With Booze, you Lose:
The Mortality Effects of Early Retirement

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Abstract

This study analyzes the effect of early retirement on male mortality. I exploit two reforms in Switzerland, which allowed men as of a certain cohort to retire one and two years before the statutory retirement age. This generates two sharp eligibility cutoff dates, which I use in a regression discontinuity design. I draw from two full sample administrative data sets: the mortality and the old age insurance register. Retiring two years before the statutory retirement age increases mortality on average by 2 percentage points per year and accumulates to 41 percentage points until the age of 83. Heterogeneity analysis reveals that the effect is driven by lifestyle diseases such as alcohol dependence and respiratory diseases related to smoking. The effects is largest for unmarried men and for men living in the German-speaking part of Switzerland who generally exhibit a stronger social norm towards work than men in the Latin-speaking part. Also, there is no effect heterogeneity regarding income, which suggests that the negative health effect is not caused by a loss in income due to retirement. The results support the lifestyle hypothesis suggesting that retirement increases mortality due to a loss of structure and a concomitant unhealthy lifestyle.

Keywords: early retirement, health behavior

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Retirement may be looked upon either as a prolonged holiday or as a rejection, a being thrown on to the scrap-heap.

Simone De Beauvoir

1 Introduction

Retirement brings a great chunk of free time. Time to fill with inspiring and gratifying activities, beyond the vicissitudes of day-to-day work. But retirement might cut both ways: While it can relieve retirees from strenuous and potentially harmful work, it also bears the risk that retirees suffer from a loss of structure and take up an unhealthy lifestyle to cope with the void.

Knowing how retirement affects health is not only important for public health, but also for the pension system. Demographic change will require reforms such as increasing the statutory retirement age to ease financial pressure of the pension system. If retiring later in life increases life expectancy, reforms that increase the retirement age have to be even stronger to compensate for the longer annuity period. On the other hand, if retiring later deteriorates health, an increase in health care costs could offset savings in pension.

Retirement is not a clean-cut treatment, but rather a package of changes—a package that can differ from person to person and from country to country. Most prominently, retirement not only increases the amount of *leisure* time but also decreases available *income*. When detecting an effect of retirement on health, it is vital to know what part of the retirement package triggered the effect. Is the gained leisure time used for harmful activities, as advocates of the lifestyle hypothesis suggest? Or is it the loss in income that deteriorates health, as implied by the income hypothesis?

In this paper, I study the causal effect of early retirement on mortality of men. Specifically, I test for empirical support of the lifetime or income hypothesis. To do so, I analyze effect heterogeneity by causes of death, geography, civil status, and lifetime income to shed light on the underlying mechanisms.

I address endogeneity by implementing a regression discontinuity design (RDD). Thereby, I exploit two reforms in Switzerland that allowed men as of a certain cohort to withdraw public pension one and two years before the statutory retirement age. The first reform allowed men born in the year 1933 to opt for early pension withdrawal in the year 1997–one year before the statutory retirement age; at 64 instead of 65. The second reform allowed men born in the year 1938 to opt for early pension withdrawal in the year 2001 two years before the statutory retirement age, at 63 instead of 65. Those two reforms generate two sharp cutoffs at the end of the birth years 1932 and 1937. The assignment to the policy is therefore as good as random around the birth cutoff.

I link two full sample administrative data sets. The first data set is the death register covering the universe of deaths and its causes in Switzerland. The second data set is the social security earnings register, which includes information about retirement and life-time income.

The results show a strong and significant increase in mortality for the reform that allowed men to retire two years before the statutory retirement age. The absolute risk of dying until between the start of the early retirement at 63 up to 83 accumulates to 41 percentage points. This corresponds to an yearly increase in absolute mortality risk of 2.05 percentage points. In contrast, retiring one year before the statutory retirement age does not lead to a significant increase in mortality. The result survive a battery of robustness tests. I do not find any effect at other cutoffs, nor do I find an effect for women at the reform cutoff. This is reassuring because women were not targeted by the policy. Also, there is no effect on mortality before the start of the policy in 2001. The causal results are qualitatively similar but quantitatively stronger than the descriptive results generated by a logit or survival model. This suggests that it is rather the healthy who select into early retirement. This suggestions is underpinned by the empirical finding that rather those men with higher life-time income opt for two years of early retirement–as income if often correlated with health.

The inquiry on the mechanisms behind the increased mortality supports the lifestyle hypothesis. Looking at mortality causes and concomitant diseases, lifestyle diseases such as alcohol dependence or chronic airways obstruction (COPD) reveal a severe and significant effect. At the same time, frequent causes of deaths that are less likely to be affected by lifestyle behavior, such as accidents or infectious diseases do not change. Furthermore, single men are more likely to have an increased mortality when retiring earlier. Also, the effect is utterly driven by men residing in the German-speaking part of Switzerland. Previous investigations propose that the social norm towards work is stronger in the German than in the Latin culture, therefore, ending work is likely to have more negative consequences in the German area (Eugster et al., 2011). When looking at heterogeneity by lifetime income, there is no evidence that the effect depends on income. If anything, the effect is strongest in the middle and at the top of the income distribution which indicates that the loss of income plays less of a role in increased mortality. Taken together, the findings suggest that early retirement decreases life expectancy because of a loss of structure followed by unhealthy coping behavior.

The finding that alcohol consumption plays an important role in increasing mortality in retirement is not surprising. According to the Swiss Federal Office of

Statistics, alcohol consumption increases sharply around the age cut-off 65, which is the standard retirement cutoff for men in Switzerland (BFS, 2019). While «only» 19 percent of the male respondents in the age group 55 to 64 report to drink alcohol daily, this share is 34 percent for the men in the age group 65 to 74. Furthermore, the share of men with harmful chronic alcohol consumption is highest for this age group shortly after retirement. Also in other countries, several studies document an increase in alcohol consumption around the retirement age (Zins et al., 2011; Zantinge et al., 2014; Wang et al., 2014; Halonen et al., 2017).

I add to the literature in several ways. First, I use a credible identification strategy allowing to differentiate short- and long-term effects: a regression discontinuity design (RDD) around a random date of birth cutoff. Most studies that use credible regression discontinuity designs utilize the default retirement age as a cutoff. This only grants to investigate mortality effects in a window around the cutoff. While this is a plausible identification strategy within a short-term window, it misses mortality effects that acquire later in life. Importantly, my time period is long enough to identify long-term effects, as the reforms took place in 1997 and 2001 and my data ends in 2019. Second, I use an objective health measure: administrative mortality data. Compared to the often used survey data, this has the advantage, that it does not suffer from justification bias, measurement error, or sampling issues. Third, the unique cultural setting of Switzerland, which inherits the border of the two largest cultural groups in Europe (German and Latin), allows to look at heterogeneous effects across cultures. Those groups are especially interesting because it has been documented that the norm towards work is considerably higher in the German-part of Switzerland. Thus, the loss of work could therefore play an important mitigator. If the effects are different within the same setting, the whole literature on health effects of retirement must be cautious when transferring findings from country to country. Fourth, analyzing two reforms with different dosages of early retirement within the same setting allows to shed light on heterogeneous effects regarding the length of early retirement. Fifth, I study the effect of voluntary retirement. For example, Kuhn et al. (2020) study (mainly) the effect of forced retirement. While this is interesting per se, from a policy point of view, voluntary early retirement might be more relevant because it is more likely that countries will implement, or already have implemented, a flexible retirement schemes with early retirement on a voluntary basis. Sixth, using administrative data on lifetime income, I can disentangle lifestyle behavior from loss in income. Seventh, I use a reform applicable for, at least, all men. Many other studies focus on specific groups, such as male army officers or civil servants (Bloemen et al., 2017; Hallberg et al., 2015; Blake and Garrouste, 2013).

Several studies investigate the health effects of retirement— which underpins the relevance of this research question. However, results are quite mixed, ranging from beneficial, to neutral, to harmful. Potential reasons for the contradictory results might be that retirement is not a homogeneous, standardized treatment but rather a bundle whose content depends on a myriad of factors, such as age of the retirement, social status, network, and many other factors. Also, the way how the dependent variable health is defined and the identification strategy might play a role.[1]

From a methodological point of view, many studies use an instrumental variable approach to tackle endogeneity of the retirement decision with the predicted probability to retire. Several other studies use a fuzzy regression discontinuity design with age as the running variable and the statutory retirement age as the cutoff (Fitzpatrick and Moore, 2018; Eibich, 2015; Müller and Shaikh, 2018). Cer-

[1] Kuhn (2018) gives a concise overview on the literature and theory of the health effect of retirement.

tainly, those RDDs provide a credible identification strategy for short term effects of early retirement on health. However, the validity of their results is restricted to a brief window around the retirement age cutoff. It is, however, quite conceivable that many health effects manifest only after a certain time—especially when they are due to lifestyle behavior in retirement.

The outcome variable *health* can be classified as objective or subjective. Objective measures, such as mortality, hospitalization or illnesses, and subjective measures for which individuals are surveyed on the perception of their health. Even when looking at objective measures of health, the results are ambiguous. Probably most related to this paper is the work by Kuhn et al. (2020). They study a policy change in Austria that allowed workers in some regions to exit the labor force three years earlier. They find that blue-collar men are more likely to die before the age of 67, but do not find an effect for blue-collar women. Further, they estimate that an additional year in early retirement increases the risk of death before age 73 by 1.47 percentage points. Different from my study, many workers were pushed involuntarily into retirement. Fitzpatrick and Moore (2018) look at short term mortality effects in the US. Using a RDD with age as the running variable, they find a discontinuous change in mortality at the US social security eligibility age 62 of 2% for males. Some of their additional analyses suggest that the increase in male mortality is connected to associated lifestyle changes. Hernaes et al. (2013) study several reforms in Norway and find no effect of retirement on mortality.

