Fatal Attraction? Extended Unemployment Benefits, Labor Force Exits, and Mortality*

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Abstract

We estimate the causal effect of a permanent and premature exit from the labor force on mortality. To overcome the problem of negative health selection into retirement, we exploit a policy change in Austria's unemployment insurance system that allows older eligible workers to exit the labor force 3 years earlier relative to comparable non-eligible workers. Using administrative data with precise information on mortality and retirement, we find that the policy change induces eligible men (women) to retire 5.5 (8.5) months earlier. Instrumental variable estimates show that for men retiring one year earlier causes a 5.5% increase in the risk of premature death and a 2.2 months reduction in the age at death but has no significant effect for women.

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1 Introduction

Demographic changes put governments under increasing pressure to implement major reforms to old age social security systems. A focus of many reforms is to increase the effective retirement age by restricting access to early retirement schemes. Workers and their political representatives often oppose such reforms. Among the most important arguments is that, after having worked in physically demanding jobs all their lives, workers should have the option to retire early and thus avoid emerging health problems. While leaving an unhealthy work environment is, ceteris paribus, conducive to good health, the health effects of retirement may go in the opposite direction. Indeed, the empirical evidence suggests that retirement is associated with less income to invest in one's health as well as a decrease in cognitive and physical activity (Bonsang *et al.*, 2012; Mazzonna and Peracchi, 2012; Rohwedder and Willis, 2010) and an increase in unhealthy lifestyles (e.g. Balia and Jones, 2008; Henkens *et al.*, 2008).

This paper presents new evidence on the causal effect of retirement on mortality, a key measure of health, by exploiting a policy change in Austria. We measure retirement as the number of years an individual exits the labor market before the statutory retirement age, which we also call "years in early retirement" or just "early retirement", since most people in Austria retire before the statutory retirement age.[1] This definition implies that an individual does not have to be retired in the legal sense of drawing a pension. How retirement affects health is theoretically ambiguous and ultimately an empirical question. The health capital model by Grossman (1972) posits that good health is both a consumption good, because it increases utility, and an investment good, because it increases productivity and earnings. Upon retirement, the incentive to invest in good health to raise productivity and earnings vanishes. Still, good health may become more valuable, because retirees have more time to enjoy it. Retirees also face lower opportunity costs for investing in health, such as exercising or cooking healthy food.

We take advantage of an unanticipated change to the unemployment insurance (UI) system, which temporarily extended the duration of UI benefits for workers living in certain regions of Austria. Because of this change older workers in eligible regions could retire three years earlier

[1]Measuring people's retirement age relative to the statutory retirement age is simply a normalization. Except for the fact that women have a lower statutory retirement age than men, the only reason why early retirement varies across people is because they retire at different ages. Studies also show that the statutory retirement age serves as an important anchor when to retire (Behaghel and Blau, 2012; Lalive *et al.*, 2019; Seibold, 2019).

than comparable workers in non-eligible regions. Since the policy change generates variation in the timing of retirement that is plausibly exogenous to individuals' health status, we can estimate the causal impact of early retirement on mortality using an instrumental variable (IV) estimation strategy.[2]

Our study has three main features. First, the policy we exploit generates an unusually large, unanticipated shift in the time spent in retirement. We find that men in eligible regions retire 5.5 months earlier from the labor force than men in non-eligible regions. The effect is even larger for eligible women who retire 8.5 months earlier than non-eligible women. Second, the policy change took place in the late 80s, allowing us to follow individuals for several decades after the change. Third, we use comprehensive social security data for the universe of private-sector workers in Austria. These data are collected to assess individuals' eligibility to (and level of) social security benefits. Information on an individual's work history and date of death is precisely measured and our estimates are unlikely to be contaminated by measurement error.[3]

Our empirical analysis can be summarized by five main findings. First, for men retirement has a strong negative effect on health. An additional year in early retirement increases men's risk of dying before age 73 by 1.47 percentage points (equivalent to a relative increase of 5.5%) and reduces men's age at death by 2.2 months. On the other hand, for women we find that early retirement is not associated with worse health outcomes. Our finding that early retirement increases pre-mature death for men but not women is robust to placebo and other specification checks. Second, the IV estimates are considerably smaller than the corresponding ordinary least squares (OLS) estimates, which is consistent with selection into early retirement based on poor health. Third, we exploit the institutional setting in Austria to distinguish between men who could retire early via the disability insurance (DI) program and those who could retire

[2]Previous evaluations of this policy change have found a large increase in unemployment durations and early retirement among the unemployed (Inderbitzin *et al.*, 2016; Lalive, 2008; Lalive and Zweimüller, 2004a,b; Winter-Ebmer, 1998), an increase in layoffs and UI inflow (Jäger *et al.*, 2019; Winter-Ebmer, 2003), and spillover effects to non-eligible unemployed (Lalive *et al.*, 2015). Our study adds to these papers by examining the effect of this policy on early retirement behavior for the full population, instead of the unemployed only, and by studying the link between early retirement and mortality.

[3]Measurement error is non-negligible when using self-reported health measures, rather than government-reported health indicators such as mortality (Baker *et al.*, 2004). Self-reported health measures of health may also suffer from ex-post justification bias, specially if they are subjective (e.g. Bertrand and Mullainathan, 2001). Indeed, studies using subjective self-reported health measures tend to find beneficial effects of retirement (Charles, 2004; Coe and Zamarro, 2011; De Grip *et al.*, 2012; Eibich, 2015; Insler, 2014; Johnston and Lee, 2009; Neuman, 2008) and only a few find negative effects (Behncke, 2012; Dave *et al.*, 2008). Moreover, the retirement age may also be measured with error in survey data.

early via the old age (OA) insurance program. We find similar effects for both groups of men, suggesting that what matters for mortality is not the retirement pathway per se but the fact that someone retires earlier. Fourth, we document higher mortality effects for men in blue-collar occupations, men with low-work experience, and men in bad health relative to men in white-collar occupations, men with high-work experience, and men in good health. Fifth, we explore different mechanisms driving our result and find that the male mortality estimates are similar for involuntary and voluntary retirements. We also show that early retirement leads to a significant reduction in lifetime earnings, but about 65% this earnings loss is compensated with government transfers. Hence, the change in lifetime income is small and cannot explain the increased mortality among men. However, contemporaneous income shocks could matter for mortality, specially if individuals are liquidity constrained or follow a rule-of-thumb.

Recent studies on retirement and health that use mortality as the main outcome and try to establish causality have found mixed results.[4] Hernaes *et al.* (2013) estimate the effect of early retirement on mortality using Norwegian register data. They focus on institutional changes in the access to early retirement to instrument for the actual retirement age and find no causal effect of early retirement on the incidence to die before age 77. Similarly, Hagen (2018) studies the health consequences of a two-year increase in the normal retirement age of local government workers in Sweden and finds that the reform had no impact on mortality. Bloemen *et al.* (2017) focus on targeted early retirement programs for Dutch civil servants and find that early retirement reduces the risk of dying within five years after retirement. Hallberg *et al.* (2015) also use targeted early retirement programs for Swedish army officers and find that early retirement had a beneficial effect on retirees' health. We contribute to this literature by studying a policy change that generated an extremely large early retirement response. For example, the change in early retirement rules studied by Hernaes *et al.* (2013) reduced the effective retirement age by 2 to 3.5 months, which is less than half of the effect we find. Having a large early retirement response helps us to identify the impact of early retirement on mortality, especially if this effect is small.[5]

[4]A related literature examines the impact of retirement on other objective measures of health, such as health care utilization (Frimmel and Pruckner, 2018; Lucifora and Vigani, 2018), handgrip strength (Bertoni *et al.*, 2018), or the risk of cardiovascular disease and cancer (Behncke, 2012). Another related literature looks at the impact of job loss on mortality (Browning and Heinesen, 2012; Eliason and Storrie, 2009; Sullivan and von Wachter, 2009). They find that mortality rates increase substantially after displacement.

[5]Indeed, for men the IV estimates of early retirement on mortality in Hernaes *et al.* (2013) are positive across

It is important to note that our estimates as well as the estimates in previous studies apply only to those who change their retirement behavior as a result of the policy change. Given the differences in health and socio-economic characteristics of the affected populations across the existing studies and given that there may exist countervailing effects from early retirement on health, it is perhaps not surprising that the empirical evidence is mixed. In our case, individuals who retire early tend to have worked in low-skilled jobs, to be in poor health, and to have low-work experience. By contrast, the affected individuals in Bloemen *et al.* (2017) and Hallberg *et al.* (2015) tend to have higher earnings and higher education than the average individual in the rest of the population.

A recent U.S. study by Fitzpatrick and Moore (2018) examines populations similar to ours and finds a clear, negative impact of retirement on health in the short run. Specifically, they document a discontinuous increase in mortality of 2% in the month men turn 62, which is the earliest eligibility age for social security benefits in the U.S. About 31 percent of men start claiming benefits in the first month of eligibility. They also show that claiming is connected to a large and immediate increase in retirement from the labor force, suggesting that the likely driver of the increase in male mortality is retirement from work.[6] This mechanism is also important in our context, since the extension in UI benefits led to a significant increase in labor force exits. However, our study differs in two main aspects. By following individuals over a long time, we are able to capture the long run effect of retirement on mortality. Moreover, males in our sample retire almost five years earlier. Despite this difference in the retirement age, we reach qualitatively similar conclusions on the health effects of retirement.

The remainder of this paper is structured as follows. In section 2, we discuss the institutional background for Austria. Section 3 discusses the data as well as the selection of our sample and presents some descriptive statistics. Section 4 present our econometric framework. Section 5 presents the results. Section 6 concludes.

all specifications and in some case quite large, but imprecisely estimated.

