45	Over 30	Under 30
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.132***	0.088**	0.081***	0.033*	0.054**	-0.013
	(0.019)	(0.028)	(0.023)	(0.016)	(0.019)	(0.018)
	[0.000]	[0.027]	[0.007]	[0.071]	[0.044]	[0.545]
Treated* Post Reform	-0.033	-0.069**	0.002	0.008	-0.035	0.052**
	(0.022)	(0.030)	(0.021)	(0.013)	(0.020)	(0.019)
	[0.161]	[0.034]	[0.926]	[0.537]	[0.107]	[0.011]
Observations	1,920	960	960	1,920	960	960
R-squared	0.797	0.808	0.734	0.640	0.389	0.592
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	1.455	1.099	1.811	0.562	0.390	0.733
Std. dev. pre-reform	0.566	0.497	0.376	0.251	0.148	0.212

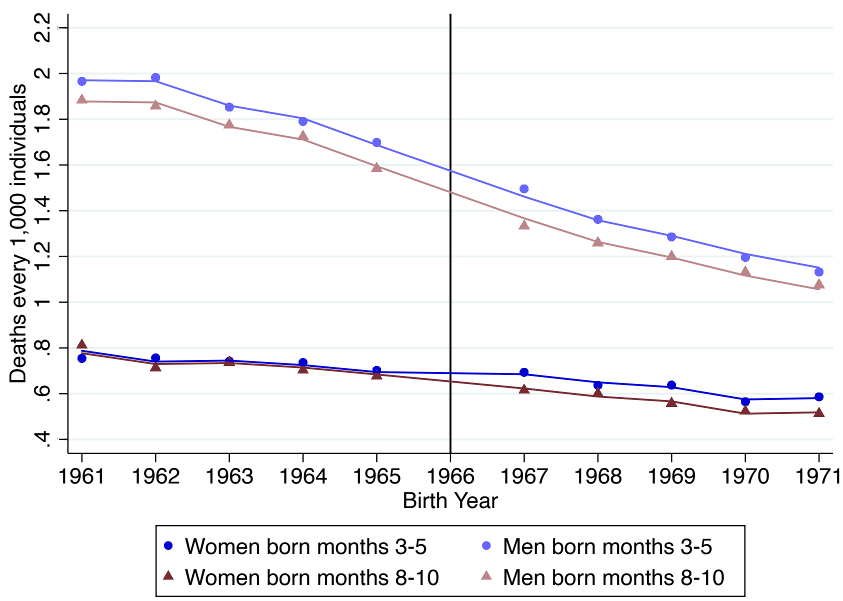
Notes: The dependent variables are the mortality rate (number of men/women that died divided by the total number of men/women born in each cohort and treatment) (1) of men between the ages of 14 and 45, (2) of men between the ages of 14 and 29, (3) of men between the ages of 30 and 45, (4) of women between 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 year of death, year of birth and month of birth 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 999 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: Gender-specific Mortality Rates by Cohort

(a) Mortality rate of individuals under 30



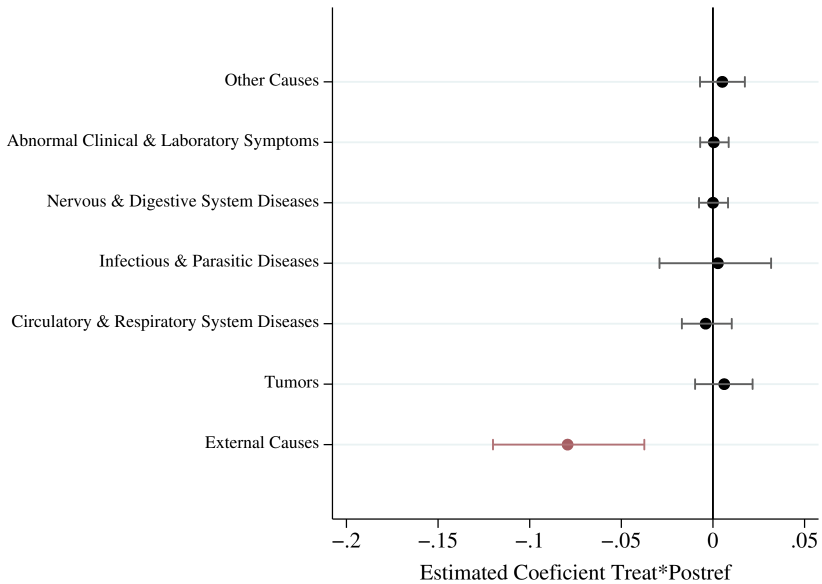
(b) Mortality rate of individuals over 30



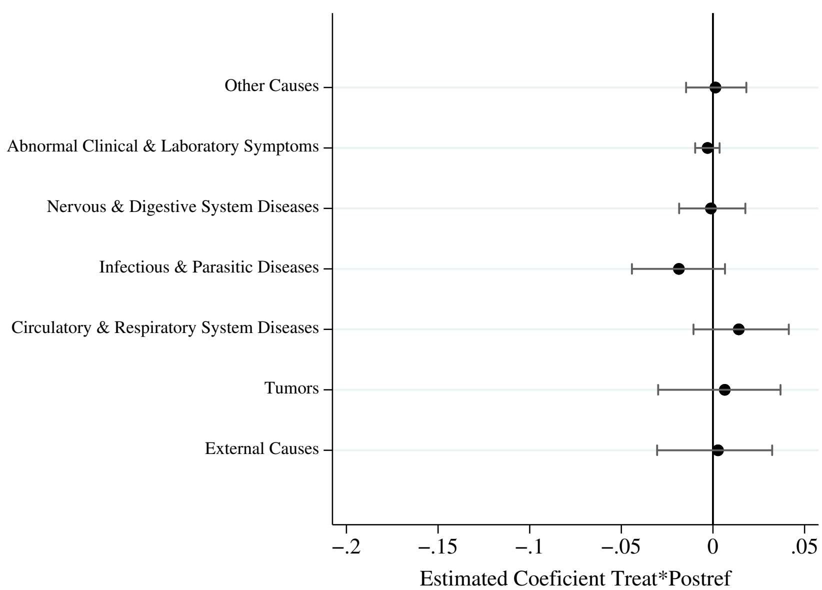
Notes: The dots and triangles represent the average mortality rate of men/women in each cohort, 1961-1971. The lines are the linear predictions from Regression 1. *Source*: Mortality Registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Figure 4: Effect of the Reform on Men's Mortality Rate by Cause of Death

(a) Mortality rate by cause of death of men under 30



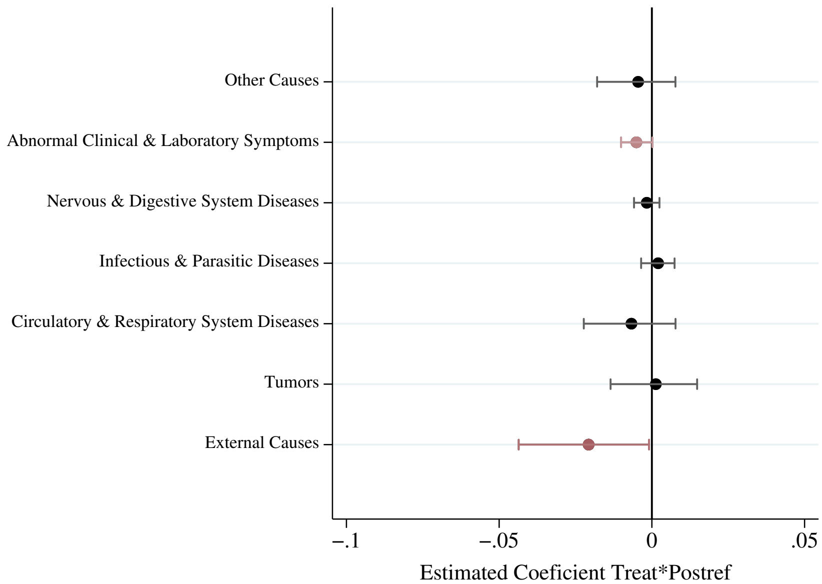
(b) Mortality rate by cause of death of men over 30



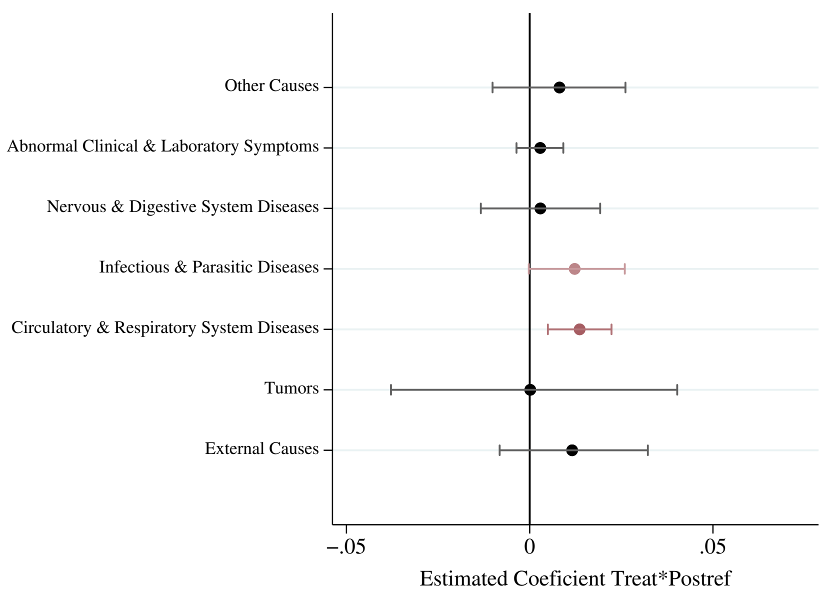
Notes: This figure shows the impact of the reform on men's mortality rate by cause of death. The graphs report the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1. The dependent variables are the mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) (a) between the ages of 14 and 29, or (b) between the ages of 30 and 45, due to the different causes of death. All dependent variables are multiplied by 1,000. The estimates that are significant at the 5% (10%) level are shown in dark (light) red. *Source*: Mortality Registries (1975-2016), all men from cohorts 1961-1965 and 1967-1971.