Several studies exploit reforms for certain parts of the population. Bloemen et al. (2017) look at targeted incentive for civil servants to retire early. Similarly, Hallberg et al. (2015) look at male military officers. Both studies analyzing subpopulations find that retirement decreases mortality. Blake and Garrouste (2013) study private sector employees in France and discover that retirement increases mortality. Hagen (2018) looks at the health consequences of a two-year increase in the statutory retirement age of local government workers in Sweden and finds that the reform had no impact on mortality. Certainly, those results are not automatically valid for the entire population.

The impact on other objective health measures, such as hospital visits or health behavior, remains unclear. Behncke (2012) uses non-parametric matching and instrumental variables approach to identify the effect of retirement on health measures in the UK. She finds that retirement increases the risk of being diagnosed with a chronic condition. Specifically, it raises the risk of severe cardiovascular disease and cancer. Also, retirees have a higher risk to develop the metabolic syndrome which is considered as an important risk factor for both cardiovascular diseases and cancer. On the other hand, Insler (2014) and Eibich (2015) find that positive health behavior increases because of retirement. In a recent study, Rose (2020) finds no immediate effect of retirement on cognitive ability, health care utilization, or mortality. For Denmark, Nielsen (2019) shows that early retirement leads to decreases in GP visits and hospitalizations of 8–10% in the short run, but has no effect on mortality. Heller-Sahlgren (2017) looks at short and long run mental health effects using retirement age thresholds. The results show no short-term effects of retirement on mental health, but a large negative longer-term impact. Fé and Hollingsworth (2016) find that retirement opens the gate to a sedentary life with an impoverished social component.

Subjective health measures should be considered with care. The literature finds mostly beneficial effects of retirement on health when health is measured subjectively (Eibich, 2015; Johnston and Lee, 2009; Insler, 2014). Müller and Shaikh (2018) states that retirement affects own health positively, while the own retirement affects the health of the spouse negatively. Heller-Sahlgren (2017) finds no negative effect on mental health in the short-run, but does so in the long-run. Subjective health measures are prone to justification bias—the tendency of humans to justify their decision by denying potential negative consequences. For example, Johnston and Lee (2009) find different effects for objective and subjective health measures, even when using the same identification strategy and the same data. Thus, subjective health status should be looked at carefully. As stated by Kuhn (2018), mortality and its causes are the preferred measures available. Mainly, because there are many potential channels through which retirement affects health, and thus broader measures such as mortality should be preferred.

I structure the rest of this paper as follows. Section 2 describes the institutional setting in Switzerland and provides an overview of the data. Section 3 lays out the empirical strategy used to identify the effect. Section 4 shows the main results and provides several robustness checks. Section 5 looks at underlying mechanisms. The last section concludes.

2 Institutional Setting and Data

2.1 Public Pension in Switzerland

The Swiss Old Age Insurance System offers a full pension to anyone reaching the statutory retirement age (SRA). For men, the statutory retirement age is set at 65, for women at 64. During work-life, people contribute to the pay-as-you go pension system by paying social security taxes of 8.4% of their wage. Both employees and employers are required to pay contributions. Employee contributions are deducted directly from the salary. This contribution requirement starts from the age of 20 until the SRA. One year without contribution leads to a reduction in pension of 2.3% (1/44). Individuals without gaps in their contribution history receive a pension between roughly 14,000 CHF if average earnings are lower than 14,000 CHF and a maximum of approximately 28,000 CHF, if average earnings are higher or equal 84,000 CHF.

In the year 1997 and 2001, two reforms were introduced that allowed men to draw early pension before the statutory retirement age. In 1997, men born after December 31, 1932 were allowed to withdraw public pension at age 64 instead of 65. In 2001, men born after December 31, 1937 were allowed to take early public pension at the age of 63. The reform was known to the public several years in advance. This is because the reform was subject to a public mandatory referendum. On June 25, 1995, 60.7% of the Swiss population voted in favor of the new law. Retiring one year before the statutory retirement age comes at an actuarial reduction of the pension benefit of 6.8% per year. In case of retiring two years earlier this amounts to 13.6%.

Throughout the paper, I refer to retiring at 63 instead of 65 as retiring two years before the statutory retirement age. When retiring at 63 was introduced in 2001, men were already able to draw public pension at 64 and thus one year of early retirement. One could argue, that the reform that allowed men to retire at 63 only decreased eligible retirement age for one year. This would be true if the treatments are considered to increase linearly in intensity. However, it is more sensible to look at the two reforms as two distinct treatments instead: *Retiring at 63* and *Retiring at 64*. As I will show, the effects are quite different and also the men selecting into the treatments.

How well does early public pension withdrawal coincide with early retirement? Although pension withdrawal does not force people to stop working, the incentives are such that it is unfavorable to continue working once early retirement benefits are drawn. If individuals continue working, they are forced to pay social security taxes—even though they will never profit.[2] Under the occupational pension scheme, it is possible to take early retirement (starting from the age of

[2]There is an amount of exception: Until 1,400 Swiss Francs per month, you are not required to pay taxes.

58). This is the second pillar of the Swiss pension system. Thus, it is also possible that some retirees have stopped working before the public pension early retirement age. If this was the case, this would make the share of retirement compliers smaller than the share of early pension withdrawals. Consequently, the reduced form estimate is divided by a smaller share, and thus the local average treatment effect depicts a lower bound.

2.2 Data

I link two full administrative data set. The first data set is the retirement register. It contains information on income during work-life, public pension benefits, and early pension withdrawals. It is provided by the Central Compensation Office which is Switzerland's central implementing body for first-pillar social security. The second data set is the mortality register. It covers all deaths in Switzerland since 1969, including information on cause of death (ICD-10 codes), date of birth and municipality of residence. For the underlying research question, cohorts from 1930 to 1955 and observations years from 1990 to (mid) 2020 have been kindly provided. In addition, I use aggregated census data issued by the Federal Office of Statistics to measure the number of persons alive in the year 1990 per day of birth.

Table 1 provides an overview of the data. Column (1) shows that included cohorts span from 1930 to 1955. 74% of men die before the age of 80. The mean age of death is 75 years. Column (2) shows the sample of men opting for early retirement at age 64. Here the cohorts span from 1933 to 1955, as early retirement at 64 is first available for cohort 1933. The mean age of death is 73 years. Column (3) looks at the sample of men retiring at age 63. Here, the cohorts start at 1938. Although younger than the ones from column (2), more of those men are dead in 2019. And almost all deaths occur before the age of 80. The real lifetime income

Table 1: Summary Statistics

	(1)			(2)			(3)		
	All Men			***Early Retirement at 64***			***Early Retirement at 63***		
	Cohorts: 1930 - 1955			Cohorts: 1933 - 1955			Cohorts: 1938 - 1955		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Share Death until 2019	0.22	0.42	1,763,261	0.11	0.32	120048	0.14	0.34	705,11
Share Death Age < 80	0.74	0.44	393,292	0.85	0.36	13713	0.98	0.13	9,526
Mean Age at Death	75.38	6.21	393,292	73.31	5.69	13,713	70.21	4.49	9,526
Average Real Income	60,368	65,411	176,3261	53,908	32,233	12,0048	57,239	353271	70,511
Observations	1,763,261			120,048			70,511		

This table shows the sample available for the analysis. Column (1) shows that included cohorts span from 1930 to 1955. Of all men, 22% are dead. Furthermore, 74% of men die before the age of 80. The mean age of death is 75 years. Column (2) shows the sample of men opting for ER retirement at age 64. Here the cohorts span from 1933 to 1955, as early retirement at 64 is only available as of cohort 1933. As they are younger, 11% are dead at the end of the sampling period. The mean age of death is 73 years. Also, real life time income (before retirement). Column (3) looks at the sample of men retiring at age 63. Here, the cohorts start at 1938. Although younger than the ones from column (2), more of those men are dead in 2019. An almost all of them that die are dead before the age of 80. The real life time income is, however, higher then from the ones retiring at 64.

is, however, higher than for men retiring at 64. Compared to the income of men retiring at or after the mandatory retirement age, it is pretty similar. Remember that lifetime income is measured before the retirement. Thus, retiring two years earlier mechanically leads to a lower lifetime income. If one accounts for this, lifetime income for those retiring at age 63 is similar to those men retiring at 65 or later. Figure A1 supports this finding: Early retirement take up is almost independent of income rank.

3 Empirical Strategy

3.1 Identification

Simply regressing mortality on early retirement is likely to yield biased results. Reversed causality is one bias: Whether one retires early might itself depend on health status and subsequently on mortality. If unhealthier people are more likely to retire early, I would overestimate the effect of retirement on mortality. The results would also be biased because of omitted variables: Early retirees are likely to differ in characteristics that influence both health and the retirement decision.

To circumvent those biases, I use exogenous variation induced by two policy changes. Whether men were eligible for early retirement changed discontinuously at a certain date of birth. At the extreme, if a man were born a few seconds later at New Year's Eve 1932/33 or 1937/1938, he was eligible for early pension withdrawal.