[6]Another related study is Black *et al.* (2017). They estimate the effect of DI benefit receipt on U.S. mortality and find that mortality increases within the first 10 years of benefit receipt. Because DI causes lower labor supply, they argue that this result suggests that withdrawing from work increases mortality. A caveat is that their data capture deaths of beneficiaries more accurately than deaths of non-beneficiaries, because the Social Security Administration has a greater financial incentive to monitor beneficiaries' date of death. This non-classical measurement error will bias estimates towards finding that DI benefit receipt increases mortality, but they argue that this problem is small.

2 Institutional Background

2.1 Retirement Pathways in Austria

Since our study focuses on workers retiring during the late 1980s and early 1990s, we start with a description of the different retirement pathways in the Austrian pension and unemployment insurance (UI) system during that time.[7] We define retirement as the date at which an individual withdraws permanently from the labor market (see section 3 below for details).

The Austrian public pension system covers almost all workers and provides OA and DI pensions, which are the main source of income in retirement. The formula for computing the benefit level is the same for both pensions. It consists of a pension coefficient, which increases with the number of insurance years up to a maximum of 80% (about 45 insurance years), multiplied by an assessment basis, which corresponds to the average indexed capped earnings over the best 15 years.[8] All pensions are subject to income taxation and mandatory health insurance contributions. The replacement rate after income and payroll taxes is on average 70% of pre-retirement net earnings.

A first pathway into retirement is the direct transition from employment by claiming an OA pension. The statutory retirement age is 65 for men and 60 for women, but workers with enough insurance years may claim an OA pension at any age after 60 for men and 55 for women.[9] Apart from direct transitions from employment, the most important pathway into retirement is the indirect transition via the UI system. Regular UI benefits replace 55% of the prior net wage, subject to a minimum and maximum. On top of regular UI benefits, family allowances are paid. Workers above age 50 can claim UI benefits for up to 52 weeks (30 weeks before August 1989). Benefits are available immediately to individuals who are laid off by their employers, while those who quit or are fired for cause have a 28-day waiting period. Individuals who exhaust the regular UI benefits can apply for UI assistance. These means-tested transfers last for an indefinite period

[7] There were several changes to the pension system starting in the mid-1990s. However, these changes affected both the treatment and the control group in the same way. See Staubli and Zweimüller (2013) and Mullen and Staubli (2016) for details.

[8] Insurance years comprise both contributing years (periods of employment, including sickness, and maternity leave) and qualifying years (periods of unemployment, military service, or secondary education).

[9] These statutory ages were increased by pension reforms during the 2000s. Under the rules that were in place during the study period, a worker must have had accumulated at least 35 insurance years to claim a pension before the statutory retirement age; the insurance years requirement was only 25 years for workers who had drawn UI benefits for the past year.

and are about 70 percent of regular UI benefits. Additionally, unemployed men (women) age 59 (54) or older can claim "special income support" for one year, if they have contributed to the UI program for at least 15 out of the previous 25 years.[10] Thus, the UI system allows older men (women) to exit the work force at age 58 (53) and bridge the gap to an old-age pension via regular UI benefits and special income support.

A third retirement pathway is via the DI program, which grants relaxed access to a DI pension at age 55. Applicants below age 55 are awarded a DI pension if a physical or mental health impairment reduces the earnings capacity to less than half relative to that of a healthy person with comparable education in any "reasonable" occupation the individual could be expected to hold. At age 55 the comparison changes from a healthy worker performing any type of work in the economy to a healthy worker in a similar occupation.[11] As a direct consequence, disability enrollment rises significantly beginning at the age threshold.

2.2 The Regional Extended Benefit Program

The Regional Extended Benefit Program (REBP) was introduced in response to a steel crisis in the late 1980s, which caused layoffs and downsizing of production plants, particularly in regions where the steel industry was important. To protect older workers against adverse labor market conditions in these regions, the Austrian government extended the potential UI benefit duration from 52 to 209 weeks for workers who satisfied each of the following criteria: aged 50 and above, a continuous work history (15 employment years in the last 25 years), and at least 6 months of residence in one of the 28 eligible regions.

The program was introduced in June 1988 and applied to all new unemployment spells starting in June 1988 or later as well as to spells in progress in June 1988. In January 1992, a reform became effective which abolished the benefit extension for new UI claims in 6 of the originally 28 regions. Moreover, eligibility criteria were tightened, as not only location of residence but also an individual's workplace had to be in a REBP region. In the remaining 22 regions, the REBP was terminated in August 1993. The termination left all UI claims in progress unaffected; only new claims were no longer eligible for the benefit extension.

[10] Special income support is equivalent to a regular UI spell in legal terms but grants 25% higher benefits.

[11] The age at which disability screening is relaxed was raised to 57 in 1996 for men and in 2000 for women (see Staubli, 2011).

Figure 1

Figure 1 shows a map of treatment regions (TRs) and regions that were never treated (CRs). TR1s were treated until 1991 and TR2s were treated until 1993. The regions eligible for the program were selected by the minister for social affairs, a member of the ruling social democratic party. Lalive and Zweimüller (2004a) show that both employment and unemployment rates for (potentially) eligible workers were similar before the start of the program, but eligible regions had a higher share of employment in the steel sector (17% compared to 5% in non-REBP regions). In our analysis, we exclude individuals who have worked in the steel sector.

Figure 2 shows that the introduction of the REBP significantly increased the incentive to exit the labor force via the UI system. The numbers in parenthesis are the net replacement rates (net benefits relative to net earnings) of the different transfer programs for an individual with average earnings. Thanks to the REBP, eligible men (women) could already retire at age 55 (50) by claiming regular UI benefits for 4 years and special income support for 1 year, followed by claiming an OA pension at age 60 (55). In contrast, male and female workers not eligible for the REBP could only retire at ages 58 and 53, respectively.

Figure 2 also shows that the REBP increased the incentive to retire for men below age 55. Without the REBP, men below age 55 could retire at age 54 by claiming UI benefits for one year followed by claiming a DI pension at age 55. With the introduction of the REBP, this option was already available at age 51.

Figure 2

3 Data and Sample

Data. We use administrative data from the Austrian Social Security Database (ASSD), described in more detail in Zweimüller *et al.* (2009). The ASSD covers the universe of Austrian workers and contains detailed information on the labor market and earnings histories of individuals between 1972 and 2017. Information on insurance relevant states prior to 1972 is available for individuals who have claimed a public pension by the end of 2008. The data also contain a limited set of socio-economic characteristics (year and month of birth, sex, general occupation) and a unique firm identifier (from 1972 onward) that allows us to link several firm-level characteristics (geographical location, industry affiliation, and size).

A key feature of the ASSD is that it contains precise information on the date of retirement (i.e. labor market exit) and the date of death up to 2017, as well as all information necessary to determine an individual's eligibility to the REBP. More specifically, the month of birth and employment history allow us to determine whether a worker meets the age and employment criteria set by the REBP. We do not observe the place of residence for all individuals and proxy community of residence by the community of work. This introduces some measurement error due to the false classification of REBP-eligible workers as non-eligible and vice versa. We find that this issue is not a major drawback, as most individuals in our sample work in the same labor market region where they live.[12]

Sample selection. Starting from the population data set, we impose three restrictions. First, we focus on men born in January 1927 to December 1944 and women born in January 1932 to December 1944. Men (women) born before June 1930 (June 1935) as well as men and women born after July 1943 did not benefit from the REBP. They were either older than age 58 (age 53 for women) when the REBP was introduced and could retire early without the REBP, or they were younger than age 50 when the REBP was abolished and thus not eligible. On the other hand, men born in June 1930 to July 1943 and women born in June 1935 to July 1943 could benefit from the REBP, because they eventually turned age 50 during the REBP and were younger than age 58 (men) or age 53 (women) when the REBP was introduced.

Second, we exclude workers from the steel sector because the REBP did not induce changes in the retirement age for them. Apart from the REBP, there was a nation-wide program to alleviate problems associated with mass redundancies in the steel sector, the "steel foundation". The steel foundation guaranteed regular UI benefits of three (later four) years to displaced workers in the steel sector, regardless of place of residence. We therefore do not find any difference in the retirement age between steel-workers in TRs and CRs. Third, we focus on workers who meet the experience criterion of the REBP (i.e. at least 15 employment years in the last 25 years) but

[12]We can check the extent of measurement error introduced by this proxy because we can observe the place of residence for individuals on unemployment benefits. We correctly assess REBP-eligibility for more than 90% of all individuals in this subsample if place of work instead of place of residence is used to assess REBP eligibility.

use workers who do not satisfy this criterion for placebo tests. We also drop individuals who permanently leave the labor force or die before age 50.

Key variables. The key variables of our analysis are measures of retirement and mortality. Because information on labor-market histories and mortality is available until December 2017, cohorts in our sample can be tracked at least up to age 73. We define two outcome variables related to mortality. The first is a dummy indicating whether an individual died before reaching age 73. This indicator is meaningful in the present context, because we are considering older cohorts whose life expectancy is quite low. In our sample, the probability of death before age 73 is 27.5 percent for men and 14.0 percent for women. The second measure is the age at death. This measure is interesting because it comprises both the effect of retirement on the probability of premature death (extensive margin) and the length of a life (intensive margin). We censor age at death at age 73 for those individuals who are still alive at age 73.

Theoretically, retirement could affect mortality both in the short run, for example through sudden shocks triggered by changes in work activity or income, or in the long run, for example through retirement-related changes in health investments (e.g., Grossman, 1972; Yogo, 2016). To capture short- and long-run effects, we measure retirement as the time span between the statutory retirement age and the effective retirement age (the age at the end of the last employment spell). [13] We call this variable "years in early retirement" or just "early retirement", because the retirement age is measured relative to the statutory retirement age, i.e. early retirement = 65/60 - retirement age. A positive (negative) number implies that an individual has retired before (after) the statutory retirement age. We choose the statutory retirement age as a reference point, because most people in Austria retire before this age. We could choose an age other than the statutory retirement age, the differences in the number of years spent in retirement across men (women) would be the same.