Figure 5: Effect of the Reform on Women's Mortality Rate by Cause of Death

(a) Mortality rate by cause of death of women under 30



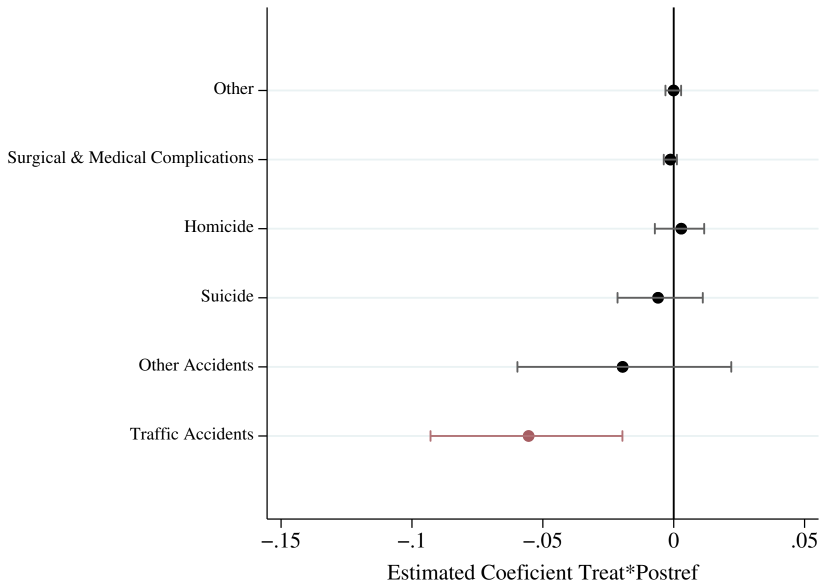
(b) Mortality rate by cause of death of women over 30



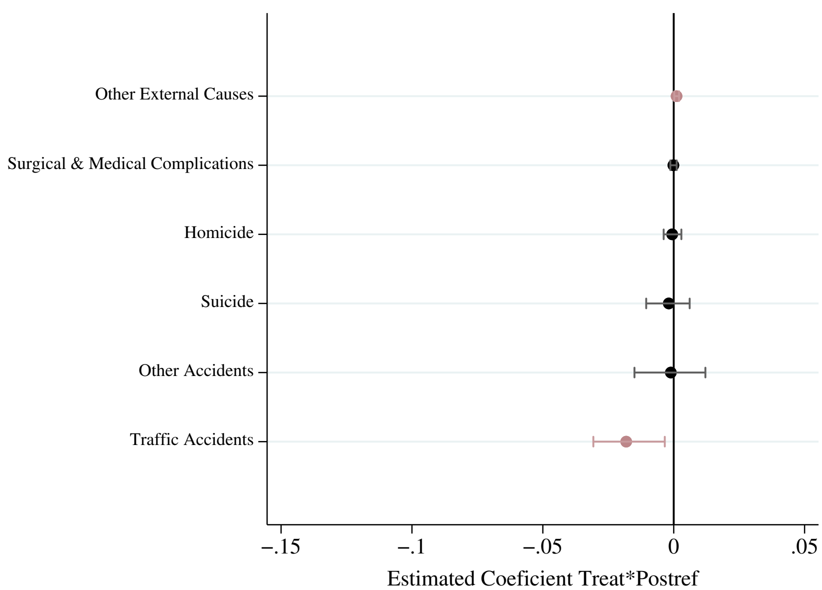
Notes: This figure shows the impact of the reform on women's mortality rate by cause of death. The graphs report the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1. The dependent variables are the mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) (a) between the ages of 14 and 29, or (b) between the ages of 30 and 45, due to the different causes of death. All dependent variables are multiplied by 1,000. The estimates that are significant at the 5% (10%) level are shown in dark (light) red. *Source*: Mortality Registries (1975-2016), all women from cohorts 1961-1965 and 1967-1971.

Figure 6: Effect of the Reform on Mortality Rate by Cause of Death (Under 30)

(a) Mortality rate by cause of death of men under 30



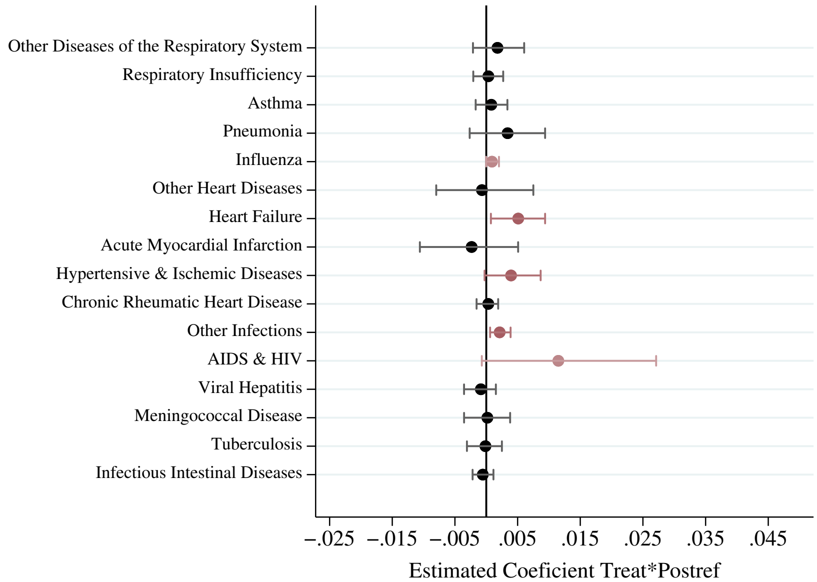
(b) Mortality rate by cause of death of women under 30



Notes: This figure shows the impact of the reform on the mortality rate by cause of death of (a) men or (b) women under the age of 30. The graphs reports the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1. 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 (a) men or (b) women aged 14-29 by the different causes of death.All dependent variables are multiplied by 1,000. The estimates that are significant at the 5% (10%) level are shown in dark (light) red. *Source*: Mortality Registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Figure 7: Effect of the Reform on Mortality Rate by Cause of Death (Over 30)

(a) Mortality rate by cause of death of women over 30



Notes: This figure shows the impact of the reform on the mortality rate by cause of death of (a) men or (b) women over the age of 30. The graphs reports the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1. 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 (a) men or (b) women aged 30-45 by the different causes of death.All dependent variables are multiplied by 1,000. The estimates that are significant at the 5% (10%) level are shown in dark (light) red. *Source*: Mortality Registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Table 2: Effect of the Reform on Women's Sexual Habits

	Ever Used Injectable Drugs (1)	Age at First Sexual Relationship (2)	Total Number Sexual Partners (3)	Ever Taken HIV Test (4)	Knows Results of HIV Test (5)
Treated	0.028	-0.411	-0.079	-0.113	-0.075
	(0.017)	(0.565)	(0.197)	(0.084)	(0.073)
	[0.183]	[0.482]	[0.697]	[0.262]	[0.393]
Treated* Post Reform	-0.014	0.234	-0.029	0.112*	0.107**
	(0.013)	(0.427)	(0.092)	(0.054)	(0.051)
	[0.338]	[0.609]	[0.780]	[0.055]	[0.036]
Observations	1,733	1,484	1,661	1,718	1,732
R-squared	0.012	0.009	0.033	0.021	0.020
Year-Interview FE	YES	YES	YES	YES	YES
Birth-Year FE	YES	YES	YES	YES	YES
Birth-Month FE	YES	YES	YES	YES	YES
Mean pre-reform	0.0101	19.49	1.770	0.295	0.280
Std. dev. pre-reform	0.100	3.260	1.103	0.456	0.449

Notes: The dependent variables are (1) the probability of ever used injectable drugs, (2) the age at which women had their first sexual relationship, (3) the total number of sexual partners, (4) the probability of having ever taken a HIV test, and (5) the probability of knowing the results of the HIV test. Regressions include year of death, year of birth and month of birth 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 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Survey on Health and Sexual Habits (2003), all women from cohorts 1961-1965 and 1967-1971.