I will focus on two effects. First, the effect of the policy introduction on overall mortality: Does the introduction of the policy affect mortality? This makes up a sharp regression discontinuity design (SRD). Second, the effect of early retirement on mortality: How does early retirement affect mortality? Here, the policy serves as an instrument for early retirement and the research design constitutes of a fuzzy regression discontinuity design (FRD).[3]

The identifying assumptions are distinct for the SRD and the FRD, in the sense that FRD requires more assumptions than SRD. The first assumption is the *stable unit treatment value assumption* (SUTVA). It states that the potential outcomes for each person are unrelated to the treatment status of other individuals. If spillovers are present, this assumption can be violated. In my study this would be the case if the early retirement (eligibility) affected the mortality of the control group, for example due to general equilibrium effects. However, it is unlikely that large general equilibrium effects are present because around the treatment

[3]The two estimates are closely linked. The SRD constitutes the reduced form estimate in the FRD. The difference is that in the FRD the effect of the policy on mortality is divided by the share of men reacting to the policy (i.e. the first stage).

cutoff only one male cohort was eligible and only around 4% of men actually opted for early retirement.

The second assumption is the *continuity of conditional regression function*. This assumption implies that the running variable (date of birth) can be related to the outcome variable, but its association has to be smooth. This is the case when using mortality as an outcome and date of birth as a running variable because mortality is on average a continuous function of age. It also means that absent the introduction of the policy, mortality would not have changed at the cutoff. Importantly, this requires that other determinants of mortality are not allowed to jump at the cutoff. Although I can never prove that there are not some unobserved determinants changing at the cutoff, there is to the best of my knowledge no other reform—for example in the realm of army, school, or health— related to this cutoff. Conveniently, I can use women to perform a placebo test at the cutoffs because they were not affected by the reform. I do not see an effect for women. Thus, if there are some unobserved determinants changing discontinuously at the cutoff, they would need to only affect men and not women. This certainly reduces the probability of a violation of the continuity assumption. Furthermore, graphical analysis shows that the outcome variable does not significantly jump at other end-of-year cutoffs and at other random cutoffs (see Figure A2 and Table A1).

Another assumption is that the running variable cannot be *manipulated*. Here the running variable is date of birth. Thus, manipulation can be ruled out by construction.[4]

[4]Birth scheduling at the end-of-year cutoff has been documented. It is possible that parents at the end-of-year 1937 manipulated the date of birth of their offspring for some other reasons, e.g. school grade optimization. If this was the case, more people with date of birth in January should be observed. However, as my outcome variable is relative to the number of men with a certain day of birth, this bias is irrelevant in this context.

The FRD adds two more assumptions *monotonicity* and *relevance of the first stage*. *Monotonicity* means that no one is discouraged from treatment by crossing the cutoff. Crossing the cutoff cannot cause some units to take up and others to reject the treatment. In this study, this means that introducing the early retirement option does not cause people to work longer. This could be violated if introducing the policy led some people to work longer, e.g. because the equilibrium wage increases because of some people leaving the workforce. If anything, those effects are likely to be tiny as less than 5% of a male cohort opt for early retirement.

Another important requirement to estimate a (robust) effect in the FRD, is that the share of people reacting to the policy is sufficiently large. This is the same as saying that the *predictive power of the instrument* on the treatment take-up has to be sufficiently large (i.e. relevant for the first stage). Otherwise the instrument may be weak and the coefficients artificially blown up. As shown in Figure A3, the share of people reacting to the policy is around 4% and thus sufficiently large. Importantly, the cutoff is very predictive of taking early retirement, because there is perfect non-compliance on the left side of the running variable.

The resulting effect of the FRD is a local average treatment effect (LATE). That is, the average effect of the policy on men that reacted to the policy. This does not necessarily translate to an average treatment effect (ATE), as the complier population is likely to differ in characteristics from the non-complier population. The LATE would only translate to an ATE if we would assume homogeneous effects—which would impose a very strong assumption.

3.2 Estimation

The primary goal of this study is to estimate the following reference model and retrieve the parameter τ which aims to capture the effect of early retirement on mortality.

$$Mortality_{itx} = \alpha + \tau EarlyRetirment_{itx} + u_{itx} \tag{1}$$

where i stands for individual, t for (calendar) time, and x for date of birth. The dependent variable $Mortality_{itx}$ measures the probability to die at calendar day t for each day of birth x. As explained above, τ is likely to be biased in this regression because $cov(EarlyRetirement_{itx}, u_{itx}) \neq 0$.

To describe the non-causal relationship between early retirement and mortality, I also show a survival model. In this model, $Mortality$ depicts the *hazard* to die at day t conditional on having survived up to day t. Thereby, I use two distributions to model the hazard function: The Weibull and the Gompertz distribution:[5]

$$Mortality = \lambda(t|a) = exp(a'\beta)t^{(\alpha-1)} \tag{2}$$

where $\lambda(t|a)$ depicts the hazard to die at day t conditional on having survived up to day t and conditional on a. α is a parameter for duration dependence, and a a vector for covariates — such as early retirement or year of birth.

To circumvent endogeneity, I estimate the treatment effect within a regression discontinuity design (RDD). Let $\tilde{x}$ be the running variable *date of birth* centered around the cutoff dates x_0, precisely $\tilde{x} = x - x_0$. T_i indicates, if a man is born after the cutoff date and is therefore eligible for early retirement. This yields the following equation for the SRD which is the same as the reduced form in the FRD:

$$Mortality_x = \beta_0 + \beta_1\tilde{x} + \delta T_x + \beta_2 T_x\tilde{x} + \epsilon_x \tag{3}$$

[5]The Gompertz distribution differs from the Weibull model in that logarithmic hazards are assumed to be a linear function of survival times with $\lambda(t) = \lambda exp(\gamma t)$ where $\lambda > 0$

where $Mortality_x$ measures the probability to be dead at the end of the data period per date of birth x. Specifically, for every date of birth, I calculate the sum of men dying between 1990 and 2019 and divide it by the number of men alive in 1990:

$$Mortality_x = \frac{1}{n_{x,1990}} \sum_{i \in x}^{t \leq 2019} \mathbb{1}[Death_i = 1]$$

Another measure for mortality would be the sum of deaths per day. However, I use this relative measure in the main specification to enhance robustness and facilitate interpretation. It makes the measure even more robust to manipulation around the cutoff, such as birth scheduling, and helps against unlikely compositional changes (e.g. immigrants that due to administrative issues all got assigned the same date of birth). Also, it helps to interpret the effect, because the discontinuity in the outcome variable at the cutoff measures the increase in absolute risk of mortality.

In Equation 3, δ shows the net effect of the policy change T on overall male mortality in the population. While this effect is interesting per se, the main goal is to determine the effect of the policy on a retiree's mortality. The policy cutoff thereby serves as an instrument for early retirement. This is done in a two stage least square model (2SLS).

I start with the following first stage equation to determine the share of men reacting to the policy:

$$EarlyRetirement_x = \gamma_0 + \gamma_1 \tilde{x} + \pi T_x + \gamma_2 T_x \tilde{x} + \nu_x \tag{4}$$

where the coefficient π shows the share of men reacting to the policy around the cutoff for each date of birth x.

Finally, the LATE is estimated by the following equation:

$$Mortality_x = \kappa_0 + \kappa_1 \tilde{x} + \kappa_2 T_x \tilde{x} + \tau \widehat{ER}_x + \xi_x \tag{5}$$

where τ captures the causal effect of early retirement on male mortality.

In the above equations, the functions of date of birth x are modeled as a piece-wise linear trend. This allows trends to differ before and after the cutoff as suggested by Lee and Lemieux (2010). Further, I will also model mortality with a piece-wise quadratic trend—which could be more appropriate in the case of mortality. Mortality is usually modeled with a Gompertz distribution which resembles more a quadratic function. Also, I estimate the models using different Kernels. A more important role, however, plays the bandwidth h. To trade-off bias and variance, I use the data driven bandwidth selection method proposed by Calonico et al. (2014). Nevertheless, I will test whether the results are robust to shorter and longer, hand-picked bandwidths.

4 Results

4.1 Survival Model and Logit Results

I start with the non-causal results of the logit and survival model. Figure 1 compares cumulative death hazard estimates of men retiring early (red) to men without early retirement (blue). In Panel (a) early retirement is defined as *two years*, in Panel (b) as *one year*. The two figures show that the hazard is distinct for two and one year of early retirement. Men retiring two years earlier are at higher risk of dying at any age compared to those retiring at 65 or later. In contrast, men retiring one year earlier are at lower risk of dying at until the age of roughly 82, but at higher risk after.

Table 2 shows the results of the logit and survival models. Columns (1) and (2) show the estimated coefficients as odds ratios of a logit regression on the probability to die before the year 2020 while holding the year of birth fixed. Men retiring at the age of 63 have 60% increased odds of dying before 2020. This is approximately a 32% increase in relative risk.[6] In contrast, men retiring at the age of 64 have 22% smaller odds of dying before 2020 (16% smaller relative risk). Column (2) adds controls for lifetime income, which decreases the odds of dying for both retirement schemes.

The logit estimates in columns (1) and (2) measure the cumulative probability to die before the year 2020 (end point) and can be prone to sampling errors, such as timing of the end point. Survival models are less prone to this bias. Columns (3) to (6) of Table 2 show different specifications of survival models with the hazard function defined as death at time t conditional on having lived up to time t. The models differ in the chosen distribution and control variables. The death hazard increases by around 50% in all survival model specifications when retiring at 63 compared to 65 or later. Similarly, men retiring at 64 have on average a 13% smaller hazard of dying.