Descriptive statistics. In our analysis, we stratify the sample by gender because retirement rules and mortality patterns differ for men and women. Our final sample consists of 358,694

[13] An individual does not have to be retired in the legal sense of drawing an OA pension. Instead, effective retirement hinges upon the last day of employment and does not refer to a particular transfer an individual gets after ceasing work. Retired individuals in our sample can draw unemployment benefits, disability benefits, old-age benefits, some other type of benefit, or no transfer.

men and 162,979 women. The female sample is smaller than the male sample because the female sample consists of fewer birth cohorts and because female labor force participation among the birth cohorts in our sample was low. Consequently, women are less likely to fulfill the work history requirement for REBP eligibility.[14] Table 1 present summary statistics by gender and region before/after the REBP (men born in January 1927–May 1930 or August 1943–December 1944 and women born in January 1932–May 1935 or August 1943–December 1944) and during the REBP (men born in June 1930–July 1943 and women born in June 1935–July 1943).

Panel A displays summary statistics for the outcome variables of interest. TRs exhibit a drop in the retirement age and an increase in the years in early retirement during the REBP relative to before/after the REBP. This pattern is consistent with the REBP inducing individuals to retire early, although CRs also show a decline in the retirement age and an increase in the years in early retirement. The probability to die before age 73 generally declines during the REBP relative to before/after the REBP, and this decline was somewhat smaller for men in TRs compared to CRs. The age at death in TRs, relative to CRs, exhibits a slightly larger drop for men and a smaller increase for women.

Panel B displays summary statistics for the background characteristics. These characteristics are measured at age 49 or before when individuals are not yet eligible for the REBP. Individuals in TRs tend to earn lower wages and work more in blue-collar occupations and manufacturing, and less in wholesale trade, but overall the differences in background characteristics across regions are small.

Table 1

4 Empirical Strategy

Our primary aim is to estimate the causal effect of early retirement on mortality using regressions of the following form:

$$y_{icr} = \beta_0 + \beta_1 ER_{icr} + \mathbf{X'_{icr}}\boldsymbol{\beta_2} + \lambda_\mathbf{c} + \eta_\mathbf{r} + \epsilon_{icr}, \tag{1}$$

[14] The male sample shrinks to 273,405 when we apply the same birth cohort restriction used for the female sample. The remaining difference is explained by lower female labor force participation.

where i denotes individual, c denotes the year-month of birth, r denotes the region of residence, and y_{icr} is a mortality outcome (such as an indicator for death before age 73 or the age at death). ER_{icr} denotes the years spent in early retirement, i.e. the difference between the statutory retirement age and the age of permanent exit from work, $\mathbf{X_{icr}}$ is a set of individual controls, λ_c and η_r are full sets of birth year-month and region fixed effects, respectively, and ϵ_{icr} is an error term.[15]

In observational data, inference on the parameter of interest β_1 is hampered if unobserved characteristics, such as health shocks, affect both mortality and early retirement. If $\beta_1 > 0$ and if unobserved health shocks are positively correlated with early retirement, OLS overestimates the magnitude of the coefficient on early retirement.[16] To deal with unobserved health shocks, we instrument the years spent in early retirement by workers' eligibility for the REBP.

Specifically, we estimate equation (1) by two-stage least squares (2SLS), using the following first-stage equation:

$$ER_{icr} = \gamma_0 + \gamma_1 \text{Eligible}_{icr} + \mathbf{X'_{icr}}\boldsymbol{\gamma_2} + \boldsymbol{\theta_c} + \boldsymbol{\omega_r} + \mu_{icr}, \tag{2}$$

where Eligible_{icr} is an indicator measuring whether individual i born in year-month c and living in region r could take advantage of the REBP or not.[17] The corresponding reduced-form effect of REBP on mortality is estimated by the following equation:

$$y_{icr} = \delta_0 + \delta_1 \text{Eligible}_{icr} + \mathbf{X'_{icr}}\boldsymbol{\delta_2} + \boldsymbol{\pi_c} + \boldsymbol{\zeta_r} + \nu_{icr}, \tag{3}$$

in which y_{icr} is a mortality outcome again.[18]

The coefficient β_1 on ER_{icr} from 2SLS estimation of equation (1) captures the local average treatment effect of early retirement on mortality among the individuals who retire early with

[15]Individual controls include dummies for industry at age 49, blue collar status at age 49, daily wage at age 49 as well as number of years on sick leave, unemployed, and employed between ages 44-48.

[16]There is ample evidence documenting a negative health selection into retirement (e.g. Disney *et al.*, 2006; Dwyer and Mitchell, 1999).

[17]More specifically, Eligible_{icr} denotes the years of additional UI benefits an individual could draw with the REBP before reaching age 58 (53), divided by the maximum years of additional UI benefits offered by the REBP (3 years). For example, men in TRs born in June 1932 were age 56 when the REBP was introduced and we therefore set Eligible_{icr} equal to 2/3.

[18]Inderbitzin *et al.* (2016); Lalive and Zweimüller (2004a,b); Lalive *et al.* (2015); Winter-Ebmer (1998) use similar research designs, but they define the treatment based on region and age, i.e. being older than age 50, instead of region and birth cohort.

the REBP and who would not retire early without the REBP (Imbens and Angrist, 1994). Because we have one endogenous variable and one instrument, the local average treatment effect estimate of β_1 is given by the ratio of the reduced-form coefficient (equation 3) and the first-stage coefficient (equation 2), or $\frac{\delta_1}{\gamma_1}$. Since the main source of the exogenous variation in the retirement age is at the cohort-region level, all standard errors we report are adjusted for clustering at this level.

The key assumption for REBP eligibility to be a valid instrument for early retirement is that individuals' assignment of REBP eligibility is independent of unobserved characteristics that are correlated with mortality and early retirement. Since we can control for region-specific and cohort-specific level differences, this assumption boils down to whether there are unobserved region-specific shocks. We test the validity of the identifying assumption in three ways. First, we examine early retirement and mortality trends for cohorts that were too old or too young to benefit from the REBP. If the identifying assumption holds, then we should not find any significant differences in early retirement or mortality trends between TRs and CRs for those cohorts. Second, as a robustness check we define REBP eligibility based on workers' location of residence at age 40 when no one knew about the REBP. The estimates for this robustness check should be similar to the main estimates, unless workers did move from CRs to TRs during the REBP to gain eligibility. Such strategic moving behavior would violate the identifying assumption. Third, we examine early retirement and mortality trends for individuals who have less than 15 employment years in the past 25 years and are therefore not eligible for the REBP. For these individuals we should not find any significant differences in early retirement and mortality trends between TRs and CRs if our identifying assumption is valid.

We need three additional assumptions for the IV estimates to measure the causal impact of early retirement on mortality. First, there is a first-stage relationship between REBP eligibility and the early retirement date (i.e. $\gamma_1 > 0$ in equation 2). This assumption is uncontroversial, as the REBP has a strong impact on early retirement behavior. Second, the exclusion restriction indicates that REBP eligibility affects mortality only through its impact on the duration of early retirement, and not directly in any other way. It is likely that this assumption holds in the present context, because it is difficult to imagine that the mere eligibility for extended UI benefits should have a direct effect on mortality.[19] Third, the monotonicity assumption requires that REBP eligibility would not result in individuals retiring later. This assumption is not uncontroversial because the REBP could have had general equilibrium effects. Indeed, Lalive *et al.* (2015) find that non-eligible job losers in REBP regions had higher job finding rates and lower unemployment durations. Two testable implication of the monotonicity assumption are that the first stage estimates should be non-negative for non-eligible workers and for subsamples of eligible workers. Indeed, we find that first stage estimates are insignificant for non-eligible workers and are consistently positive and sizeable for different subgroups of individuals.

5 Results

5.1 The First Stage: The REBP and Early Retirement

Main results. Table 2 presents first-stage estimates of equation (2) for men and women who have at least 15 employment years in the past 25 years. For men, we also report estimates for two subsamples, which include the same before- and after-REBP birth cohorts as the full sample but differ in the during-REBP birth cohorts. The during-REBP cohorts in "access to DI pathway" are men who were younger than age 55 when the REBP was in effect. Given the institutional setting in Austria (see Figure 2), these men would use the REBP to retire early via the DI pathway. In contrast, the during-REBP cohorts in "access to OA pathway" were age 55 or older at some point during the REBP and also had the option to retire early via the OA pathway.

Contrasting these two subgroups is interesting, because individuals retiring via the DI pathway differ from those retiring via the OA pathway in their socioeconomic and health characteristics.[20] For example, those retiring via the DI pathway are more likely to be blue-collar workers and having worked in the construction or manufacturing sector. They tend to have lower earnings and are more likely to have been unemployed and on sick leave in the past. Moreover,

[19]REBP eligibility could affect mortality other than through the duration of early retirement if there are peer effects in mortality, e.g. from one spouse to another. Indeed, a common finding in the literature is that health is reduced following spousal bereavement (e.g. van den Berg *et al.*, 2011). Unfortunately, we are not able study health effect of spousal bereavement, because we cannot link spouses in our data.

[20]Table A.1 in the Appendix shows separate summary statistics for REBP-eligible individuals retiring via the DI and OA pathway, respectively.

individuals retiring via the DI pathway exit the labor force around four years earlier than those retiring via the OA pathway. Given these differences, it seems plausible that the effect of early retirement on mortality would not necessarily be the same for both subgroups.