Table 3: Effect of the Reform on Women's Health Habits

	Smokes	Ex-smoker	Never smoked	Drinks	Drinks daily	Drinks less once month
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.081*	-0.010	0.089*	0.003	-0.046**	0.022
	(0.031)	(0.024)	(0.036)	(0.043)	(0.014)	(0.027)
	[0.065]	[0.653]	[0.064]	[0.932]	[0.029]	[0.443]
Treated * PostReform	0.079**	-0.028	-0.068**	-0.015	0.050**	-0.115**
	(0.024)	(0.016)	(0.024)	(0.049)	(0.021)	(0.032)
	[0.017]	[0.111]	[0.042]	[0.767]	[0.025]	[0.020]
Observations	2,956	2,956	2,956	2,949	2,029	2,029
R-squared	0.012	0.009	0.017	0.013	0.031	0.028
Year-Interview FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.325	0.245	0.395	0.770	0.104	0.394
Std. dev. pre-reform	0.469	0.430	0.489	0.420	0.305	0.489

Notes: The dependent variables are (1) the probability that the woman smokes, (2) the probability that the woman is an ex-smoker, (3) the probability of never have smoked, (4) the probability of having ever drunk alcohol, (5) the probability of consuming alcohol daily, and (6) the probability of consuming alcohol less than once a month.Regressions include year of death, year of birth and month of birth 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 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Spanish National Health Survey (2006 and 2012), all women from cohorts 1961-1965 and 1967-1971.

Table 4: Robustness Check: Men's Mortality Rate under 30

	Mortality rate- Men under 30					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.103**	0.137***	0.150***	0.091***	0.088**	0.222***
	(0.024)	(0.029)	(0.033)	(0.025)	(0.029)	(0.043)
	[0.019]	[0.003]	[0.003]	[0.006]	[0.020]	[0.000]
Treated* Post Reform	-0.067*	-0.053*	-0.067*	-0.062**	-0.069**	-0.077**
	(0.025)	(0.030)	(0.030)	(0.028)	(0.030)	(0.026)
	[0.051]	[0.095]	[0.062]	[0.032]	[0.034]	[0.015]
Post Reform	0.088					
	(0.223)					
	[0.647]					
Observations	1,056	960	864	17,952	960	1,920
R-squared	0.810	0.804	0.796	0.249	0.834	0.761
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	1.050	1.087	1.076	1.147	1.099	1.129
Std. dev. pre-reform	0.474	0.485	0.481	1.368	0.497	0.510

Notes: The dependent variable is men's mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) between the ages of 14 and 29 (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 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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.

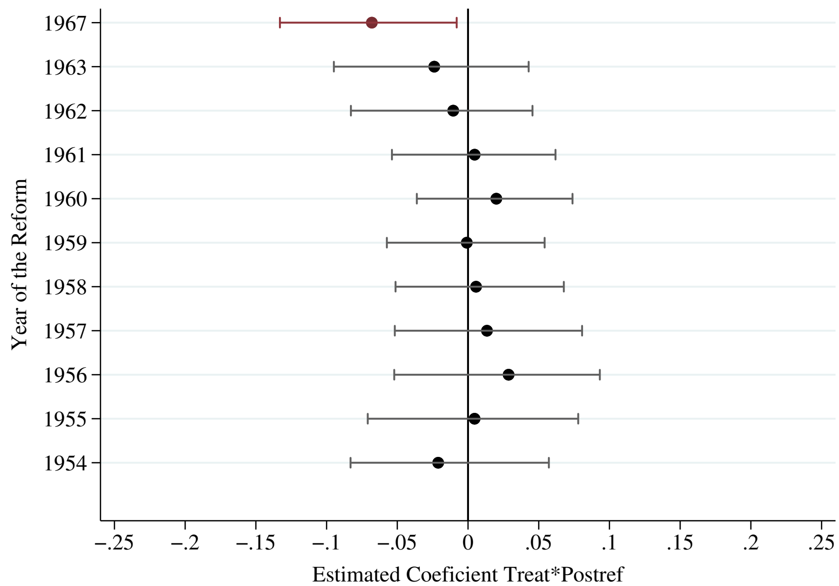
Table 5: Robustness Check: Women's Mortality Rate over 30

	Mortality rate- Women over 30					
Treated	0.006	0.005	0.024	0.020	-0.013	0.045**
	(0.030)	(0.025)	(0.026)	(0.018)	(0.018)	(0.016)
	[0.856]	[0.911]	[0.410]	[0.306]	[0.545]	[0.034]
Treated* Post Reform	0.061**	0.045**	0.049	0.042**	0.052**	0.034***
	(0.025)	(0.021)	(0.026)	(0.018)	(0.019)	(0.013)
	[0.014]	[0.048]	[0.101]	[0.034]	[0.011]	[0.005]
Post Reform	-0.046					
	(0.117)					
	[0.724]					
Observations	1,056	960	864	17,952	960	1,920
R-squared	0.589	0.595	0.595	0.098	0.616	0.575
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	NO
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	0.752	0.730	0.733	0.718	0.733	0.740
Std. dev. pre-reform	0.207	0.219	0.220	1.025	0.212	0.222

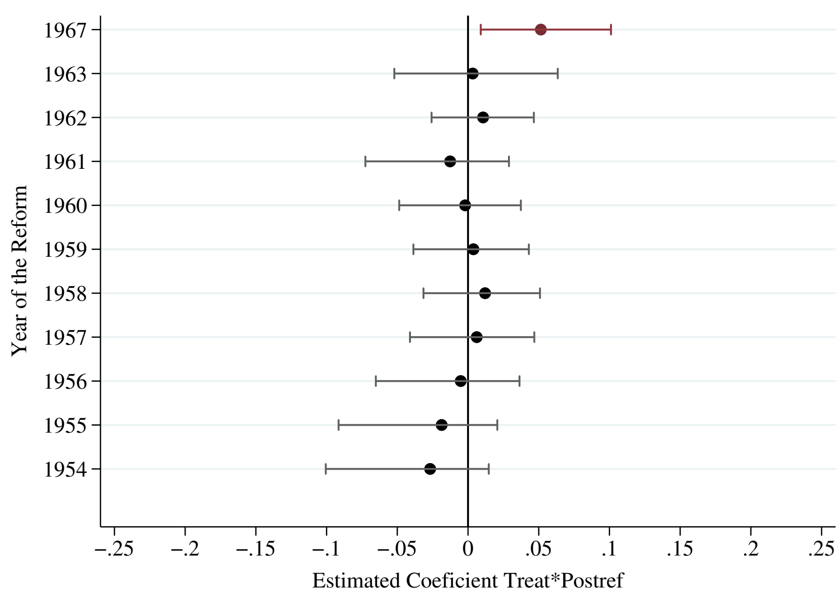
Notes: The dependent variable is women's mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 30 and 45 (multiplied by 1,000). Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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.

Figure 8: Placebos

(a) Mortality rate for men under 30



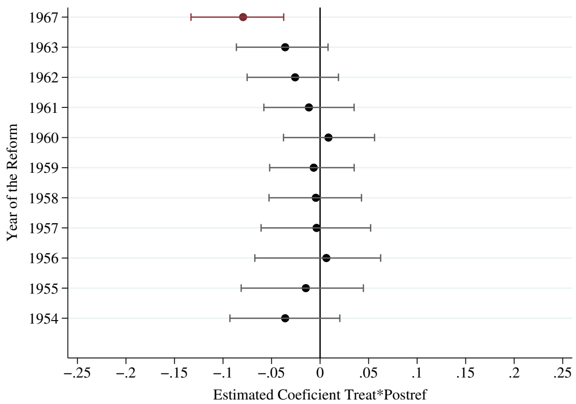
(b) Mortality rate for women over 30



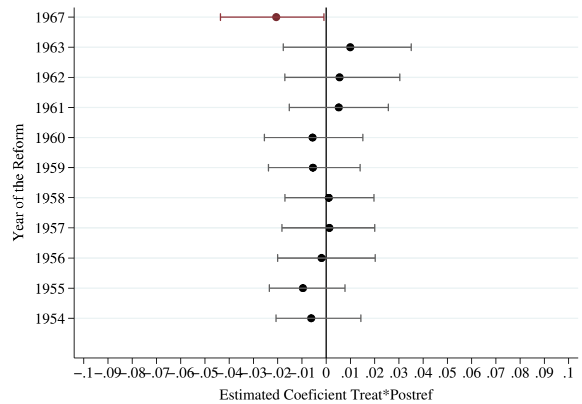
Notes: These graphs report the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1 assuming the first individuals affected by the reform where those born in 1954, 1955, 1957, 1958, 1959, 1960, 1961, 1962, and 1963. The dependent variables are the mortality rate (number of men/women that died divided by the total number of men/women born in each cohort and treatment) (a) of men between the ages of 14 and 29, or (b) of women between the ages of 30 and 45. All dependent variables are multiplied by 1,000. For the placebo estimates (the ones in black), we only consider cohorts not affected by the real reform: 1951-1965. For comparison, we also report (in red) the point estimate and the 95% confidence interval of the interaction term of regression that can be found in column 2 and 6 of Table 1. Source: Mortality Registries (1975-2016), all men and women from the 1951-1965 cohorts.