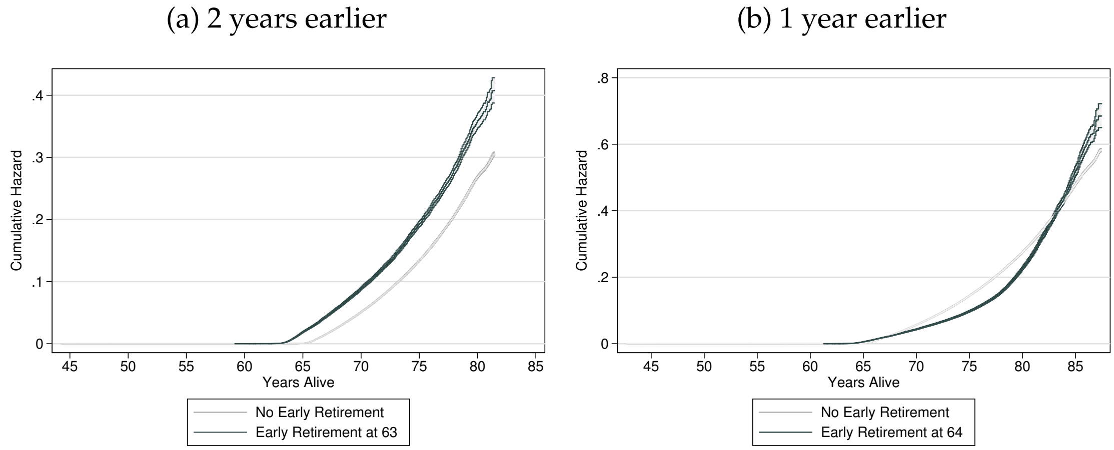
4.2 Regression Discontinuity Results

4.2.1 First Stage

Figure A3 shows the share of men opting for two and one years of early retirement as a function of date of birth. As given by the rule of law, no men take up early retirement before the eligibility cutoff. Table A2 shows estimates on the share of men opting for early retirement at the date of birth eligibility cutoff— as stated in Equation 4. Over 4% of the male population in Switzerland opt for early

[6]To approximately convert the odds ratio from the logit model into relative risk, I use the formula proposed by Zhang and Kai (1998): $RR = OR/(1 - p_0 + p_0 * OR)$, which is $1.60/(1 - 0.34 + 0.34 * 1.60) = 1.32$

Figure 1: Nelson-Aelen cumulative hazard estimates



This figure shows the non-parametric, cumulative hazard estimates for men retiring two years (a) and one year earlier (b) compared to those retiring at the statutory retirement age of 65 or later. Panel (a) shows that the cumulative hazard to die is always higher for men retiring at age 63. Panel (b) shows that men retiring one year earlier have a lower risk of dying at the beginning of retirement, but a higher risk at the end of the period.

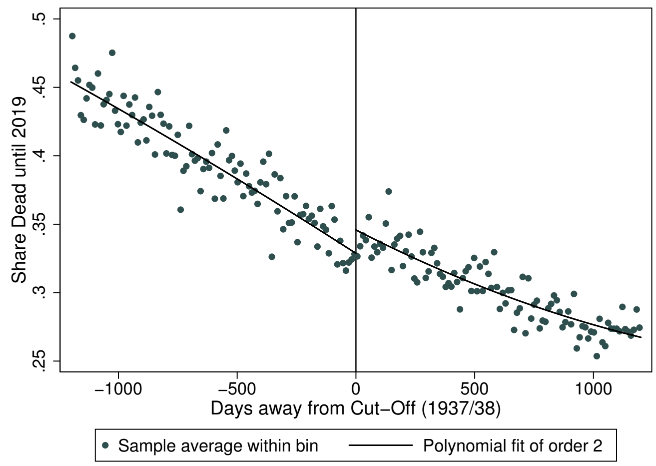
retirement at 63 at the eligibility cutoff. For retirement at 64, the share is smaller with 2.2 to 2.9%. For both discontinuities, retirement age 63 and 64, the jump is highly significant. This is due to the fact that there is perfect non-compliance before the cutoff. Table A3 shows the number of men opting for early retirement by year of birth. The data driven bandwidth selection in the FRD estimation proposes a bandwidth of 873 days (first-order polynomial) and 1068 (second-order polynomial) for the analysis of the effect of two years early retirement. This means that roughly 6,000 men are included in the sample for early retirement. However, for one year of early retirement, only around 1'500 men are included in the analysis.

4.2.2 Reduced Form

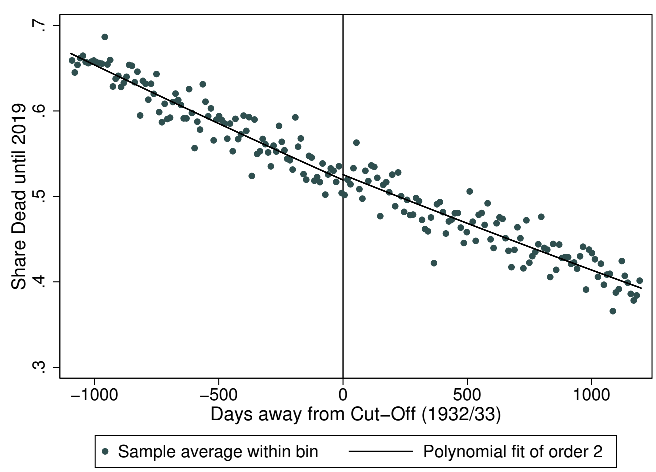
Figure 2 shows, for each day of birth, the share of men that died before 2020. In Panel a, the eligibility cutoff date is for two years early retirement, while Panel b

Figure 2: Reduced Form

(a) 2 years earlier



(b) 1 year earlier



This figure shows the reduced form estimates for the two policy reforms: retiring at age 63 (a) and retiring at age 64 (b). The vertical axis measures the share of men dead until 2019 per day of birth x. The horizontal axis measureas the days away from the cutoff.

Table 2: Logit and Surivial Model Results (non-causal)

	Death before 2020		Survival Models (Hazard Rate)			
	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	Logit	Weibull	Weibull	Weibull	Gompertz
	b/se	b/se	b/se	b/se	b/se	b/se
Mortality						
Early Retirement at 63	1.604***	1.546***	1.508***	1.535***	1.483***	1.514***
	(0.019)	(0.020)	(0.016)	(0.017)	(0.016)	(0.016)
Income Quintile FE	No	Yes	No	No	Yes	No
Birth Year FE	Yes	Yes	No	Yes	Yes	Yes
Mortality						
Early Retirement at 64	0.781***	0.690***	0.854***	0.824***	0.740***	0.867***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Income Quintile FE	No	Yes	No	No	Yes	No
Birth Year FE	Yes	Yes	No	Yes	Yes	No
Distribution	Logit	Logit	Weibull	Weibull	Weibull	Gompertz
N (individuals)	1,762,199	1,619,208	1,762,199	1,762,199	1,619,208	1,762,199

Exponentiated coefficients (odds ratio for columns (1) and (2) and hazard ratio for columns (3) to (6))

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the association between early retirement and mortality. Columns (1) and (2) show the exponentiated coefficient (odds ratio) of a logit regression of dying before the year 2020 while controlling for birth year fixed effects. In addition to column (1), column (2) controls for lifetime income quintile. Columns (3) to (6) show the hazard ratio of several survival models on the risk of dying conditional on having survived up to a certain period (hazard rate). Columns (3) to (5) use a Weibull distribution, while column (6) uses a Gompertz distribution.

looks at one year early retirement. The points are binned averages, while the line is a quadratic approximation. For two years of early retirement, the discontinuity is clearly visible. At this end of birth year cutoff (1937/1938), mortality increases discontinuously. In Panel b, there is almost no discontinuity visible.

The upper half of Table 3 shows several estimates for the reduced form effect of the two years earlier retirement reform, as specified in Equation 3. The estimates are mostly significant and around 1.5 percentage points. Thus, the introduction of the policy increases the share of men dying until 2019 by roughly 1.5 percentage points. On average, 35% of men born around 1937/1938 are dead until 2019. Thereupon, in relative terms this increase amounts to a 4% increase in mortality. The shape of Figure 2 suggests that a quadratic polynomial is a better approximation. This leads to Column (2) as a baseline estimate as it combines data driven bandwidth selection with a second order polynomial approximation. Apart from Column (5), the estimates also center around the point estimate from Column (2).

As expected by visual inspection of Figure 2b, the discontinuity for the policy that introduced one year early withdrawal is not significant. Nevertheless, the point estimate is positive which could indicate a discontinuity, but a lack of power due to the relatively poor first stage.

4.2.3 Local Average Treatment Effect (LATE)

Let us now turn to the effect of most interest: How does early retirement affect mortality for those actually opting for early retirement, that is, the local average treatment effect (LATE) of early retirement on mortality as specified in Equation 5.

The estimates for the LATE are shown in Table 4. Again, the upper half of the Table shows the LATE for early retirement at 63, while the lower half shows the LATE for early retirement at 64. Unsurprisingly, given the reduced form and first stage effect, the effect is strong and significant for early retirement at 63 and insignificant, but positive, for retirement at 64.

The effects for early retirement at 63 range from a 23 to 44 percentage points increase in mortality. Per year of the duration of retirement (20 years)[7], this

[7] from age 63 (start of early retirement) to age 83 (end of data period)

Table 3: Reduced Form Results

	(1)	(2)	(3)	(4)	(5)	(6)
Mortality (Share Men Dead until 2019)						
Above Cutoff for 2 Years	0.015***	0.017**	0.018***	0.017 *	0.010***	0.015***
	(0.005)	(0.007)	(0.007)	(0.010)	(0.004)	(0.005)
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left	873	1068	600	600	2000	2000
BW right	873	1068	600	600	2000	2000
Observations	7670	7670	7670	7670	7670	7670
Mortality (Share Men Dead until 2019)						
Above Cutoff for 1 Year	0.015	0.006	0.012	0.015	0.004	0.007
	(0.009)	(0.014)	(0.008)	(0.011)	(0.005)	(0.007)
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left	408	413	600	600	2000	2000
BW right	408	413	600	600	2000	2000
Observations	7670	7670	7670	7670	7670	7670

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the overall effect of the policy introduction on mortality (reduced form). The variable *Above Cutoff* shows the discontinuity in mortality (death before 2020) at the eligibility date of birth. All columns use a triangular kernel, but differ in the order of the polynomial, the bandwidth (BW) selection method, and the selected bandwidth.

amounts to an average increase in mortality by 1.15 to 2.20 percentage points. This translates to an increase in accumulated relative risk of dying before the age of 83 by a factor between 0.62 to 1.22 or on a yearly basis to between a 0.031 and 0.056 increase in relative risk.[8] As suggested in the previous section, Column (2) serves as a baseline estimate. This baseline estimate suggests that mortality increases by 41 percentage points or 2.05 percentage points per year. In relative terms this amounts to an increase of relative risk by a factor 1.17 or 0.058 per year in retirement. [9] How can this mortality effects be put in context? For example, smoking increases relative risk of premature death factor 2 to 3 (Carter et al., 2015). Thus, two years of early retirement is almost about half as dangerous as smoking.