Column 1 of Panel A shows that for men being eligible for the REBP increases the time spent in early retirement by 0.441 years, or almost 5.5 months. This represents a substantial 6.4 percent increase relative to the baseline in CRs during the REBP. The point estimates are similar when we split the male sample by the type of retirement pathway individuals had access to (columns 2 and 3). Indeed, we cannot reject the null hypothesis that the coefficient estimates are identical (p=0.7). Column 4 of Panel A shows that for women the estimate is significantly larger. Being eligible for the REBP increases the time spent in early retirement by 0.722 years, or almost 9 months. As Panel B shows, the point estimates barely change when we include additional control variables that capture observed characteristics of individuals.

The statistical significance of the early retirement effects is also reflected in the relevant F-statistic, reported at the bottom of each panel. It amounts to 132 or higher for all the estimates reported, well above the threshold value of 10 above which IV is not supposed to be subject to a weak instruments critique (Staiger and Stock, 1997).

Table 2

Different pathways. We further explore the impact of the REBP on early retirement by estimating equation (2) separately for each age in the interval 49-63 for men and 49-60 for women. We define three dependent variables to highlight the pathways through which individuals exit the labor force. The first is simply an indicator for whether an individual retires at a given age. The second is an indicator for whether an individual retires at a given age by eventually claiming a DI pension (retirement via DI pathway). The third is an indicator for whether an individual retires at a given age by eventually claiming an OA pension, without having claimed a DI pensions before (retirement via OA pathway).

Panel (a) of Figure 3 shows that the probability to retire does not change for men younger than age 50 but is positive and statistically significant for men between ages 50 and 55, consistent with the retirement incentives created by the REBP. On the other hand, coefficient estimates turn negative and significant between ages 56 and 61. This suggests that the excess retirements between ages 50 and 55 are driven by men who, in the absence of the REBP, would have retired between ages 56 and 61. Panel (b) shows that many men retiring between ages 50 and 55 do so via the DI pathway. In fact, most claim the DI pension at age 55 when DI eligibility rules are relaxed. Retirement via the DI pathway is lower between ages 56 and 58, because some men who, without the REBP, would have retired via the DI pathway at these ages use the REBP to retire even earlier via the DI pathway or the OA pathway. Consistent with this idea, we find a significant spike in the fraction of men who retire at age 55 via the OA pathway (Panel c).

Turning to the results for women, Panel (d) shows that the difference in the probability to retire between TRs and CRs during the REBP relative to before/after the REBP displays a large spike at age 50, which is when REBP-eligible women can first retire via the UI system. The probability to retire is also significantly higher at ages 51 and 52. In contrast, the incidence of retirement between ages 53 and 56 is significantly lower and becomes insignificant at ages 57 to 60. Panel (e) shows that estimates for retirement via the DI pathway are negative and statistically between ages 50 to 54, suggesting that some women who would have retired via the DI pathway use the REBP to retire via OA pathway instead. However, this substitution effect from the DI pathway to the OA pathway is quantitatively small. Instead, Panel (f) suggests that the REBP affected retirement behavior of women primarily through the possibility to exit the labor force at age 50 via the OA pathway.

Figure 3

Differential trends. We next examine whether the size of the early retirement response varies across birth cohorts by replacing Eligible$_{icr}$ in equation (2) with a set of birth year times TR_{icr} interaction terms (where TR_{icr} is an indicator taking the value one if an individual lives in a TR).[21] These interaction terms also offer the possibility to test whether early retirement trends are the same across regions before and after the REBP.

Figure 4 plots the estimated coefficients of the interaction terms with years spent in early retirement as dependent variable. Several things can be observed. First, the time spent in early retirement is significantly higher for REBP-eligible birth cohorts (those in between the two

[21] We lump men (women) born between January-May 1930 (1935) and between August-December 1943 with the birth cohort 1929 (1934) and 1944, respectively. Since the REBP was in effect June 1988-July 1993, men (women) born in January-May 1930 (1935) were too old to be eligible for the REBP, while individuals born in August-December 1943 were too young.

vertical lines) in TRs relative to CRs, particularly for women. Indeed, a Wald test rejects the null hypothesis that the coefficient estimates for REBP-eligible birth cohorts are jointly equal to zero with $p<0.01$. Second, the point estimates are generally smaller for older eligible cohorts and become larger for younger eligible cohorts. This pattern is expected: when the REBP was introduced, older cohorts were already close to the earliest age they could retire without the REBP and therefore only needed to rely on the extended UI benefits for a short duration. In contrast, younger cohorts could take full advantage of the REBP to retire early. Third, the estimated coefficients fluctuate around 0 for birth cohorts who are not eligible for the REBP because they were either too old when the REBP was introduced, or too young when the REBP was abolished, supporting the validity of the identifying assumption.

Figure 4

5.2 The Effect of Early Retirement on Mortality

Main results. In this section, we present our main estimates of the effect of early retirement on mortality. Table 3 reports the effect of early retirement on mortality estimated by OLS and IV (equation 1) as well as the reduced form effect of the REBP on mortality (equation 3). The OLS estimates in Panel A show that for men an additional year in early retirement is associated with a 2.43 percentage points increase in the probability to die before age 73. This corresponds to a relative increase in the risk of premature death of about 9.1% (=2.43/26.66). The OLS estimates are similar for the subgroups of men who had access to the DI pathway or the OA pathway, respectively. The corresponding OLS estimate for women is about three times smaller, but still sizeable when expressed in relative terms because women are less likely to die before age 73.

The reduced form estimates in Panel A indicate that men who are eligible for the REBP have a 0.67 percentage points higher probability to die before age 73. The effect is slightly larger (but also less precisely estimated) for men who have access to the DI pathway (0.74 percentage points) and slightly smaller (and statistically insignificant) for men who have access to the OA pathway (0.47 percentage points). In contrast, we do not find a statistically significant effect for women. We find that the IV estimates are consistently smaller than the OLS estimates, which is expected if unhealthy individuals self-select into early retirement. The IV estimates imply that an additional year in early retirement increases men's mortality incidence before age 73 by 1.47 percentage points (5.5%) but has no effect on women's mortality. We can reject the hypothesis that the IV estimates for men and women are identical at the 10% level. The IV estimate is larger for men with access to the DI pathway (1.65 percentage points) than for men with access to the OA pathway (1.02 percentage points), but a Wald test cannot reject the hypothesis that these estimates are identical (p=0.537).

Panel B of Table 3 reports analogous estimates of the effect of early retirement on the age at death. The estimates show a similar pattern as the estimates for the incidence of death. The OLS estimates are large and statistically significant in all cases. For men they range from -0.322 years to -0.353 years, while for women the estimate is about three times smaller in absolute terms (-0.121 years). The IV estimates imply that an additional year in early retirement is associated with a reduction in the men's age at death of 0.185 years, which is about half of the corresponding OLS estimate, and has no effect on women's age at death (although we fail to reject the null hypothesis that the estimates for men and women are identical). The estimates are similar for men with access to the DI pathway (-0.176 years) and the OA pathway (-0.196 years), which is interesting and suggests that what matters for mortality is not the choice of retirement pathway per se, but the fact that someone is out of the labor force for a long time.

Table 3

Differential trends. Figure 5 plots the estimated coefficients of the birth year times TR interaction terms for the probability to die before age 73 (Panel A) and the age at death (Panel B). Both measures are not significantly different between TRs and CRs for birth cohorts who were too old or too young to be eligible for the REBP (those left of the first and right of the second vertical line). This finding provides further support that our estimates do not simply capture pre-existing difference in mortality trends between TRs and CRs.

For REBP-eligible men (those in between the two vertical lines), we find that the point estimates are mostly positive for the probability of death before age 73 and negative for the age at death, particularly for the younger cohorts, but they are also quite imprecisely estimated. However, we can reject the hypothesis that the coefficient estimates on the interaction terms for REBP-eligible men are jointly zero at the 10% level. In contrast, for REBP-eligible women we fail to reject the hypothesis that the coefficient estimates on the interaction terms are jointly zero ($p \geq 0.342$ and $p \geq 0.717$, respectively).

Figure 5

Cumulative mortality. To better understand whether retirement affects mortality primarily in the short run (driven by sudden shocks) or in the long run (driven by changes in health investments and shocks accumulating over time), we separately estimate the impact of the REBP on men's probability to die before age 51, 52, 53, ..., 73 using equation (3). If short-run effects are important, then the probability to die should accelerate when men exit the labor force, primarily in their 50s. On the other hand, if long-run effects matter, then the probability to die should only accelerate when men are already in their 60s or 70s.

Panel A of Figure 4 plots the estimated δ_1-coefficients for the full sample of men.[22] We find that the probability to die starts to increase quite steeply between ages 55 and 58, shortly after most men in TRs have exited the labor force and remains flat thereafter. This pattern is consistent with retirement having an immediate effect on mortality. However, we also find that the difference in the probability to die between TRs and CRs starts to increase again after age 66, suggesting that retirement increases mortality also in the long run, although the long-run effect is less pronounced than the short-run effect. Interestingly, it appears that the choice of retirement pathway may be important in this context. The long-run effect is primarily driven by men with access to the DI pathway (Panel b), while the short-run effect is stronger among men with access to the OA pathway (Panel c). However, we also note that we lack the precision to reject the null hypothesis that the point estimates for the DI and OA pathway are identical.

Figure 6

Heterogeneity. There is no reason to expect that the effect of the REBP on early retirement and mortality is the same among subgroups of individuals. Table 4 presents separate estimates for groups of men defined by occupation (blue-collar vs white-collar), time spent on sick leave, and work experience.[23] The first column in each panel displays the results of our baseline sample

[22] Figure A.1 in the Appendix shows that the estimated δ_1-coefficients for women are insignificant at all ages.

[23] Table A.2 in the Appendix reports analogous estimates for women. We find that the IV estimate of the effect of early retirement on mortality is insignificant for all subgroups of women, despite the first stage being always large and highly significant.

for comparison. Panel A shows that the first-stage estimates of the effect of the REBP on the years spent in early retirement is always positive and statistically significant, but its magnitude varies across groups. For example, eligible men in blue-collar occupations spend almost three times as much additional time in early retirement than eligible men in white-collar occupations.