Figure 9: Placebos by Cause of Death

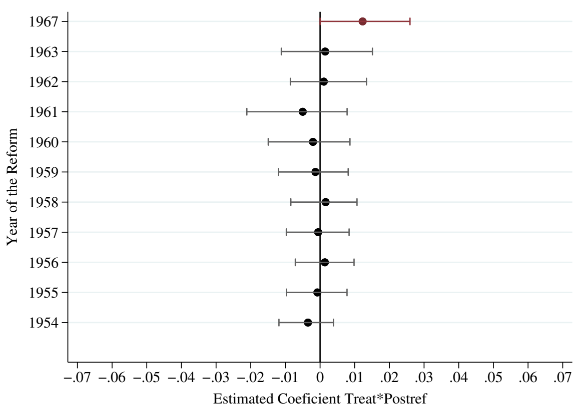
(a) External causes- Men under 30



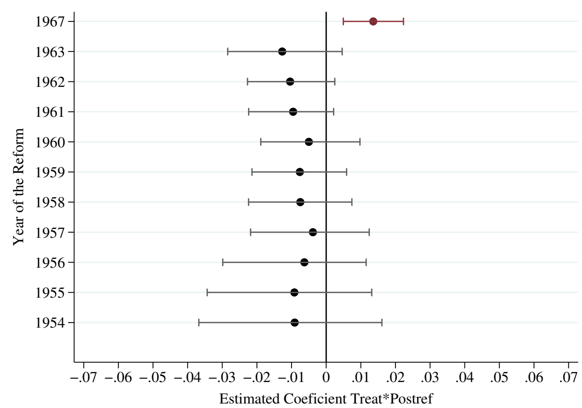
(b) External causes- Women under 30



(c) Infectious & parasitic diseases- Women over 30



(d) Circulatory & respiratory system diseases- Women over 30



Notes: These graphs report the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1 assuming the first individuals affected by the reform where those born in 1954, 1955, 1957, 1958, 1959, 1960, 1961, 1962, and 1963. 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) (a) of men between the ages of 14 and 29 due to external causes of mortality, (b) of women between the ages of 14 and 29 due to external causes of mortality, (c) of women between the ages of 30 and 45 due to infectious or parasitic diseases, or (d) of women between the ages of 30 and 45 due to diseases of the circulatory or respiratory system. All dependent variables are multiplied by 1,000. For the placebo estimates (the ones in black), we only consider cohorts not affected by the real reform: 1951-1965. For comparison, we also report (in red) the point estimate and the 95% confidence interval of the interaction term of regression that can be found in Tables 3 and 4. Source: Mortality Registries (1975-2016), all men and women from the 1951-1965 cohorts.

Appendix Tables and Figures

Table A1: Effect of the Reform on the Mortality Rate by Cause of Death among Men aged 14-29

	Mortality rate- Men under 30						
	External Causes (1)	Tumors (2)	Circulatory & Respiratory System Diseases (3)	Infectious & Parasitic Diseases (4)	Nervous & Digestive System Diseases (5)	Abnormal Clinical Laboratory Symptoms (6)	Other Causes (7)
Treated	0.048***	-0.005	-0.001	0.006	-0.002	-0.000	-0.008
	(0.013)	(0.007)	(0.010)	(0.008)	(0.003)	(0.005)	(0.005)
	[0.006]	[0.523]	[0.889]	[0.489]	[0.414]	[0.898]	[0.177]
Treated* Post Reform	-0.079***	0.006	-0.004	0.003	0.000	0.000	0.005
	(0.019)	(0.007)	(0.006)	(0.013)	(0.003)	(0.004)	(0.005)
	[0.003]	[0.417]	[0.542]	[0.851]	[0.985]	[0.891]	[0.346]
Observations	960	960	960	960	960	960	960
R-squared	0.611	0.071	0.337	0.654	0.184	0.244	0.315
Year-Death FE	YES	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.673	0.0866	0.129	0.0922	0.0236	0.0421	0.0829
Std. dev. pre-reform	0.323	0.0555	0.0850	0.173	0.0315	0.0441	0.0751

Notes: The dependent variables are the mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) between the ages of 14 and 29 due to (1) external causes of mortality, (2) tumors, (3) diseases of the circulatory or respiratory system, (4) infectious or parasitic diseases, (5) diseases of the nervous or digestive system, (6) abnormal clinical or laboratory symptoms, or (7) other causes. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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 A2: Effect of the Reform on the Mortality Rate by Cause of Death among Men aged 30 to 45

	Mortality rate- Men over 30						
	External Causes (1)	Tumors (2)	Circulatory & Respiratory System Diseases (3)	Infectious & Parasitic Diseases (4)	Nervous & Digestive System Diseases (5)	Abnormal Clinical Laboratory Symptoms (6)	Other Causes (7)
Treated	0.010	0.008	0.008	0.033*	0.003	0.001	-0.009
	(0.013)	(0.017)	(0.012)	(0.015)	(0.005)	(0.005)	(0.007)
	[0.425]	[0.705]	[0.550]	[0.072]	[0.509]	[0.780]	[0.257]
Treated* Post Reform	0.003	0.006	0.014	-0.019	-0.001	-0.003	0.001
	(0.014)	(0.015)	(0.012)	(0.010)	(0.008)	(0.003)	(0.007)
	[0.865]	[0.659]	[0.248]	[0.129]	[0.893]	[0.382]	[0.848]
Observations	960	960	960	960	960	960	960
R-squared	0.435	0.628	0.525	0.843	0.418	0.200	0.176
Year-Death FE	YES	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.533	0.297	0.292	0.425	0.123	0.0716	0.0943
Std. dev. pre-reform	0.187	0.186	0.138	0.308	0.0800	0.0553	0.0590

Notes: The dependent variables are the mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) between the ages of 30 and 45 due to (1) external causes of mortality, (2) tumors, (3) diseases of the circulatory or respiratory system, (4) infectious or parasitic diseases, (5) diseases of the nervous or digestive system, (6) abnormal clinical or laboratory symptoms, or (7) other causes. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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 A3: Effect of the Reform on the Mortality Rate by Cause of Death among Women aged 14-29

	Mortality rate- Women under 30						
	External Causes (1)	Tumors (2)	Circulatory & Respiratory System Diseases (3)	Infectious & Parasitic Diseases (4)	Nervous & Digestive System Diseases (5)	Abnormal Clinical Laboratory Symptoms (6)	Other Causes (7)
Treated	0.011	-0.004	0.005	0.002	0.002	0.003	0.002
	(0.010)	(0.005)	(0.008)	(0.004)	(0.002)	(0.003)	(0.005)
	[0.273]	[0.422]	[0.551]	[0.594]	[0.269]	[0.365]	[0.653]
Treated* Post Reform	-0.021**	0.001	-0.007	0.002	-0.002	-0.005*	-0.005
	(0.009)	(0.006)	(0.007)	(0.002)	(0.002)	(0.002)	(0.006)
	[0.034]	[0.849]	[0.337]	[0.436]	[0.415]	[0.059]	[0.437]
Observations	960	960	960	960	960	960	960
R-squared	0.201	0.064	0.112	0.562	0.081	0.066	0.097
Year-Death FE	YES	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.160	0.0656	0.0610	0.0364	0.0114	0.0145	0.0519
Std. dev. pre-reform	0.0909	0.0522	0.0472	0.0635	0.0210	0.0239	0.0469

Notes: The dependent variables are the mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 14 and 29 due to (1) external causes of mortality, (2) tumors, (3) diseases of the circulatory or respiratory system, (4) infectious or parasitic diseases, (5) diseases of the nervous or digestive system, (6) abnormal clinical or laboratory symptoms, or (7) other causes. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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 A4: Effect of the Reform on the Mortality Rate by Cause of Death among Women aged 30-45

	Mortality rate- Women over 30						
	External Causes (1)	Tumors (2)	Circulatory & Respiratory System Diseases (3)	Infectious & Parasitic Diseases (4)	Nervous & Digestive System Diseases (5)	Abnormal Clinical Laboratory Symptoms (6)	Other Causes (7)
Treated	0.004	0.033*	-0.015**	-0.009	-0.006	0.002	0.004
	(0.009)	(0.016)	(0.004)	(0.007)	(0.006)	(0.003)	(0.006)
	[0.727]	[0.071]	[0.021]	[0.309]	[0.382]	[0.495]	[0.539]
Treated* Post Reform	0.012	0.000	0.014***	0.012*	0.003	0.003	0.008
	(0.008)	(0.017)	(0.004)	(0.006)	(0.007)	(0.003)	(0.008)
	[0.208]	[0.989]	[0.002]	[0.061]	[0.692]	[0.323]	[0.347]
Observations	960	960	960	960	960	960	960
R-squared	0.088	0.669	0.228	0.599	0.161	0.104	0.077
Year-Death FE	YES	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.112	0.292	0.106	0.111	0.0388	0.0203	0.0604
Std. dev. pre-reform	0.0674	0.175	0.0677	0.0967	0.0396	0.0304	0.0468