How do those results compare to the results from the previously shown survival model and logit? For two years of early retirement, the logit model attributes an increase in the relative risk of dying of around 0.31, while the causal effect ranges from 0.62 to 1.22. Although the direction of the effect is the same, the estimates of the FRD are considerably larger. This suggests that contrary to suggestions of previous studies, selection into early retirement is not driven by unhealthy, but rather by healthier men. Even though, I do not have information on pre-retirement health, I can use income as a proxy for health status. Table 1 shows that men retiring at 63 have an average lifetime earnings of CHF 57,239 (around USD 57,239). Compared to the full sample of men, this is only smaller at first sight. Men retiring at 63 also have 2 years of income missing. Thus, the difference in average lifetime earning equalizes if one would add the two years of missed

[8] The relative mortality risk has to be put in context with the (male) life expectancy in general. Worldwide, Switzerland ranks first in male life expectancy with an average life expectancy of 82.42 years. To compare, male life expectancy in Germany is 79.62 years or 76.61 years in the US (World Bank, 2018). Thus, the baseline mortality is low in and consequently, relative risks are estimated to be high

[9] The mean share of men dead right before the cutoff is 0.35, thus the relative risk is $(0.35 + 0.41)/0.35 = 2.17$

income.[10] This suggests that there is no selection of unhealthy men into early retirement. A similar picture shows Figure A1. It depicts the share of people retiring early by male income rank. For two years of early retirement, the share of people retiring early is almost independent of income. Interestingly, the same Figure A1 shows that retiring one year earlier shows a monotonic increase by income rank.

Figure A4 shows how the mortality effect changes over time. Mortality is significantly higher within six years of the policy reform. Within the next six years, the point estimate becomes smaller and the effect insignificant. Within the last seven years of the sample period, the effect increases again. While it is surprising that the effect already manifests within a few years, it is consistent with Fitzpatrick and Moore (2018) who find increased mortality within a short period of time—after the default retirement age.

4.3 Robustness

I perform several checks to assess the robustness of the increase in mortality due to two years of early retirement. I begin with checking whether there is a discontinuity at the male policy eligibility cutoff for women. This serves as a placebo test because the mortality of women should not jump at this cutoff. Figure A5 shows that there is no discontinuity around the 1937/1938 cutoff for women. Thus, if any other policy generated the jump at the cutoff, that policy can only have affected men.

Then, I test if there is a discontinuity at other cutoffs. First, I focus on end of birth year cutoffs. The results are shown in Figure A2. Apart from the end-of-year cutoff 1937/1938 (2 years of early retirement eligibility cutoff), there is no positive, significant change. Second, I test for discontinuities at other random cutoffs. To

[10] Average earnings per life year: $60368/40 = 1509$

Table 4: Effect of Early Retirement on Mortality (LATE)

	(1)	(2)	(3)	(4)	(5)	(6)
Mortality (Dead until 2019)						
Early Retirement at 63	0.345^{***}	0.412^{**}	0.421^{***}	0.440^{*}	0.234^{***}	0.331^{***}
	(0.130)	(0.177)	(0.159)	(0.248)	(0.084)	(0.117)
First Stage	0.042^{***}	0.041^{***}	0.042^{***}	0.040^{***}	0.042^{***}	0.045^{***}
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left/right	873	1068	600	600	2000	2000
Observations	7670	7670	7670	7670	7670	7670
Mortality (Dead until 2019)						
Early Retirement at 64	0.693	0.304	0.536	0.775	0.143	0.274
	(0.448)	(0.665)	(0.330)	(0.579)	(0.159)	(0.270)
First Stage	0.021^{***}	0.021^{***}	0.023^{***}	0.020^{***}	0.029^{***}	0.026^{***}
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left/right	408	413	600	600	2000	2000
Observations	7670	7670	7670	7670	7670	7670

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the effect of early retirement on male mortality, the local average treatment effect. The estimates indiate the percentage increase in mortality due to early retirement. All columns use a triangular kernel, but differ in the order of the polynomial, the bandwidth (BW) selection method, and the selected bandwidth.

do so, I use the method suggested by Imbens and Lemieux (2008). Thereby, I split the sample at the cutoff value x_0 and run the RDD estimation at the new cutoffs that are themselves the median of the split sample. Table A1 shows the p-values of this artificial cutoffs. None of the values differ significantly from 0. Then, I split the sample again at the new median cutoffs and create another cutoff point at the median of the new samples. At those newly created four cutoffs, there is no significant discontinuity.

Finally, I check when the mortality effect manifests. If it is indeed the early retirement treatment that triggers the mortality effect, no mortality effect should be visible before the treatment starts or before it was announced. Figure A4 measures how the effect varies along years relative to the reform. One can see that the mortality effect does not start before the implementation year of the policy and is significant thereafter.

4.4 Mechanisms

Retirement is not a uniform treatment, but a bundle of several elements that change. So, what exactly is it that increases male mortality when retiring two years earlier? To shed light on mechanisms behind the increase, I analyze which diseases drive the increase and how the effect changes by civil status, municipality of residence, and life-time income.

4.4.1 Causes of Death

Data from the death register allows to analyze which diseases are behind the increase in mortality. For each death, I observe the cause of death and concomitant diseases as ICD-10 codes. I do not distinguish between cause of death and concomitant diseases because this is often arbitrary and causes of death as well as concomitant disease are informative. For example, when categorizing a death related to alcohol diseases, this is true if alcohol related ICD-10 codes occur as cause of death or concomitant disease. For each of the disease categories, I run a regression discontinuity regression to see whether there is a discontinuity at the date of birth cutoff for this specific disease.

Figure 3a shows the effect heterogeneity on the highest aggregation level, the ICD-10 main disease groups. The increase is significant and largest for cancer and respiratory disease. Vascular diseases show a high point estimate too, but the effect is not significantly different from zero. The other groups infectious diseases, mental diseases, and injuries have smaller point estimate and do not differ significantly from zero.

Next, I explore whether diseases influenced by lifestyle behavior increase at the cutoff. Figure 3b shows the effect for diabetes 2 (ICD E11.9), diseases attributed to smoking according to Rostron (2012), diseases attributed to alcohol according to Shield et al. (2020), and drug related diseases (ICD F1). Deaths related to alcohol increase strongly at the cutoff. The same holds true for deaths related to drug intake. The point estimate for smoking is large as well, but not significantly different from zero. Interestingly, diabetes 2, which is associated with bad nutritional habits, does not increase at the cutoff.

Figure 3c digs deeper into alcohol-related causes. There is a discontinuous jump in deaths related to alcohol dependence. Also, alcohol related liver cirrhosis and alcohol liver disease show a positive point estimate but are not significantly different from zero. Figure 3d does the same for smoking-related diseases that do not show a significant effect at a more aggregate level in Figure 3b. Here, I find a marginally significant increase in COPD, the chronic airway obstruction disease. In terms of effect size, it is only little less than alcohol dependence. Also, the point estimate for lung cancer is positive, but imprecisely estimated.

Figure 3e looks at other frequent diseases or disease groups, that are less likely related to lifestyle behavior. Certainly, some of them might still be influenced or correlate with unhealthy behavior such as smoking or drinking, but one would still expect to see a smaller effect—if it is indeed unhealthy behavior that triggers the effect. Indeed, none of the effects are significant and the point estimates are in general small. Among psychological diseases not related to lifestyle are, for example, personality disorders that already manifest during childhood.

The results from the analysis of the diseases are in line with the lifestyle hypothesis. Alcohol dependence and COPD, both likely caused by excessive alcohol intake and smoking show a significant increase at the cutoff. At the same time, diabetes 2 does not increase, which suggests that unhealthy nutrition plays less of a role.

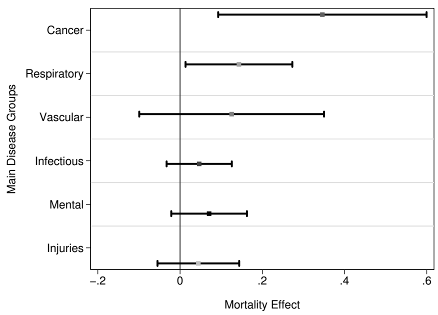
4.4.2 Income

How different is the effect regarding income? Figure 4a shows the effect for different income groups. Those are estimates individually calculated, thus the increase in mortality is divided by the probability of retiring two years earlier. The most striking feature is that the effect is precisely zero for the lowest income quintile. For all the higher income quintiles, the point estimate is positive. Although not significantly different from zero, the highest estimates are in quintiles 2, 3, and 5. Thus, there is no empirical ground to assume that the mortality effect decreases with higher income. Consequently, it is unlikely that lower financial resources mediate the higher mortality.