Panel B presents estimates of the effect of early retirement on the incidence of death before age 73. The IV estimates are consistently smaller than the OLS estimates, except for men who have spent time on sick leave before age 50, and there are striking differences among certain subgroups. We find that the point estimates are positive and statistically significant for blue-collar workers, men who have spent some time on sick leave, and men with low work experience, and statistically insignificant for white-collar workers, men who have not spent time on sick leave, and men with high work experience. However, only for men who have and have not spent time on sick leave we can reject the null hypothesis that the point estimates are identical (p=0.049).

As Panel C shows, the IV estimates for the age at death show a similar pattern as the estimates for the incidence of death: additional time in early retirement reduces the age at death for men in blue-collar jobs, men with prior sick leave history, and men with little work experience. Reassuringly, we find the same qualitative patterns in the reduced form estimates. The reduced form is estimated via OLS and therefore unbiased, while the IV could be biased if the first stage is not sufficiently strong. However, this concern is unfounded because the relevant F-statistic for all subgroups is at least 88 (not reported), well above the threshold value of 10 above which an instrument is not considered weak.

Table 4

Placebo and Robustness. Table 5 presents several robustness and placebo checks to examine the sensitivity of our estimates. Panel A reports estimates of a proportional hazard model where we assume that the baseline hazard follows a Gompertz distribution.[24] The hazard estimates confirm our prior findings. Specifically, we find that being eligible for the REBP is associated with a 3% increase in the mortality hazard among men and has no effect on the mortality hazard among women. The estimate appears to be somewhat larger for men with access to the

[24]In a Gompertz model, the logarithm of the mortality is linear in age and, as shown by Chetty *et al.* (2016), this log-linear approximation fits age-specific mortality rates very well.

DI pathway than for men with access to the OA pathway, but the difference is not statistically significant.

We can use the hazard estimates to calculate the effect of early retirement on mortality beyond age 73. Specifically, assuming that the difference in the hazard ratio stays constant at 3% until age 100, we calculate the effect of the REBP on the male survivor function after age 73, which gives us a predicted change in the age of death between ages 73 and 100 of -0.162 years. We then re-scale this estimate by the first stage (0.458) and add it to the age at death estimate reported in Panel B of Table 3. This way we find that an additional year in early retirement reduces the age at death among men by 0.539 years (=0.185+0.162/0.458). Applying the same approach, we find that an additional year in early retirement reduces the age at death by 0.565 years for men with access to the DI-pathway and by 0.453 years for men with access to the OA-pathway. Note that these estimates are likely upper bounds of the true effects. As Figure 6 shows, most of the excess mortality driving the regional difference in the hazard ratio occurs before age 60, suggesting that at higher ages the difference in the hazard ratio is less than 3%.

One potentially important confounding factor of our analysis is unobserved regional differences in access to health care.[25] To check the robustness of our results, we compare workers living within 77 kilometer driving distance to the border between TRs and CRs that have arguably more similar health care access.[26] It turns out that the point estimates are similar, if at all they are larger and more precisely estimated. Panel C presents estimates when we define REBP eligibility based on workers' location of residence at age 40. This robustness test illuminates whether our main estimates may be biased due to workers moving from CRs to TRs to gain REBP eligibility. Such moves would upward bias our main estimates if the movers are more likely to retire and die early. However, the point estimates for this robustness test are very similar to the main estimates, suggesting that such selective moving behavior is not widespread.

Panel D reports estimates from a placebo test in which we estimate the same set of regressions for the sample of individuals with less than 15 employment years in the past. These

[25]As in most industrialized countries, access to health care in Austria varies significantly between urban and rural areas (OECD, 2013). For example, the number of physicians per 1,000 inhabitants varies from 3.6 in Vorarlberg to 6.6 in Vienna. Similarly, the number of hospital beds per 10,000 inhabitants varies between 34 in Tyrolian Oberland to 155 in Graz.

[26]We choose 77 kilometers because the maximum driving distance from a treated region community to the nearest control region community is 76.5 kilometers, hence this restriction guarantees that we include all treated regions.

individuals are not eligible for the REBP and we should therefore not find any significant effects of the REBP on early retirement and mortality. Indeed, all point estimates are statistically insignificant and much smaller in magnitude than for the main sample, although for the mortality estimates we lack the statistical power to reject the null hypothesis that the placebo and main estimates are identical. These findings further support the idea that the REBP, and not some underlying region-specific shock, drives the observed changes in early retirement and mortality among eligible individuals.

Table 5

5.3 Mechanisms

This section tries to shed light on some of the potential mechanisms that could explain why early retirement increases male mortality.

Lifetime income. Retiring (early) is associated with a reduction in earnings and one might argue that this loss in earnings is the key driver behind our results. However, the different social insurance programs in Austria offer generous income replacement, which help to compensate the loss in earnings. Since our data contain detailed information on earnings and transfers from different social insurance programs, we can examine the extent to which early retirement is associated with a change in lifetime income. We follow the approach in Inderbitzin *et al.* (2016) and calculate, based on an individual's observed earnings history and retirement path, how much an individual receives in earnings, UI benefits, sick leave (SI) benefits, and pension (=DI+OA) benefits between ages 50-73 (measured in 2016 euros).[27]

Table 6 reports estimates of equation (2) using as dependent variable our measure for (lifetime) earnings or transfers from a program. We also report estimates for the total income, which is simply the sum of earnings and all transfers. The first column shows that the REBP is associated with a loss in lifetime earnings of 11,923 euros among men and 11,375 euros among

[27] Inderbitzin *et al.* (2016) calculate the fiscal costs of the REBP for a sample of male job losers between ages 50-59. Their calculations differ in several aspects: (1) they assume that everybody receives an OA pension after age 65 (while we use the effective number of years receiving an OA pension after age 65), (2) they assume that all individuals live to age 78 – the average life expectancy of men during that time period (while we calculate the effects up to age 73 and use individuals' effective date of death), and (3) they discount future income streams with a discount rate of 2.5% (while we assume a discount rate of 0%).

women. However, a significant portion of the earnings loss, about 30% among men and 55% among women, is compensated through additional UI benefits (2. column). Moreover, we also find that REBP-eligible men collect more pension benefits, because they are more likely to exit the labor force at age 55 through the DI program. Summing all up all transfers and earnings, we find that the REBP reduces total income by 3,845 euros among men and 4,465 euros among women, or about 1% relative to the baseline total income in CRs. Overall, these change in total income are too small to explain the increase in mortality.

Table 6

Contemporaneous income. However, if individuals do not smooth consumption due to liquidity effects or because they follow a rule-of-thumb, contemporaneous income may be more relevant for mortality than lifetime income. Therefore, in Figure 7 we plot estimates of how the REBP affects total income at each age (Figure A.2 in the Appendix reports the corresponding estimates for earnings, UI benefits, and pension benefits). The figure shows that the REBP reduced total income during ages 53-58 for men and ages 51-55 for women. Interestingly, for men these ages largely coincide with the ages where we see the steepest increase in cumulative mortality rates (Figure 6), suggesting that contemporaneous income shocks may matter.

Figure 7

Voluntary versus involuntary retirement. The impact of retirement on health may also differ depending on whether the retirement decision was voluntary or involuntary. In the data we cannot directly observe whether a job separation was involuntary or voluntary. As in the literature on job displacement, we deal with this limitation by defining involuntary separations to be those from firms experiencing a closure. Specifically, we separate REBP-eligible workers into those who did, and did not, work at a firm that closed when the worker was between 50 to 58 years old (50 to 53 years old for women).[28] Table 7 reports estimates when we interact REBP eligibility (years in early retirement) with a dummy for whether the worker's firm experienced a closure. Panel A shows that workers in closing firms spend 2-3 times more time in early retirement

[28]As in Fink *et al.* (2010), we say that a firm closes when it disappears from the data and less than 50% of the workforce transitions to the same, new firm (in order not to confound closures with takeovers.)

than workers in non-closing firms, suggesting that a firm closure often leads to (involuntary) early retirement. However, the impact of an additional year in early retirement on mortality before age 73 (Panel B) and the age at death (Panel C) is similar for both groups of workers. Hence, it appears that the impact of retirement on health does not per se depend on whether the retirement decision was voluntary or involuntary.

Table 7

Other reasons. Our estimates reflect the health effects of a change in the timing of retirement that was plausibly unanticipated by individuals. The REBP began in June 19988, less than two months after it was enacted by the parliament. It is possible that the health effects could differ for retirement decisions that are anticipated long in advance. The nature of our policy experiment precludes us from answering this issue directly, but previous studies find no clear relationship between health and whether retirement was anticipated or not. For example, Fitzpatrick and Moore (2018) find an increase in male mortality to an anticipated change in eligibility for U.S. Social Security benefits at age 62, while Hagen (2018) and Hernaes *et al.* (2013) find no mortality effect to changes in program eligibility that were largely anticipated.

Another potential explanation is that reduced employment and retirement-related lifestyle changes drive the increase in mortality. Fitzpatrick and Moore (2018) provide additional evidence that the increase in male mortality at age 62 in the U.S. is likely connected to retirement from work and retirement-related lifestyle changes. These channels likely play an important role in our context too, given that male mortality rates in TRs increase roughly at the same ages where we observe an increase in labor market exits. Moreover, the differential mortality patterns between blue- and white-collar men on the one side and women and men on the other are also suggestive that retirement-related lifestyle changes are important to the extent that they differ among those subgroups.

6 Conclusions

This paper provides novel evidence on the long-run effect of early retirement on mortality, an important and objective indicator of health. We define early retirement as the time span between the statutory retirement age and the age at labor force exit. Exploiting a large policy change in Austria's unemployment insurance system, which allowed eligible workers to exit work 3 years earlier, we document a significant drop in the effective retirement age of 5.5 months among men and 8.5 months among women.