Notes: The dependent variables are the 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 (1) external causes of mortality, (2) tumors, (3) diseases of the circulatory or respiratory system, (4) infectious or parasitic diseases, (5) diseases of the nervous or digestive system, (6) abnormal clinical or laboratory symptoms, or (7) other causes. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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 A5: Effect of the Reform on the Mortality Rate among Men aged 14-29 due to External Causes

	Mortality rate due to external causes- Men under 30					
	Traffic Accidents (1)	Other Accidents (2)	Suicide (3)	Homicide (4)	Surgical & Medical Complications (5)	Other (6)
Treated	0.060***	0.007	-0.004	0.005*	0.000	-0.000
	(0.015)	(0.016)	(0.009)	(0.003)	(0.001)	(0.002)
	[0.000]	[0.646]	[0.646]	[0.077]	[0.799]	[0.882]
Treated* Post Reform	-0.055***	-0.020	-0.006	0.003	-0.001	-0.000
	(0.017)	(0.018)	(0.007)	(0.004)	(0.001)	(0.001)
	[0.00]	[0.306]	[0.451]	[0.507]	[0.354]	[0.965]
Observations	960	960	960	960	960	960
R-squared	0.475	0.469	0.389	0.150	0.043	0.363
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.325	0.225	0.0620	0.0269	0.00243	0.00537
Std. dev. pre-reform	0.170	0.134	0.0609	0.0353	0.00934	0.0170

Notes: The dependent variables are the mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) between the ages of 14 and 29 due to (1) traffic accidents, (2) other type of accidents, (3) suicide, (4) homicide, (5) surgical or medical complications, or (6) other external causes of mortality. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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 A6: Effect of the Reform on the Mortality Rate among Women aged 14-29 due to External Causes

	Mortality rate due to external causes- Women under 30					
	Traffic Accidents (1)	Other Accidents (2)	Suicide (3)	Homicide (4)	Surgical & Medical Complications (5)	Other (6)
Treated	0.020**	0.007*	0.001	0.004*	0.000	-0.001
	(0.006)	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)
	[0.015]	[0.091]	[0.752]	[0.139]	[0.991]	[0.096]
Treated* Post Reform	-0.018**	-0.001	-0.002	-0.001	-0.000	0.001**
	(0.006)	(0.006)	(0.004)	(0.001)	(0.001)	(0.000)
	[0.021]	[0.864]	[0.621]	[0.641]	[0.898]	[0.029]
Observations	960	960	960	960	960	960
R-squared	0.181	0.085	0.128	0.050	0.071	0.200
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.0847	0.0445	0.0172	0.00648	0.00153	0.00154
Std. dev. pre-reform	0.0591	0.0430	0.0270	0.0154	0.00776	0.00777

Notes: The dependent variables are the mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 14 and 29 due to (1) traffic accidents, (2) other type of accidents, (3) suicide, (4) homicide, (5) surgical or medical complications, or (6) other external causes of mortality. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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.

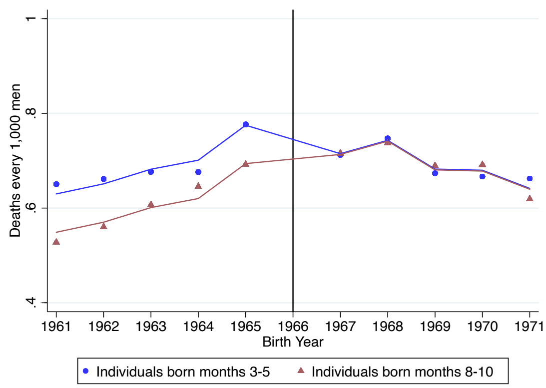
Table A7: Effect of the Reform on the Mortality Rate among Women aged 30-45 due to Infectious & Parasitic Diseases or Circulatory & Respiratory System Diseases

	Mortality rate due to- Women over 30															
	Infectious & Parasitic Diseases						Circulatory & Respiratory System Diseases									
	Infectious Intestinal Diseases	Tuberculosis	Meningococcal Disease	Viral Hepatitis	AIDS & HIV	Other	Chronic Rheumatic Heart Diseases	Ischemic Diseases	Acute Myocardial Infarction	Heart Failure	Other Heart Diseases	Influenza	Pneumonia	Asthma	Respiratory Insufficiency	Other Respiratory Diseases
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treated	0.001	-0.000	-0.001	0.003	-0.011*	-0.001	-0.001	-0.001	-0.002	-0.003	-0.001	0.001	-0.002	-0.000	-0.000	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.005)	(0.001)	(0.001)	(0.001)	(0.005)	(0.003)	(0.005)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)
	[0.267]	[0.958]	[0.770]	[0.167]	[0.084]	[0.324]	[0.394]	[0.500]	[0.780]	[0.350]	[0.858]	[0.062]	[0.655]	[0.978]	[0.807]	[0.395]
Treated* Post Reform	-0.001	-0.000	0.000	-0.001	0.012*	0.002**	0.000	0.004**	-0.002	0.005**	-0.001	0.001*	0.003	0.001	0.000	0.002
	(0.001)	(0.001)	(0.002)	(0.001)	(0.006)	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.000)	(0.002)	(0.001)	(0.001)	(0.002)
	[0.402]	[0.932]	[0.906]	[0.590]	[0.082]	[0.008]	[0.553]	[0.063]	[0.508]	[0.017]	[0.837]	[0.054]	[0.223]	[0.496]	[0.696]	[0.335]
Observations	960	960	960	960	960	960	960	960	960	960	960	960	960	960	960	960
R-squared	0.108	0.058	0.042	0.051	0.626	0.059	0.060	0.083	0.103	0.056	0.115	0.079	0.034	0.045	0.052	0.057
Year-Death FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.00221	0.00161	0.00498	0.00398	0.0928	0.00138	0.00143	0.00465	0.0124	0.00823	0.0489	7.94e-05	0.00749	0.00374	0.00189	0.0125
Std. dev. pre-reform	0.00934	0.00791	0.0131	0.0120	0.0922	0.00780	0.00703	0.0139	0.0229	0.0174	0.0437	0.00174	0.0168	0.0116	0.00807	0.0207

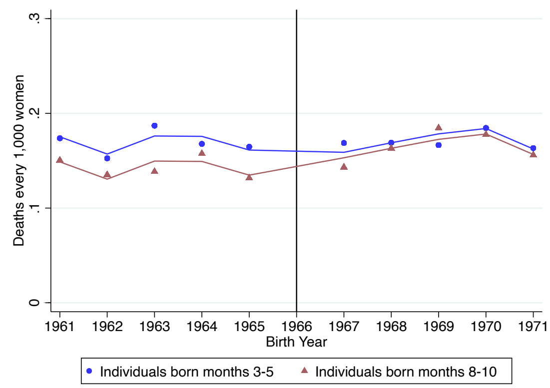
Notes: The dependent variables are the mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 30 and 45 due to (1) infectious intestinal diseases, (2) tuberculosis, (3) meningococcal disease, (4) viral hepatitis, (5) AIDS or HIV, (6) other infectious or parasitic diseases, (7) chronic rheumatic heart diseases, (8) ischemic diseases, (9) acute myocardial infarction, (10) hearth failure, (11) other heart diseases, (12) influenza, (13) pneumonia, (14) asthma, (15) respiratory insufficiency or (16) other respiratory diseases. All dependent variables are multiplied by 1,000. Regressions include year of death, year of birth and month of birth 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 999 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.

Figure A1: Gender-specific Mortality Rates by Cohort

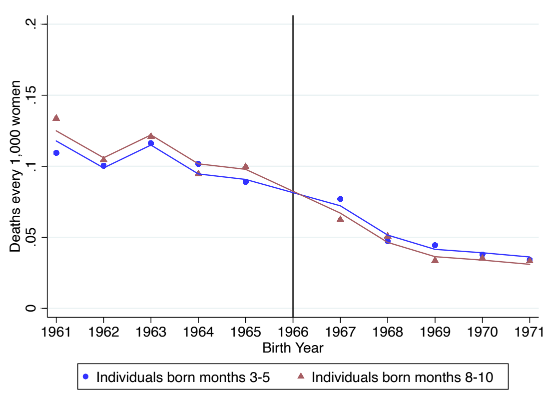
(a) Mortality rate of men under 30 due to external causes



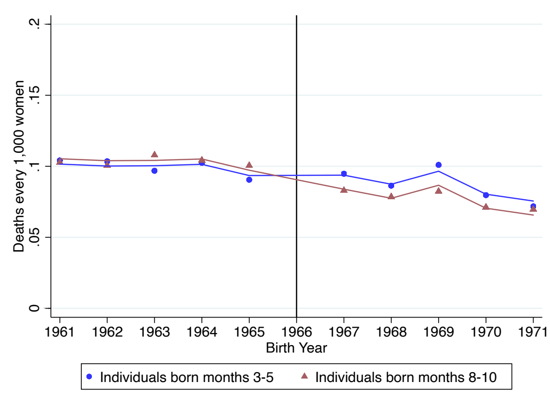
(b) Mortality rate of women under 30 due to external causes