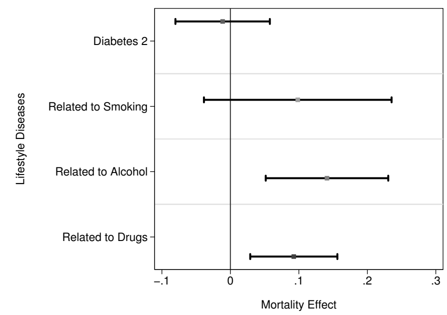
There are also other institutional settings of Switzerland that support the idea that income does not play an important role. Switzerland offers rather generous social welfare. If retirement pension and income do not cover minimum living costs, retirees are entitled to supplementary benefits.

Figure 3: Effect Heterogeneity by Diseases

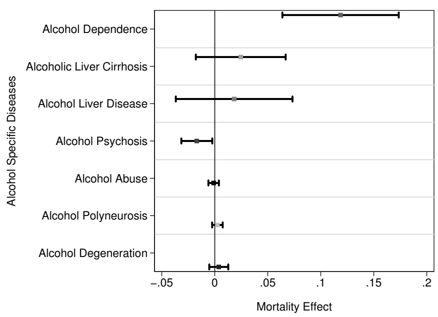
(a) Main Disease Groups



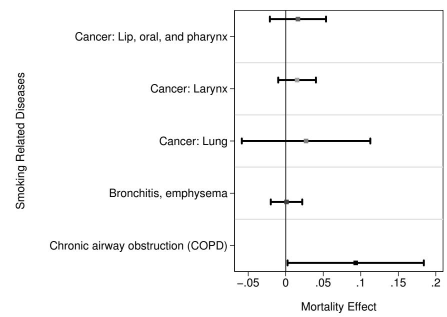
(b) Diseases related to Lifestyle Behavior



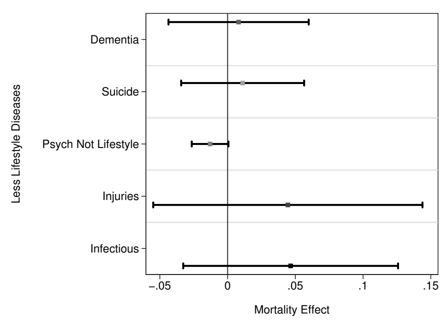
(c) Diseases related to Alcohol



(d) Diseases related to Smoking



(e) Diseases Less related to Lifestyle



This figures shows what diseases caused death or are concommitant at death. Each coefficient and its 95% interval are estimated within a fuzzy regression discontinuity designed that is specificied with a second order polynmial, triangular kernel, and automatic bandwidth selection.

Figure A6 shows that unemployment at the year 2001, when early retirement at 63 was first possible, was below 2% and lowest for individuals above the age of

55. It is therefore unlikely that unemployed selected into early retirement at 63. This yields further support that it is not a loss of income that led to the negative effect.

4.4.3 Civil Status

Figure 4b shows how the effect differs by civil status. Although a small group, the coefficient for single men is significantly positive. The point estimate for married man is higher, but the estimate is much noisier and not different from zero. The coefficient for widowed men is less noisy and not different from zero. The fact that unmarried men have a higher mortality indicates that the loss of structure plays an important role. One might speculate that whether a marriage is conducive to health depends on the very nature of the marriage. This could explain why the effect is imprecisely estimated.

4.4.4 Geography

Switzerland inherits the border between two large cultural groups of Europe: the German and the Latin culture. Thus, it provides a convenient setting to test if mortality effects are driven by culture. Figure 4c shows the coefficients of a FRD with automatic bandwidth selection for different geographic factors. The first group shows the effects of the language groups. Interestingly, the effect seems to be entirely driven by people living in German-speaking municipalities. In French- and Italian-speaking municipalities, the point estimate is very close to zero.

Cultural differences have been shown to affect labor market outcomes (Eugster et al., 2017). This indicates that preference and norms towards work are higher in the German-speaking part. Therefore, it is natural to assume that the loss of work due to retirement has stronger consequences for Swiss-Germans. These results are in line with studies from France by Bozio et al. (2020). They do not find a harmful effect of early retirement on mortality. At the same time, Kühntopf and Tivig (2012) show tentative evidence from Germany by documenting an increase in mortality due to early retirement.

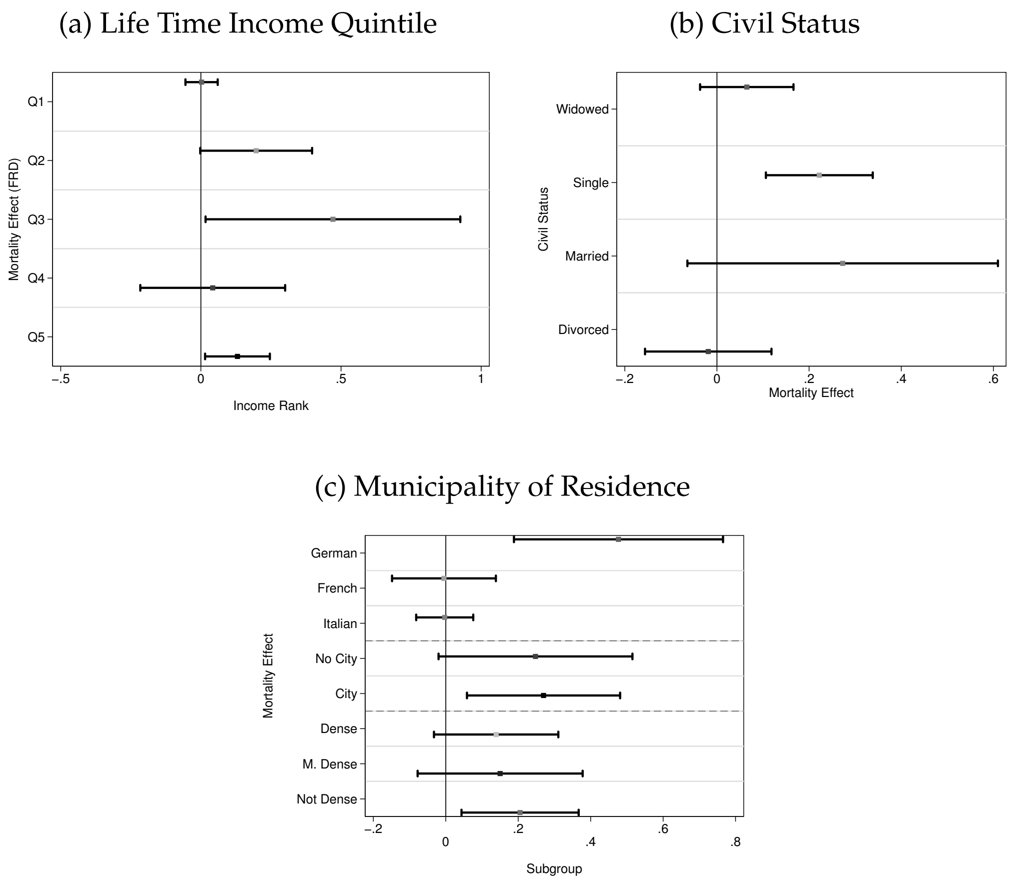
Figure 4c also shows how the effect differs between municipality characteristics, such as urbanization (city or no city) and population density. The point estimates of these characteristics are similar. The effect is, however, significant for cities and for regions with low population density. Although this could be driven by chance, one interpretation could be that the loneliness and thus lack of social control is highest in anonymous cities and in regions with low population density.

5 Summary and Concluding Remarks

This paper studies the effect of early retirement on male mortality. Because regressing early retirement on mortality is likely to yield biased coefficients, I exploit exogenous variation provided by two policy reforms. Those policy reforms allowed men born after a certain day of birth to draw public pension one and two years earlier. The treatment assignment around those cutoff dates is therefore as good as random, and I can identify the causal effect of early retirement on mortality in a regression discontinuity design. I draw from two full sample administrative data sets which include precise information on retirement, lifetime income, and mortality causes. Combining a credible identification strategy with precise data and an objective health measure allows to yield trustworthy estimates on the effect of early retirement on mortality. Compared to other studies with a credible identification that use the default retirement age as an RDD cutoff, my design has the advantage that I can analyze long-term mortality effects.

Retiring two years earlier leads to a strong increase in male mortality. I estimate that retiring two years early increases the absolute risk of dying before the age

Figure 4: Effect Heterogeneity by Personal Characteristics



This figure shows several estimates of a fuzzy regression discontinuity design for retirement at 63 (two years earlier). It is specificied as a second order polynomial, triangular kernel, and automatic bandwidth selection. Panel (a) shows the results for five quintiles of the lifetime income before retirement. Thereby, for each quintile the share of compliers is separately calculated. Panel (b) shows the FRD results by civil status. Here, the coefficients of the reduced form are not adjusted by the exact complier share, because civil status information is only available for dead men. Panel (c) looks at characteristics of the municipality of residence when diseased: the language region (German, French, Italian), whether the municipality is a city, and on the population density (dense, medium dense, and not dense).
Life time income quintiles of the 1938 have the following median life time income in 2017 Swiss Francs (almost equal to USD): $Q_1 = 16,458$; $Q_2 = 39,246$; $Q_3 = 53,172$; $Q_4 = 68,364$; $Q_5 = 98,748$

of 83 by 2.05 percentage points per year. On the other hand, retiring one year earlier does not show a significant increase. The effect of an increase in mortality for two years of early retirement is robust to several robustness checks. There are no jumps at other non-policy year cutoffs and no jumps at other random cutoffs. Further, there is no discontinuity for women at the same cutoff date. Also, the mortality effect does not materialize before early retirement at 63 was eligible. Manipulation or selection around the cutoff can be ruled out by construction because the day of birth is very hard to manipulate. Even more, it would have required a gift of clairvoyance, because the policy was announced when the date of birth was already defined.