Leveraging this variation in the retirement age in an instrumental variable approach, we find that an additional year in early retirement increases men's probability to die before age 73 by 1.47 percentage points (equivalent to a relative increase of 5.5%) and reduces the age at death by 2.2 months. This finding is robust to a variety of placebo and other specification checks. On the other hand, for women we find that early retirement is not associated with worse health outcomes, which is in line with previous evidence (e.g., Fitzpatrick and Moore, 2018). There are several reasons why female mortality is not affected by early retirement. It may be that women are more able to cope with major life events, have less unhealthy retirement-related lifestyle changes, and suffer less from a loss of social status than men.

However, one needs to exercise caution when interpreting our results. One caveat is that our estimates apply to workers who change their retirement behavior due to the policy change. They are more likely to work in blue-collar occupations, have some pre-existing health conditions, and have low-work experience. Another caveat is that the policy change allowed workers to retire from the labor force at a very early age compared to what is the norm in other countries. These caveats may limit the generalizability of our findings to other countries, and more research will be required to better understand the link between retirement and mortality.

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Table 1: Summary statistics

	Men				Women			
	TRs		CRs		TRs		CRs	
	Before/ after	During	Before/ after	During	Before/ after	During	Before/ after	During
A. Outcomes								
Retirement age (years)	58.2	57.3	58.8	58.1	55.1	54.4	55.7	55.5
Years in early retirement	6.75	7.74	6.20	6.89	4.87	5.60	4.35	4.51
Percent died by age 73	27.69	26.25	28.44	26.66	13.20	11.85	15.03	13.80
Age at death (years)	70.89	70.83	70.80	70.78	72.00	72.06	71.78	71.89
B. Background characteristics								
Years employed, ages 44-48	4.84	4.81	4.81	4.78	4.78	4.79	4.78	4.79
Years sick leave, ages 44-48	.050	.029	.045	.031	.029	.028	.033	.029
Years unemployed, ages 44-48	.076	.127	.082	.133	.110	.111	.106	.111
Daily wage at age 49 (euro)	89.1	94.3	92.0	96.8	65.6	68.7	70.7	73.9
Share blue collar at age 49	.621	.622	.525	.539	.517	.461	.419	.383
Share industries at age 49								
Agriculture	.110	.092	.048	.043	.042	.033	.015	.013
Utilities	.035	.032	.036	.032	.006	.009	.007	.009
Manufacturing	.391	.387	.339	.320	.319	.297	.250	.241
Construction	.148	.153	.154	.162	.030	.039	.027	.030
Wholesale trade	.105	.114	.143	.148	.174	.190	.211	.214
Accommodation	.012	.010	.020	.020	.044	.044	.071	.070
Transportation	.036	.042	.051	.059	.022	.024	.037	.035
Finance and insurance	.141	.147	.176	.183	.256	.259	.279	.282
Health care	.014	.013	.015	.015	.086	.086	.080	.082
Arts and entertainment	.007	.009	.018	.019	.019	.019	.022	.024
Observations	21,451	60,476	69,388	207,379	10,317	20,817	42,190	89,655

Notes: Sample consists of men born January 1927 to December 1944 and women born January 1932 to December 1944. "Before/after" denotes men (women) born January 1927 (1932) to May 1930 (1935) as well as men and women born August 1943 to December 1944. "During" denotes men born June 1930 to July 1943 and women born June 1935 to July 1943. Daily wage is adjusted for inflation.

Table 2: Effect of REBP on years in early retirement

	Men			Women
	All	Access to DI pathway[a]	Access to OA pathway[b]	All
A. Base Controls				
Coefficient	0.441***	0.439***	0.422***	0.722***
	(0.030)	(0.038)	(0.035)	(0.044)
Mean in CRs	6.89	6.87	6.90	4.51
Equality test (p-value)			0.700	0.000
First-stage F-statistic	221.1	132.1	141.9	274.3
Observations	358,694	220,528	258,161	162,979
B. Full controls				
Coefficient	0.458***	0.447***	0.465***	0.704***
	(0.028)	(0.036)	(0.033)	(0.042)
Mean in CRs	6.89	6.87	6.90	4.51
Equality test (p-value)			0.666	0.000
First-stage F-statistic	272.3	154.7	196.2	287.8
Observations	358,694	220,528	258,161	162,979

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. All regressions include dummies for birth year-month and region. Additional controls in Panel B are daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies industry at age 49 and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs. Equality test in column 3 (4) tests the null hypothesis that the first-stage coefficients for access to DI pathway and access to OA pathway (women and men) are identical.

[a] Access to DI pathway: men born January 1927 to May 1930 and August 1938 to December 1944. Treated birth cohorts were younger than age 55 during the REBP and could only use the REBP to exit the labor force via the DI pathway.

[b] Access to OA pathway: men born January 1927 to July 1938 and August 1943 to December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

Table 3: Effects of early retirement on mortality

	Men			Women
	All	Access to DI pathway[a]	Access to OA pathway[b]	All
A. Died by age 73				
OLS	2.43***	2.32***	2.53***	0.84***
	(0.02)	(0.03)	(0.03)	(0.02)
Reduced form	0.67**	0.74*	0.47	-0.11
	(0.33)	(0.41)	(0.42)	(0.40)
IV	1.47**	1.65*	1.02	-0.16
	(0.71)	(0.90)	(0.89)	(0.57)
Equality test IV (p-value)			0.537	0.075
Mean in CRs	26.66	24.66	28.60	13.80
B. Age at death				
OLS	-0.340***	-0.322***	-0.353***	-0.121***
	(0.003)	(0.003)	(0.003)	(0.002)
Reduced form	-0.085**	-0.079*	-0.091**	-0.027
	(0.034)	(0.041)	(0.044)	(0.039)
IV	-0.185**	-0.176*	-0.196**	-0.039
	(0.073)	(0.091)	(0.093)	(0.056)
Equality test IV (p-value)			0.856	0.118
Mean in CRs	70.78	70.90	70.65	71.89
Observations	358,694	220,528	258,161	162,979

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs. Equality test in column 3 (4) tests the null hypothesis that the IV-coefficients for access to DI pathway and access to OA pathway (women and men) are identical.

[a] Access to DI pathway: men born January 1927 to May 1930 and August 1938 to December 1944. Treated birth cohorts were younger than age 55 during the REBP and could only use the REBP to exit the labor force via the DI pathway.

[b] Access to OA pathway: men born January 1927 to July 1938 and August 1943 to December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

Table 4: Heterogeneity, men

	All	Blue-collar	White-collar	Past sick leave = 0	Past sick leave > 0	Low work experience	High work experience
A. Years in early retirement							
Coefficient	0.458***	0.558***	0.223***	0.422***	0.519***	0.494***	0.410***
	(0.028)	(0.033)	(0.049)	(0.037)	(0.041)	(0.037)	(0.039)
Equality test (p-value)			0.000		0.074		0.109
Mean in CRs	6.89	7.88	5.76	6.17	8.24	7.64	6.15
B. Died by age 73							
OLS	2.43***	3.02***	1.98***	2.12***	3.05***	2.65***	2.18***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)
Reduced form	0.67**	0.94**	-0.01	0.09	1.54***	0.91*	0.35
	(0.33)	(0.44)	(0.49)	(0.40)	(0.56)	(0.48)	(0.43)
IV	1.47**	1.69**	-0.03	0.22	2.97***	1.84*	0.85
	(0.71)	(0.78)	(2.19)	(0.94)	(1.07)	(0.96)	(1.03)
Equality test IV (p-value)			0.460		0.049		0.476
Mean in CRs	26.66	31.65	20.96	22.45	34.58	31.92	21.48
C. Age at death							
OLS	-0.340***	-0.444***	-0.262***	-0.288***	-0.452***	-0.377***	-0.300***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)
Reduced form	-0.085**	-0.113**	-0.006	-0.013	-0.186***	-0.137***	-0.025
	(0.034)	(0.048)	(0.047)	(0.040)	(0.063)	(0.052)	(0.043)
IV	-0.185**	-0.202**	-0.028	-0.031	-0.358***	-0.277***	-0.060
	(0.073)	(0.083)	(0.210)	(0.094)	(0.117)	(0.103)	(0.104)
Equality test IV (p-value)			0.442		0.029		0.136
Mean in CRs	70.78	70.31	71.31	71.19	69.99	70.28	71.26
Observations	358,694	197,071	161,623	225,884	132,810	178,572	180,122

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Blue/white collar measured at age 49. Past sick leave: years on sick leave between ages 39-48, low (high) experience: employment years last 15 years before age 49 below (above) median. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs. Equality tests in column 3, 5, and 7 test the null hypothesis that coefficients for white-collar, past sick leave > 0, and high work experience are identical to coefficients for blue-collar, past sick leave = 0, and low work experience, respectively.