(c) Mortality rate of women over 30 due to infectious or parasitic diseases



(d) Mortality rate of women over 30 due to respiratory or circulatory system diseases



Notes: The dots and triangles represent the average mortality rate of men/women in each cohort, 1961-1971, due to that specific cause of death. The lines are the linear predictions from Regression 1. *Source*: Mortality Registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

Table A8: Effect of the Reform on Men's Sexual Habits

	Ever Used Injectable Drugs (1)	Age at First Sexual Relationship (2)	Total Number Sexual Partners (3)	Ever Taken HIV Test (4)	Knows Results of HIV Test (5)
Treated	-0.007	-0.068	-0.138	0.041	0.039
	(0.032)	(0.463)	(0.231)	(0.072)	(0.073)
	[0.831]	[0.871]	[0.572]	[0.601]	[0.612]
Treated* Post Reform	-0.033**	0.580	-0.103	0.013	-0.007
	(0.014)	(0.451)	(0.157)	(0.035)	(0.039)
	[0.041]	[0.255]	[0.530]	[0.710]	[0.868]
Observations	1,487	1,300	1,402	1,480	1,485
R-squared	0.010	0.021	0.029	0.018	0.017
Year-Interview FE	YES	YES	YES	YES	YES
Birth-Year FE	YES	YES	YES	YES	YES
Birth-Month FE	YES	YES	YES	YES	YES
Mean pre-reform	0.0356	18.63	2.910	0.208	0.193
Std. dev. pre-reform	0.185	3.356	1.496	0.406	0.395

Notes: The dependent variables are (1) the probability of ever used injectable drugs, (2) the age at which men had their first sexual relationship, (3) the total number of sexual partners, (4) the probability of having ever taken a HIV test, and (5) the probability of knowing the results of the HIV test. Regressions include year of death, year of birth and month of birth 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 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Survey on Health and Sexual Habits (2003), all men from cohorts 1961-1965 and 1967-1971.

Table A9: Effect of the Reform on Men's Health Habits

	Smokes (1)	Ex-smoker (2)	Never smoked (3)	Drinks (4)	Drinks daily (5)	Drinks less once month (6)
Treated	-0.048	0.046	0.005	0.021	-0.004	0.005
	(0.030)	(0.037)	(0.056)	(0.029)	(0.023)	(0.028)
	[0.193]	[0.225]	[0.923]	[0.617]	[0.862]	[0.906]
Treated * PostReform	0.067	-0.030	-0.049	0.003	-0.031	-0.063**
	(0.053)	(0.025)	(0.052)	(0.031)	(0.033)	(0.020)
	[0.264]	[0.278]	[0.394]	[0.936]	[0.420]	[0.019]
Observations	2,441	2,441	2,441	2,438	2,074	2,074
R-squared	0.006	0.022	0.030	0.006	0.060	0.045
Year-Interview FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Mean pre-reform	0.392	0.286	0.286	0.908	0.242	0.236
Std. dev. pre-reform	0.488	0.452	0.452	0.289	0.428	0.425

Notes: The dependent variables are (1) the probability that the man smokes, (2) the probability that the man is an ex-smoker, (3) the probability of never have smoked, (4) the probability of having ever drunk alcohol, (5) the probability of consuming alcohol daily, and (6) the probability of consuming alcohol less than once a month. Regressions include year of death, year of birth and month of birth 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 999 replications in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. *Source*: Spanish National Health Survey (2006 and 2012), all men from cohorts 1961-1965 and 1967-1971.

Table A10: Robustness Check: Mortality Rate among Men under 30 due to External Causes

	Mortality rate due to external causes					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.082*	0.105***	0.115**	0.073**	0.069***	0.170***
	(0.016)	(0.024)	(0.026)	(0.014)	(0.016)	(0.029)
	[0.076]	[0.011]	[0.015]	[0.002]	[0.002]	[0.000]
Treated* Post Reform	-0.074**	-0.079**	-0.096***	-0.077***	-0.079***	-0.071***
	(0.021)	(0.022)	(0.018)	(0.017)	(0.019)	(0.017)
	[0.047]	[0.011]	[0.007]	[0.001]	[0.003]	[0.000]
Post Reform	0.030					
	(0.173)					
	[0.849]					
Observations	1,056	864	768	17,952	960	1,920
R-squared	0.619	0.601	0.599	0.160	0.765	0.557
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	NO
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	0.614	0.647	0.647	0.708	0.647	0.673
Std. dev. pre-reform	0.290	0.308	0.308	1.060	0.308	0.323

Notes: The dependent variable is men's mortality rate (number of men that died divided by the total number of men born in each cohort and treatment) between the ages of 14 and 29 due to external causes (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 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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 A11: Robustness Check: Mortality Rate among Men and Women under 30 due to External Causes

	Mortality rate due to external causes					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.041*	0.028***	0.031**	0.027**	0.031***	0.040***
	(0.015)	(0.010)	(0.013)	(0.009)	(0.009)	(0.014)
	[0.076]	[0.007]	[0.007]	[0.012]	[0.001]	[0.000]
Treated* Post Reform	-0.023**	-0.019*	-0.024**	-0.016	-0.021**	-0.011
	(0.010)	(0.010)	(0.011)	(0.010)	(0.009)	(0.008)
	[0.047]	[0.089]	[0.039]	[0.149]	[0.034]	[0.209]
Post Reform	0.005					
	(0.063)					
	[0.209]					
Observations	1,056	864	768	17,952	960	1,920
R-squared	0.194	0.199	0.205	0.035	0.314	0.201
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	NO
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	0.156	0.156	0.156	0.173	0.156	0.160
Std. dev. pre-reform	0.0848	0.0840	0.0840	0.548	0.0840	0.0909

Notes: The dependent variable is 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 14 and 29 due to external causes (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 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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 A12: Robustness Check: Mortality Rate among Women over 30 due to Infectious or Parasetic Diseases

	Mortality rate due to infectious & parasitic diseases					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.004	-0.007	-0.012	-0.002	-0.010	0.001
	(0.008)	(0.008)	(0.008)	(0.006)	(0.007)	(0.008)
	[0.683]	[0.484]	[0.328]	[0.749]	[0.231]	[0.888]
Treated* Post Reform	0.012	0.011	0.016**	0.010*	0.012*	0.004
	(0.007)	(0.007)	(0.007)	(0.005)	(0.006)	(0.005)
	[0.121]	[0.148]	[0.015]	[0.074]	[0.061]	[0.510]
Post Reform	0.007					
	(0.044)					
	[0.880]					
Observations	1,056	864	768	17,952	960	1,920
R-squared	0.588	0.616	0.611	0.085	0.604	0.601
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	NO
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	0.114	0.107	0.107	0.101	0.107	0.111
Std. dev. pre-reform	0.0992	0.0950	0.0950	0.403	0.0950	0.0967