Analyzing mechanisms and heterogeneity suggests that the increase in mortality by two years reform is driven by an unhealthy lifestyle behavior as a coping mechanism. Deaths related to alcohol dependence and COPD show a strong and significant increase. Other frequent diseases, such as injuries or infectious diseases, that are less likely to be influenced by lifestyle behavior, show no significant increase. Also, the effect is significantly higher for unmarried men, giving further strength to the "loss of structure" argument. Interestingly, the effect is entirely driven by men living in the German part of Switzerland. The German culture has in general a stronger social norm towards work. As pointed out by Eugster et al. (2017), attitudes toward work differ between language groups and it is thus likely that the effect of job loss due to retirement differs as well. I do not find evidence for the competing hypothesis arguing that the effect is driven by a loss of income due to the forgone work income and the actuarial reduction in the annuity. Heterogeneity analysis shows that the effect is highest in the middle and at the top of the income distribution.

The finding that alcohol consumption plays an important role in increasing mortality in retirement is not surprising. According to the Swiss Federal Office of Statistics, alcohol consumption increases sharply around the age cut-off 65, which is the standard retirement cutoff for men in Switzerland (BFS, 2019). While «only» 19 percent of the male respondents in the age group 55 to 64 report to drink alcohol daily, this share is 34 percent for the men in the age group 65 to 74. Also the share of men with harmful chronic alcohol consumption is highest for this age group shortly after retirement. Also in other countries, several studies document an increase in alcohol consumption around the retirement age (Zins et al., 2011; Zantinge et al., 2014; Wang et al., 2014; Halonen et al., 2017).

How do my results compare to the literature looking at mortality effects? Despite methodological differences between early retirement and retiring at the default retirement age, it is striking that many results are similar to the ones found by Fitzpatrick and Moore (2018). They look at differences in mortality around the social security cutoff at the age of 62 in the US. Although their focus is on short-term mortality differences around the age cutoff 62, they also find that the increase in mortality is highest for non-married men. Also similar to this study, is that they show that COPD and lung cancer increases. They also note that these causes of death have previously been found to be related to job loss. Therefore, one could argue that the mortality effects of early retirement could also apply to general mortality effects of retirement at the statutory retirement age.

The primary limitation of this study is its external validity. The very fact that the positive mortality effect is entirely driven by men from the German speaking part of Switzerland shows that the results cannot be carried over to other countries without limitations. This limitation is not limited to this study but applicable to all studies in this field. As such, effects might not only differ by country specific institutional settings, but also by cultural attributes. Also, I estimate a local average treatment effect (LATE) that is only instructive for those men reacting to the policy, the compliers. Although compliers do not differ in terms of income from the general population, they might nevertheless differ in other, unobserved characteristics. Another limitation for external validity is that the negative effect of two years early retirement cannot directly be projected to an earlier or later retirement take-up. In fact, my results differ severely between one year and two years of early retirement—and retiring later than the mandatory retirement age might not necessarily results in a decrease in mortality. Also, I only study the effect of men whose behavioral response to retirement are very likely to differ from the responses of women.

The results of this study have important policy implications. Although flexible retirement might increase welfare because it allows retiring according to one's own preferences, it can also decrease welfare by reducing life expectancy and increasing health care costs. The argument that early retirement leads to shortened life expectancy and could consequently ease financial pressure of public pension is not only ethically but also economically questionable, mainly because lower health can also lead to higher health care costs. Perhaps a flexible (and earlier) retirement age is in general beneficial and thus one should fight the negative health consequences thereof. For example with policies that combat the loss of structure due to retirement. Such policies could be to support initiatives that fight loneliness in retirement, such as collaborative forms of living in old age. Or policies that increase health literacy of retirees.

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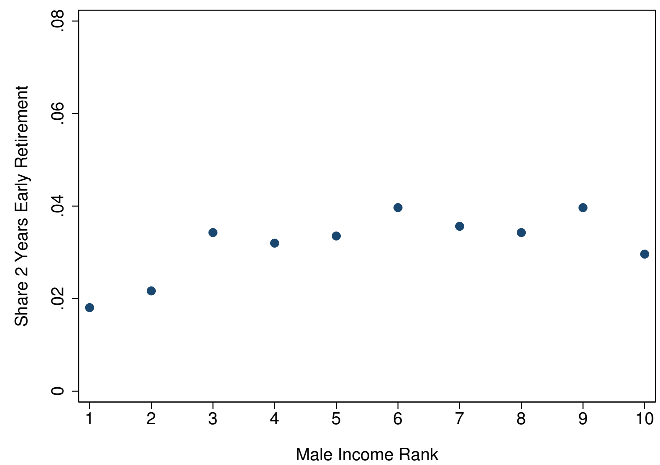
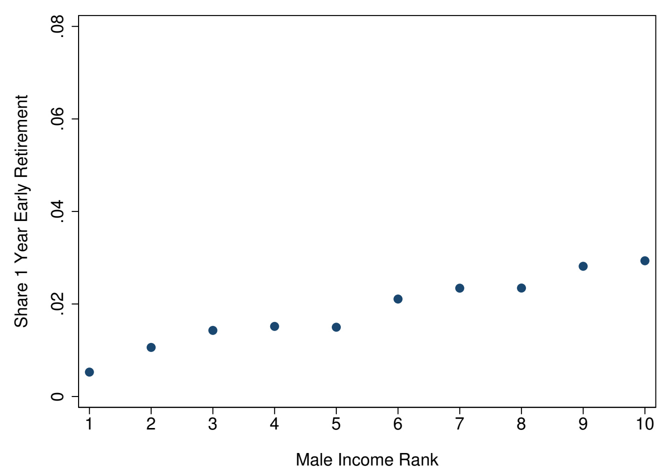
Appendix

Figure A1: Share Early Retirement by Income Rank



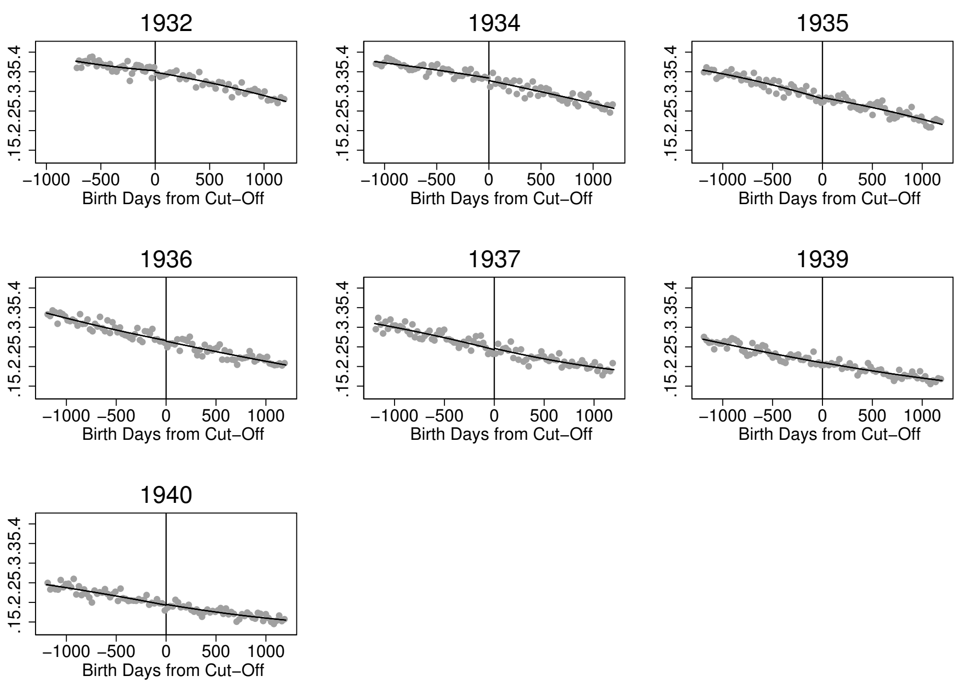
(a) 2 years earlier



(b) 1 year earlier

This figure shows the share of men taking early retirement. Panel (a) looks at early retirement of 2 years and includes cohorts 1938 to 1940. Panel (b) looks at early retirement of 1 year and includes cohorts 1933 to 1935.
Life time income tentiles of the 1938 have the following median life time income in 2017 Swiss Francs (almost equal to USD): $T_1 = 13,926; T_2 = 23,220; T_3 = 34,830; T_4 = 43,860; T_5 = 51,600, T_6 = 58,050; T_7 = 64,566; T_8 = 74,694; T_9 = 89,010; T_{10} = 126,420$

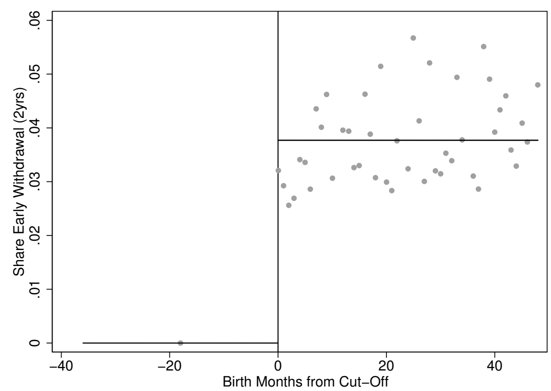
Figure A2: Placebo: Other Cutoff Years for Men



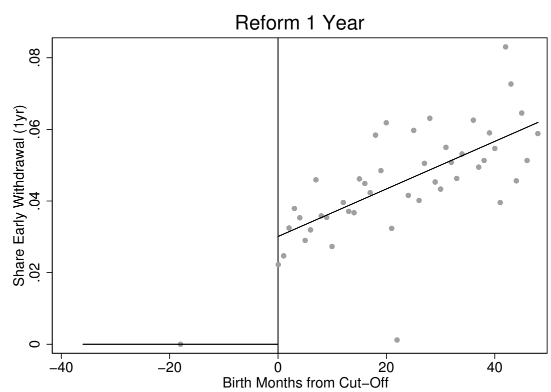
This figure shows reduced form RDD graphs around several end-of-year cutoffs that are not policy reform cutoffs.