Table 5: Robustness and placebo checks

	Men			Women
	All	Access to DI pathway[a]	Access to OA pathway[b]	All
A. Hazard rate				
Coefficient	1.030**	1.032*	1.023	0.981
	(0.015)	(0.020)	(0.020)	(0.034)
Equality test (p-value)			0.683	0.201
Observations	358,694	220,528	258,161	162,979
B. Border regions				
Years in early retirement	0.458***	0.442***	0.462***	0.697***
	(0.028)	(0.036)	(0.034)	(0.041)
Died by age 73	1.87***	2.21**	1.33	-0.24
(IV)	(0.72)	(0.92)	(0.91)	(0.57)
Equality test (p-value)			0.401	0.022
Age at death	-0.212***	-0.212**	-0.215**	-0.035
(IV)	(0.074)	(0.093)	(0.094)	(0.056)
Equality test (p-value)			0.976	0.062
Observations	310,440	190,802	222,628	144,532
C. Region of residence at age 40				
Years in early retirement	0.443***	0.414***	0.436***	0.707***
	(0.028)	(0.037)	(0.034)	(0.042)
Died by age 73	1.46**	1.72*	1.69*	0.00
(IV)	(0.72)	(0.93)	(0.92)	(0.56)
Equality test (p-value)			0.752	0.111
Age at death	-0.169**	-0.178*	-0.230**	-0.062
(IV)	(0.074)	(0.093)	(0.094)	(0.055)
Equality test (p-value)			0.978	0.248
Observations	358,694	220,528	258,161	162,979
D. <15 employment years				
Years in early retirement	-0.026	0.088	-0.136	0.139
	(0.082)	(0.104)	(0.110)	(0.091)
Equality test (p-value)	0.000	0.001	0.000	0.000
Died by age 73	-0.22	0.18	-0.33	0.10
(reduced form)	(0.56)	(0.69)	(0.74)	(0.38)
Equality test (p-value)	0.175	0.496	0.347	0.702
Age at death	0.010	-0.020	0.026	-0.023
(reduced form)	(0.065)	(0.079)	(0.087)	(0.038)
Equality test (p-value)	0.197	0.510	0.237	0.940
Observations	112,832	71,835	84,640	108,917

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs. Equality tests in column 3 (4) of Panels A, B, and C test the null hypothesis that the coefficients for access to DI pathway and access to OA pathway (women and men) are identical. Equality tests in Panel D test the null hypothesis that the coefficients for the placebo sample with less than 15 employment years are identical to those for the main sample (Tables 2 and 3).

[a] Access to DI pathway: men born January 1927 to May 1930 and August 1938 to December 1944. Treated birth cohorts were younger than age 55 during the REBP and could only use the REBP to exit the labor force via the DI pathway.

[b] Access to OA pathway: men born January 1927 to July 1938 and August 1943 to December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

Table 6: Effect of REBP on program costs and earnings between ages 50-73

	Earnings (A)	UI benefits (B)	Pension benefits (C)	SI benefits (D)	Total income (A+B+C+D)
A. Men					
Coefficient	-11,923***	3,355***	4,705***	18	-3,845***
	(822)	(261)	(718)	(46)	(953)
Mean in CRs	236,879	14,310	238,470	3,082	492,741
Observations	358,694	358,694	358,694	358,694	358,694
B. Women					
Coefficient	-11,375***	6,353***	822	-264***	-4,465***
	(857)	(247)	(815)	(41)	1,048)
Mean in CRs	125,902	8,970	247,946	1,368	384,186
Observations	162,979	162,979	162,979	162,979	162,979

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. The variables earnings, UI benefits, SI benefits, DI benefits, OA benefits, and total income measure earnings received, UI benefits received, pension (=DI+OA) benefits received, DI benefits received, OA benefits received, and total income received between ages 50 and 73. All amounts are in 2016 euros. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs.

Table 7: Voluntary vs involuntary retirement

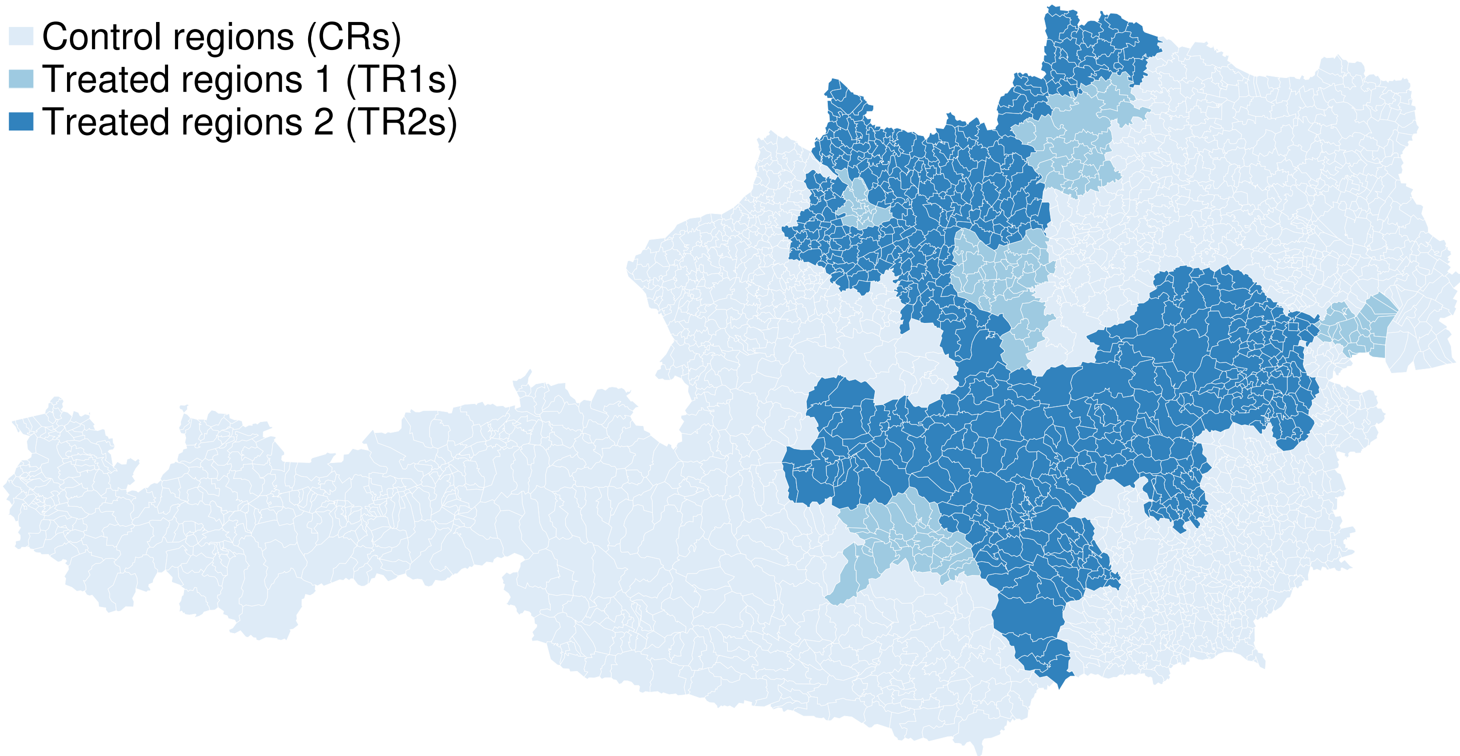
	Men			Women
	All	Access to DI pathway[a]	Access to OA pathway[b]	All
A. Years in early retirement				
Eligible, non-closing firm	0.407***	0.391***	0.420***	0.665***
	(0.028)	(0.036)	(0.034)	(0.042)
Eligible, closing firm	1.090***	1.098***	1.095***	2.389***
	(0.068)	(0.093)	(0.093)	(0.157)
Equality test (p-value)	0.000	0.000	0.000	0.000
B. Died by age 73 (2SLS)				
ER, non-closing firm	1.47**	1.67*	1.01	-0.16
	(0.71)	(0.89)	(0.89)	(0.57)
ER, closing firm	1.50**	1.85**	0.85	-0.03
	(0.66)	(0.83)	(0.83)	(0.49)
Equality test (p-value)	0.815	0.185	0.255	0.592
C. Age at death (2SLS)				
ER, non-closing firm	-0.185**	-0.175*	-0.195**	-0.039
	(0.073)	(0.090)	(0.093)	(0.056)
ER, closing firm	-0.172**	-0.170**	-0.176**	-0.023
	(0.068)	(0.084)	(0.087)	(0.047)
Equality test (p-value)	0.217	0.668	0.204	0.490
Observations	358,694	220,528	258,161	162,979
Eligible, non-closing firm	55,743	25,430	30,313	20,234
Eligible, closing firm	4,324	2,214	2,110	452

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Firm closure: firm exits and less than 50% of the workforce transits to the same new firm (see Fink *et al.*, 2010). Equality tests test the null hypothesis that the coefficients for "Eligible, non-closing firm" and "Eligible, closing firm" are identical.

[a] Access to DI pathway: men born January 1927 to May 1930 and August 1938 to December 1944. Treated birth cohorts were younger than age 55 during the REBP and could only use the REBP to exit the labor force via the DI pathway.