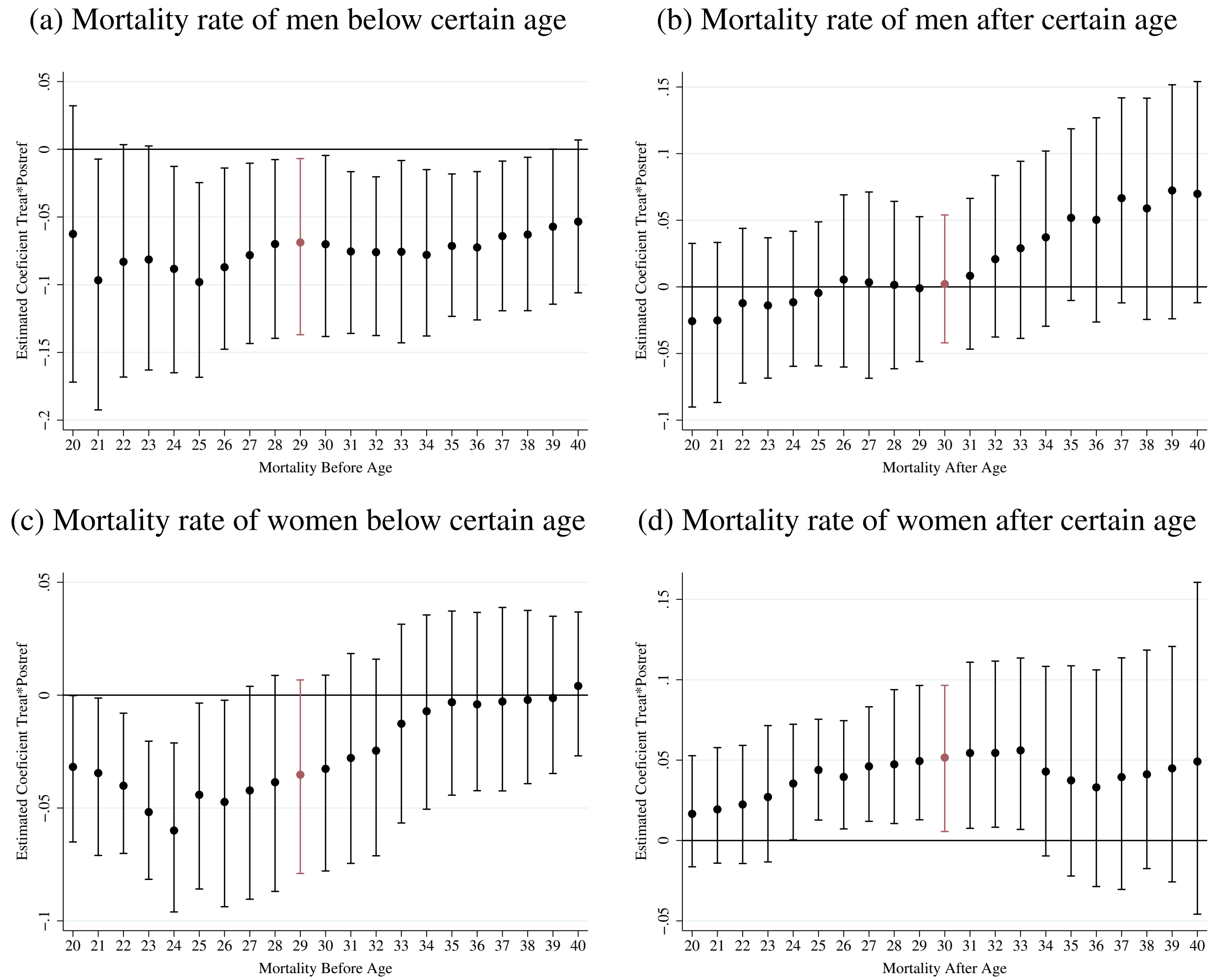
Notes: The dependent variable is women's mortality rate (number of women that died divided by the total number of women born in each cohort and treatment) between the ages of 30 and 45 due to infectious or parasitic diseases (multiplied by 1,000). Regressions (1) assume the 1964 to 1966 cohorts to be partially affected by the reform, (2-3) eliminate the cohorts 1965-66 and 1964-66 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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 A13: Robustness Check: Mortality Rate among Women over 30 due to Diseases of the Circulatory or Respiratory System

	Mortality rate due to circulatory & respiratory system diseases					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.013	-0.008	-0.008	0.004	-0.010*	0.001
	(0.007)	(0.005)	(0.006)	(0.005)	(0.005)	(0.008)
	[0.143]	[0.144]	[0.257]	[0.393]	[0.073]	[0.902]
Treated* Post Reform	0.011*	0.012***	0.012**	0.010*	0.014***	0.015***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
	[0.064]	[0.003]	[0.023]	[0.059]	[0.002]	[0.001]
Post Reform	0.061					
	(0.030)					
	[0.153]					
Observations	1,056	864	768	17,952	960	1,920
R-squared	0.235	0.239	0.239	0.023	0.246	0.226
Year-Death FE	YES	YES	YES	YES	YES	YES
Year-Birth FE	YES	YES	YES	YES	YES	YES
Month-Birth FE	YES	YES	YES	YES	YES	YES
Age Death FE	NO	NO	NO	NO	YES	NO
Region FE	NO	NO	NO	YES	NO	NO
Mean pre-reform	0.103	0.101	0.101	0.0973	0.101	0.106
Std. dev. pre-reform	0.0623	0.0638	0.0638	0.347	0.0638	0.0677

Notes: The dependent variable is 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 diseases of the circulatory or respiratory system (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 from the analysis, (4) include regional FE, (5) include age of death FE, and (6) assumes treated individuals are those born from January to July and control individuals those born from August to December. All regressions include year of death, year of birth and month of birth dummies. For regressions (1-5) 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 999 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.

Figure A2: Robustness: Gender-specific Mortality Rates with Different Age Brackets



Notes: This figure shows the impact of the reform on the mortality rate of (a, b) men or (c,d) women in different age brackets. The graphs reports the point estimates and the 95% confidence interval of the interaction term (Treated* Post Reform) from Regression 1. 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 (a) men or (b) women in the age bracket that is indicated. All dependent variables are multiplied by 1,000. *Source*: Mortality Registries (1975-2016), all men and women from cohorts 1961-1965 and 1967-1971.

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 Registries

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 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 deaths 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 B1, B2 and B3:

- **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), month of birth (March, April, May, August, September and October), and year of death (1975-2016). Then we restrict the sample to deaths of individuals aged 14-45 at the time of death. We obtain 3840 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in columns 1 and 4 in Table 1.
- **Mortality rate of men/women aged 14-29**. We first collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), and year of death (1975-2000). Then we restrict the sample to deaths of individuals aged 14-29 at the time of death. We obtain 1920 cells. We

then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in columns 2 and 5 in Table 1, Table 4, and Graph a) of Figure 8.

- **Mortality rate of men/women aged 30-45**. We first collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), and year of death (1991-2016). Then we restrict the sample to deaths of individuals aged 30-45 at the time of death. We obtain 1920 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in columns 3 and 6 in Table 1, Table 5, and Graph b) of Figure 8.

- **Mortality rate of men/women aged 14-29 by cause of death (big groups)**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), year of death (1975-2000), and cause of death (seven categories). Then we restrict the sample to deaths of individuals aged 14-29 at the time of death. We obtain 13440 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in the Graph a) of Figure 4, Graph a) of Figure 5, and Graphs a), b) and c) of Figure 9.

- **Mortality rate of men/women aged 30-45 by cause of death (big groups)**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), year of death (1991-2016), and cause of death (seven categories). Then we restrict the sample to deaths of individuals aged 30-45 at the time of death. We obtain 13440 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in the Graph b) of Figure 4, Graph b) of Figure 5, and Graphs d) and e) of Figure 9.

- **Mortality rate of men/women aged 14-29 by cause of death (detailed)**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), year of death (1975-2000), and cause of death (twenty-four categories). Then we restrict the sample to deaths of individuals aged 14-29 at the time of death. We obtain 50688 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in the Graph a) of Figure 4, Graph a) of Figure 5, and Graphs a), b) and c) of Figure 9.

- **Mortality rate of men/women aged 30-45 by cause of death (detailed)**. We collapse the death certificates by gender (men or women), year of birth (1961-1965, 1967-1971), month of birth (March, April, May, August, September and October), year of death (1991-2016), and cause of death (twenty-four categories). Then we restrict the sample to deaths of individuals aged 30-45 at the time of death. We obtain 50688 cells. We then divide the number of deaths in each cell by the number of men or women born in each month (March, April, May, August, September and October) and year (1961-1965, 1967-1971). Finally, we multiply the result by 1,000. This variable is used in the Graph b) of Figure 4, Graph b) of Figure 5, and Graphs d) and e) of Figure 9.

We examine seven different causes of death:

- **External causes of mortality**, which includes deaths due to traffic accidents, other accidents (accidental falls, drowning, accidents with fire, or accidental poisoning), suicide, homicide, surgical and medical complications, and other type of external causes of mortality. When examining the mechanisms, we these six more detailed categories as different causes of death.

- **Tumors**, which includes deaths due to malignant tumors located in different parts of the body.

- **Diseases of the circulatory and respiratory system**, which includes deaths due to chronic rheumatic hearth diseases, ischemic diseases, acute myocardial infarction, heart failure, other hearth diseases, influenza, pneumonia, asthma, respiratory insufficiency, and other respiratory diseases. When examining the mechanisms, we these ten more detailed categories as different causes of death.

- **Infections and parasitic diseases**, which includes deaths due to infectious intestinal diseases, tuberculosis, meningococcal disease, viral hepatitis, AIDS and HIV, and other infectious diseases. When examining the mechanisms, we these six more detailed categories as different causes of death.

- **Diseases of the nervous and digestive system**, which includes deaths due to meningitis, Alzheimer's, stomach ulcer, enteritis, non-infectious colitis, and intestinal vascular diseases.

- **Abnormal clinical and laboratory symptoms**, which includes deaths due to a hearth attack or other abnormal clinical and laboratory symptoms.

- **Other causes**, which includes deaths due to other causes that have not been mentioned above.

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 peoples opinions and attitudes toward HIV/AIDS infection, their transmission mechanisms, and the measures for preventing them.

The initial sample consisted of approximately 10,838 individuals within the 18-49 age group distributed in 1,700 census sections. We restrict the sample to Spanish individuals born between 1961-1965 and 1967-1971, and those individuals born in March, April, May, August, September and October. Thus, our final sample consists of 3,224 individuals (1734 women and 1490 men).

Here we define the dependent variables used in Section 3.1 (Table 2 and Table A8), whose descriptive statistics can be found in Table B4:

- **Ever Used Injectable Drugs**: A dummy variable that is equal to one if the individual has ever used injectable drugs, and zero otherwise.

- **Age at First Sexual Relationship**: A continuous variable that indicates the age at which the woman or man had when they had their first sexual relationship.