Figure A3: First Stage: Share of Men Drawing Early Retirement Benefits

(a) 2 years earlier

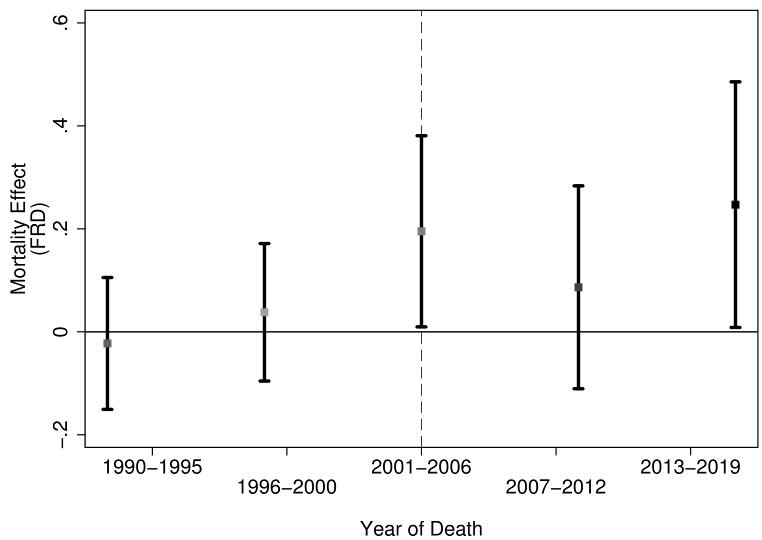


(b) 1 year earlier



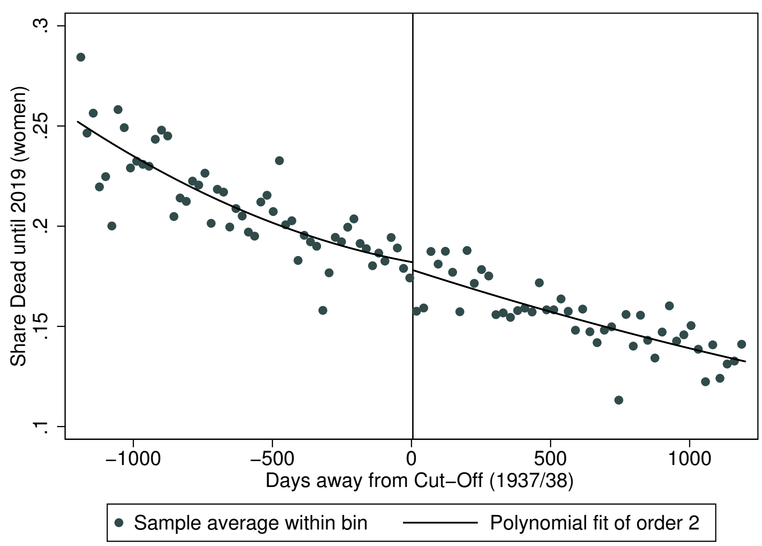
This figure shows the first stage estimates for the two policy reforms: retiring at age 63 (a) and retiring at age 64 (b).

Figure A4: Mortality Effect by Year of Death (Reform Early Retirement at 63)



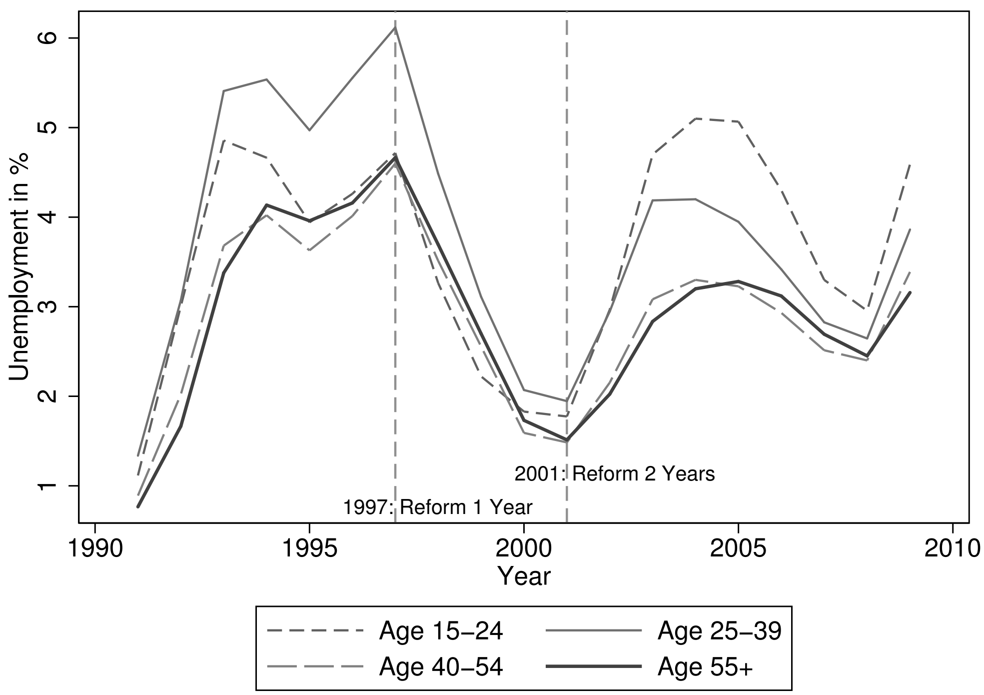
This figure shows the the coefficients for five fuzzy regression discontinuity (FRD) estimations using different outcome specific years of death. All FRDs are estimated by a triangular kernel, 2nd order polynomial, and automatic bandwidth selection. The dashed line represents year 2001, when early retirement at 63 was first possible.

Figure A5: Placebo: RDD at ER 63 Cutoff for Women



This graph shows the reduced form estimate at cutoff year 1937/38 for women only. Women are not targeted by the policy and should consequently not show a discontinuity.

Figure A6: Unemployment by Age Group



This figure shows the unemployment rate by age group and year. The age groups include men and women. Source: Federal Office of Statistics

Table A1: Testing for jumps at non-discontinuity points

	Cutoff	Estimate (Reduced Form)	p-Value
(1)	+2374	-.00062	0.907
(2)	-1463	0.01347	0.229
(3)	+3561	-0.00121	0.837
(4)	+1187	-0.01257	0.174
(5)	-732	0.01586	0.264
(6)	-2195	-0.01517	0.236

This table tests if there are no jumps at non-discontinuity points (Imbens and Lemieux, 2008). Row (1) assigns as a placebo cutoff the median above cutoff x_0 and only the sample above x_0 is used. Row (2) estimates the cutoff at the median of the sample below cutoff x_0. Row (3) to (6) split the two samples again in half and test for a discontinuity at the quartile points. All regression discontinuities estimate the reduced form and use quadratic polynomial and automatic bandwidth detection. A p-Value higher than 0.10 indicates that the hypothesis of no discontinuity at the cutoff cannot be rejected.

Table A2: First Stage Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Early Retirement at 63						
Above cutoff	0.040***	0.041***	0.042***	0.040***	0.042***	0.045***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left	309	802	600	600	2000	2000
BW right	309	802	600	600	2000	2000
F Test	.	.	.	.	.	.
Observations	7670	7670	7670	7670	7670	7670
Early Retirement at 64						
Above cutoff	0.022***	0.021***	0.023***	0.020***	0.029***	0.026***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Order Loc. Poly. (p)	1	2	1	2	1	2
BW Selection	mserd	mserd	Manual	Manual	Manual	Manual
BW left	408	413	600	600	2000	2000
BW right	408	413	600	600	2000	2000
F Test	.	.	.	.	.	.
Observations	7670	7670	7670	7670	7670	7670

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the first stage estimates for the two policy reforms: retiring at age 63 (first panel) and retiring at age 64 (second panel). The variable *above cutoff* shows the discontinutiy in drawing early public pension at the eligibility date of birth. Thus, the share of men opting for early retirement. All columns use a triangular kernel, but differ in the order of the polynomial, the bandwidth (BW) selection method, and the selected bandwidth.

Table A3: Number of Men opting for Early Retirement by Year of Birth

Birth Year	Two Years Early Retirement	One Year Early Retirement
	(1)	(2)
1930	0	0
1931	0	0
1932	0	0
1933	0	1,395
1934	0	1,950
1935	0	2,312
1936	0	2,690
1937	0	3,226
1938	1,830	2,247
1939	2,021	3,354
1940	2,179	3,462
1941	2,390	3,549
1942	3,624	9,461
1943	3,944	9,112
1944	4,094	8,859
1945	4,067	9,048
1946	4,617	9,151
1947	4,771	9,412
1948	4,298	6,502
1949	4,588	6,038
1950	4,725	5,605
1951	4,478	5,534
1952	4,772	5,281
1953	4,771	5,455
1954	5,034	4,862
1955	4,663	2,233

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This table shows the number of men opting for early retirement by year of birth. Column (1) shows men opting for two years of early retirement, Column (2) shows men opting for one year of early retirement.