- **Total Number of Sexual Partners**: Total number of sexual partners that the individual has had until this moment.

- **Ever Taken HIV Test**: A dummy variable that is equal to one if the individual has ever been tested for HIV, and zero otherwise.

- **Knows 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.

Spanish National Survey of Health

The Spanish National Health Survey is a survey conducted by the Ministry of Health, Consumption and Social Welfare with the collaboration of the National Statistics Institute, which collects health information of the population residing in Spain. It is a five-year periodic survey that collects infromation on allows sociodemographic status, health status, use of health services and health determinants. Unfortunately only the surveys conducted in 2006 and 2011 have the information necessary for our analysis (month and year of birth).

The initial sample consisted of approximately 37,646 individuals with more than 16 years old. We restrict the sample to Spanish individuals born between 1961-1965 and 1967-1971, and those individuals born in March, April, May, August, September and October. Thus, our final sample consists of 5,399 individuals (2,956 women and 2,433 men).

Here we define the dependent variables used in Section 3.1 (Table 3 and Table A9), whose descriptive statistics can be found in Table B5:

- **Smokes**: A dummy variable that is equal to one if the individual smokes at the moment, and zero otherwise.
- **Ex-smoker**: A dummy variable that is equal to one if the individual does not smoke at the moment but used to smoke in the past, and zero otherwise.
- **Never smoked**: A dummy variable that is equal to one if the individual has never smoked, and zero otherwise.
- **Drinks**: A dummy variable that is equal to one if the individual has even drunk alcohol, and zero otherwise.
- **Drinks daily**: A dummy variable that is equal to one if the individual drinks alcohol everyday, and zero otherwise.
- **Drinks less once month**: A dummy variable that is equal to one if the individual drinks alcohol less than one time a month, and zero otherwise.

Data Appendix Tables

Table B1: Descriptive Mortality Statistics

	Treated					Control				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Women										
Mortality Rate 14-45	960	0.54	0.24	0.07	1.33	960	0.51	0.24	0.00	1.57
Mortality Rate 14-29	480	0.40	0.14	0.07	0.86	480	0.37	0.14	0.00	0.77
Mortality Rate 30-45	480	0.68	0.24	0.11	1.33	480	0.65	0.24	0.11	1.57
Men										
Mortality Rate 14-45	960	1.35	0.51	0.14	2.94	960	1.27	0.50	0.10	2.69
Mortality Rate 14-29	480	1.13	0.46	0.14	2.59	480	1.05	0.47	0.10	2.14
Mortality Rate 30-45	480	1.58	0.45	0.71	2.94	480	1.48	0.43	0.62	2.69

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 B2: Descriptive Mortality Statistics for Individuals aged 14-29

	Treated					Control				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Women										
External Causes	480	0.17	0.09	0.00	0.52	480	0.15	0.09	0.00	0.56
Tumors	480	0.06	0.05	0.00	0.25	480	0.06	0.05	0.00	0.25
Circulatory & Respiratory System Diseases	480	0.06	0.05	0.00	0.22	480	0.05	0.04	0.00	0.19
Infectious & Parasitic Diseases	480	0.04	0.07	0.00	0.36	480	0.04	0.06	0.00	0.32
Nervous & Digestive System Diseases	480	0.01	0.02	0.00	0.14	480	0.01	0.02	0.00	0.11
Abnormal Clinical Laboratory Symptoms	480	0.01	0.02	0.00	0.11	480	0.01	0.02	0.00	0.11
Other Causes	480	0.05	0.04	0.00	0.22	480	0.04	0.04	0.00	0.23
Men										
External Causes	480	0.69	0.30	0.03	1.60	480	0.65	0.33	0.03	1.57
Tumors	480	0.08	0.05	0.00	0.26	480	0.08	0.05	0.00	0.29
Circulatory & Respiratory System Diseases	480	0.12	0.08	0.00	0.45	480	0.11	0.08	0.00	0.40
Infectious & Parasitic Diseases	480	0.10	0.17	0.00	0.91	480	0.09	0.14	0.00	0.76
Nervous & Digestive System Diseases	480	0.02	0.03	0.00	0.17	480	0.02	0.03	0.00	0.14
Abnormal Clinical Laboratory Symptoms	480	0.04	0.04	0.00	0.24	480	0.04	0.04	0.00	0.22
Other Causes	480	0.08	0.06	0.00	0.44	480	0.07	0.06	0.00	0.40

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 B3: Descriptive Mortality Statistics for Individuals aged 30-45

	Treated					Control				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Women										
External Causes	480	0.11	0.07	0.00	0.41	480	0.10	0.06	0.00	0.34
Tumors	480	0.29	0.18	0.00	0.85	480	0.27	0.17	0.00	0.87
Circulatory & Respiratory System Diseases	480	0.09	0.06	0.00	0.35	480	0.09	0.06	0.00	0.35
Infectious & Parasitic Diseases	480	0.08	0.08	0.00	0.46	480	0.08	0.08	0.00	0.46
Nervous & Digestive System Diseases	480	0.03	0.04	0.00	0.22	480	0.03	0.04	0.00	0.22
Abnormal Clinical Laboratory Symptoms	480	0.02	0.03	0.00	0.15	480	0.02	0.03	0.00	0.23
Other Causes	480	0.06	0.05	0.00	0.26	480	0.06	0.05	0.00	0.22
Men										
External Causes	480	0.49	0.16	0.11	1.22	480	0.47	0.16	0.10	1.21
Tumors	480	0.27	0.18	0.00	0.96	480	0.25	0.16	0.00	0.93
Circulatory & Respiratory System Diseases	480	0.27	0.14	0.00	0.80	480	0.25	0.13	0.00	0.69
Infectious & Parasitic Diseases	480	0.29	0.28	0.00	1.59	480	0.26	0.25	0.00	1.54
Nervous & Digestive System Diseases	480	0.10	0.08	0.00	0.40	480	0.10	0.07	0.00	0.38
Abnormal Clinical Laboratory Symptoms	480	0.07	0.05	0.00	0.29	480	0.07	0.06	0.00	0.37
Other Causes	480	0.09	0.06	0.00	0.28	480	0.09	0.06	0.00	0.31

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 B4: Descriptive Statistics of the Survey on Health and Sexual Habits

	Treated					Control				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Women										
Ever Used Injectable Drugs	1004	0.01	0.09	0.00	1.00	729	0.01	0.12	0.00	1.00
Age at First Sexual Relationship	855	19.44	3.19	13.00	39.00	629	19.50	3.38	11.00	35.00
Total Number Sexual Partners	958	1.87	1.15	1.00	5.00	703	1.83	1.13	1.00	5.00
Ever Taken VIH Test	995	0.34	0.48	0.00	1.00	723	0.33	0.47	0.00	1.00
Knows Results of VIH Test	1003	0.33	0.47	0.00	1.00	729	0.32	0.47	0.00	1.00
Women										
Ever Used Injectable Drugs	884	0.03	0.18	0.00	1.00	603	0.03	0.17	0.00	1.00
Age at First Sexual Relationship	771	18.46	3.38	10.00	40.00	529	18.59	3.35	12.00	40.00
Total Number Sexual Partners	837	2.97	1.48	1.00	5.00	565	3.00	1.52	1.00	5.00
Ever Taken VIH Test	883	0.23	0.42	0.00	1.00	597	0.20	0.40	0.00	1.00
Knows Results of VIH Test	883	0.22	0.42	0.00	1.00	602	0.19	0.39	0.00	1.00

Source: Survey on Health and Sexual Habits (2003), 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 B5: Descriptive Statistics of the National Survey of Health

	Treated					Control				
	Observations	Mean	Std. Dev	Min.	Max.	Observations	Mean	Std. Dev	Min.	Max.
Women										
Smokes	1453	0.32	0.47	0.00	1.00	1503	0.33	0.47	0.00	1.00
Ex-smoker	1453	0.22	0.42	0.00	1.00	1503	0.24	0.43	0.00	1.00
Never smoked	1453	0.43	0.49	0.00	1.00	1503	0.40	0.49	0.00	1.00
Drinks	1449	0.77	0.42	0.00	1.00	1500	0.78	0.42	0.00	1.00
Drinks daily	989	0.08	0.27	0.00	1.00	1040	0.09	0.29	0.00	1.00
Drinks less once month	989	0.42	0.49	0.00	1.00	1040	0.42	0.49	0.00	1.00
Men										
Smokes	1210	0.38	0.49	0.00	1.00	1231	0.38	0.49	0.00	1.00
Ex-smoker	1210	0.24	0.43	0.00	1.00	1231	0.25	0.43	0.00	1.00
Never smoked	1210	0.34	0.47	0.00	1.00	1231	0.34	0.47	0.00	1.00
Drinks	1210	0.91	0.29	0.00	1.00	1228	0.90	0.30	0.00	1.00
Drinks daily	1038	0.20	0.40	0.00	1.00	1036	0.20	0.40	0.00	1.00
Drinks less once month	1038	0.23	0.42	0.00	1.00	1036	0.26	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.