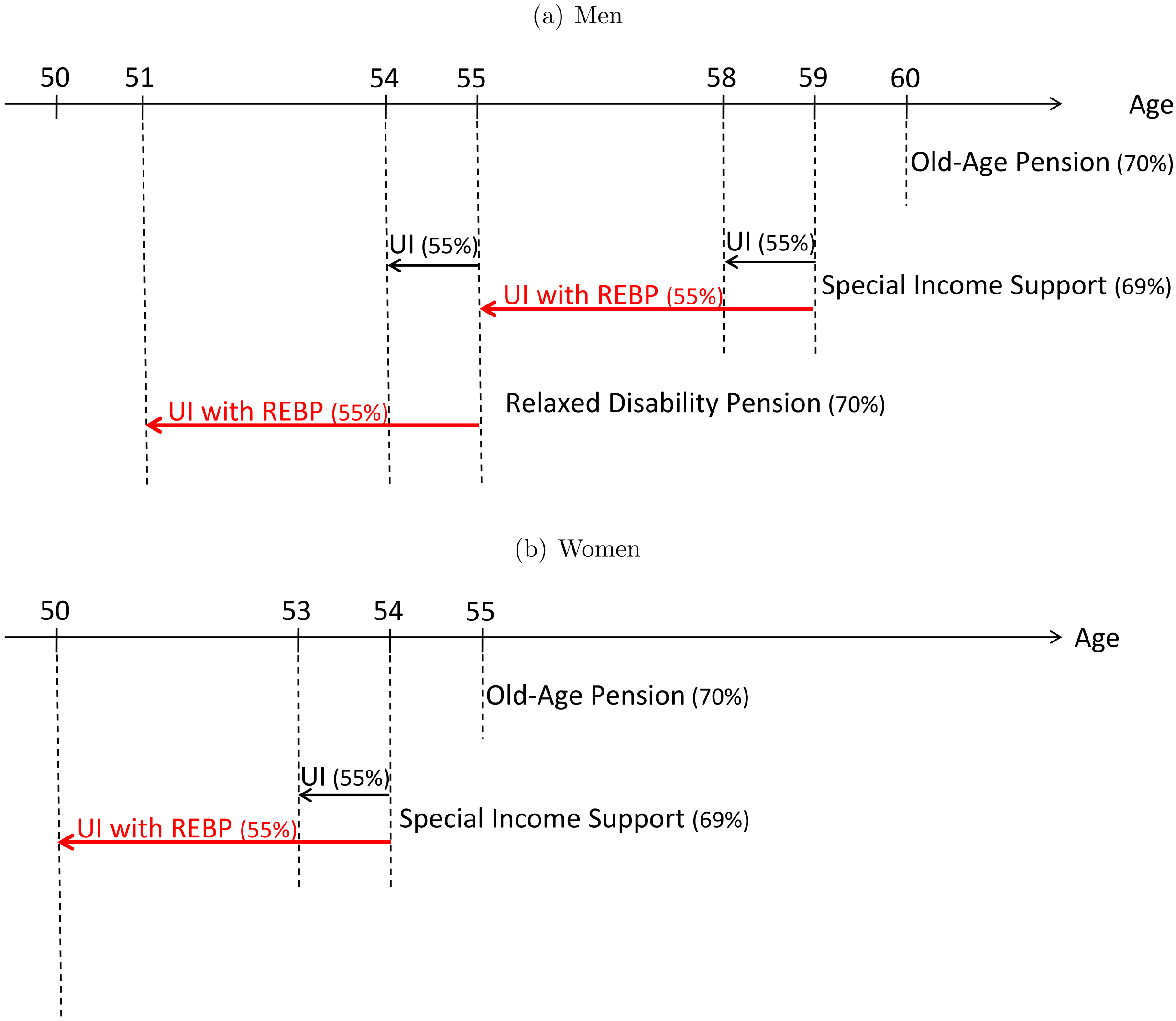
[b] Access to OA pathway: men born January 1927 to July 1938 and August 1943 to December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

Figure 1: Distribution of REBP Treatment and Control Regions



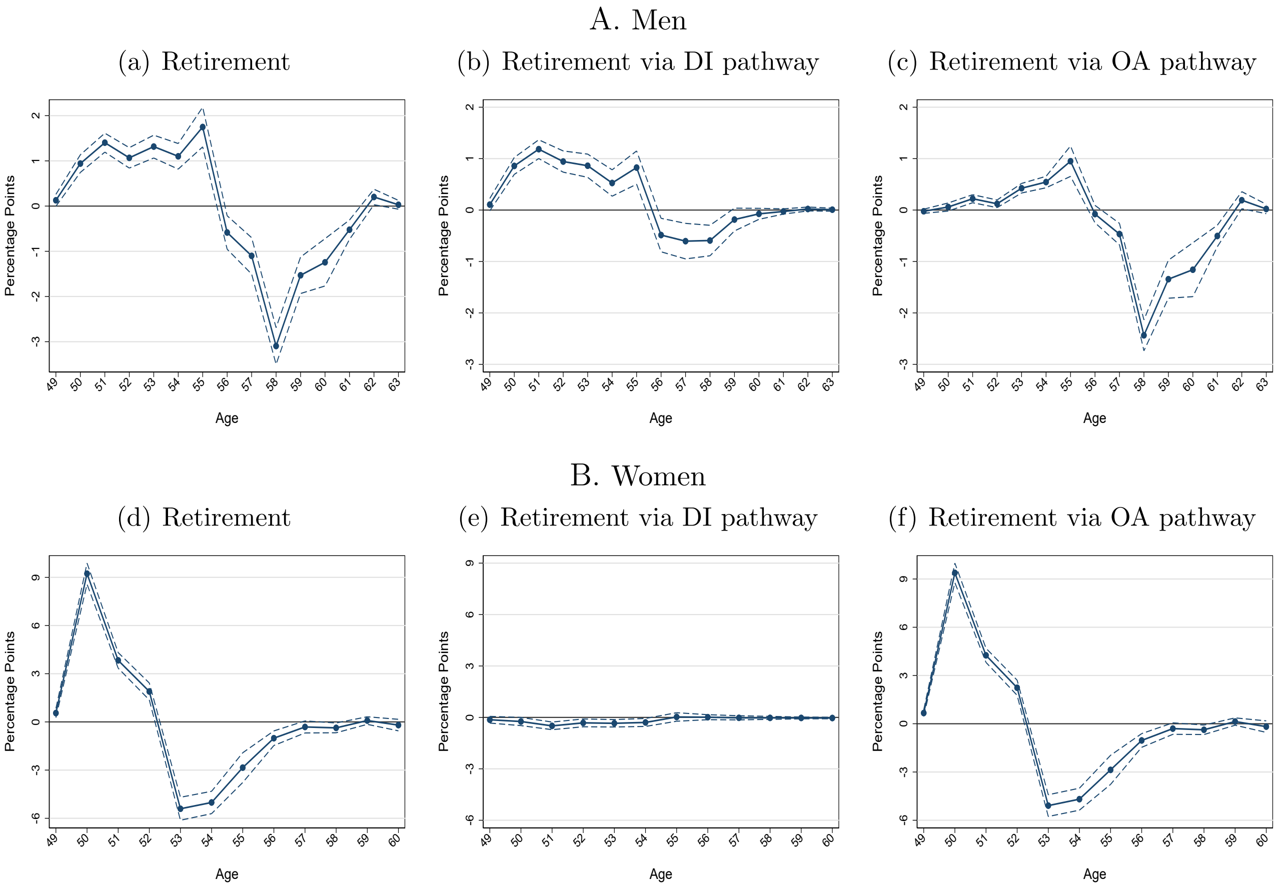
Notes: The figure shows the REBP and non-REBP regions. The REBP was introduced in June 1988 in TR1s and TR2s. The REBP was in effect until December 1991 (July 1993) in TR1s (TR2s).

Figure 2: Retirement Pathways with/without REBP Eligibility



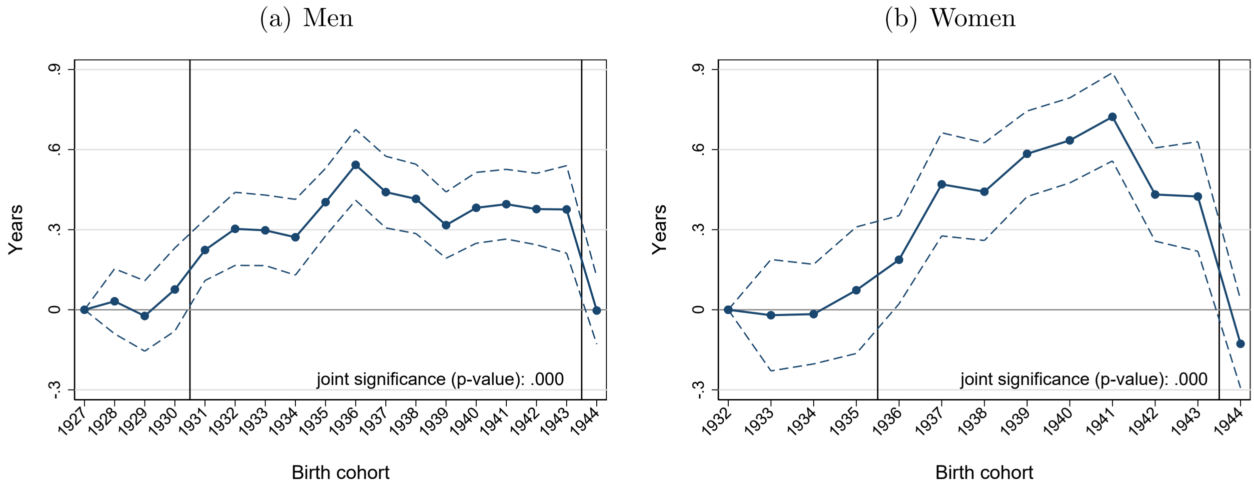
Notes: Black arrows denote maximum duration of regular UI benefits without REBP (1 year) and red arrows denote maximum duration of regular UI benefits with REBP (4 years). Percent numbers in parenthesis indicated the net replacement rates of the different benefits for an individual with average earnings. See text for details.

Figure 3: Effects on retirement at different ages by pathway



Notes: The figure plots the difference between TRs and CRs in the age-specific probability for men and women to retire, to retire via the DI pathway, and to retire via the OA pathway, respectively. Dashed lines show 90% confidence bands.

Figure 4: Effect of REBP on years in early retirement

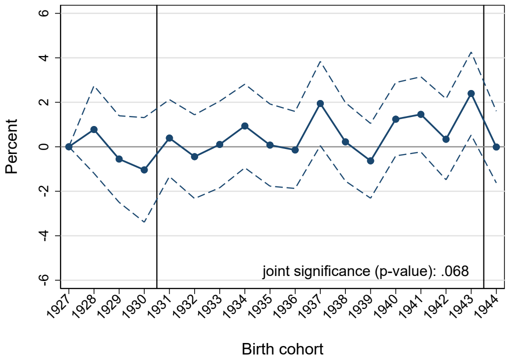


Notes: The figure plots the difference in the number of years spent in early retirement between TRs and CRs by annual birth cohorts for men and women. The reference category are men (women) born in 1927 (1932). Birth cohorts in between the two vertical lines could take advantage of the REBP. We lump men (women) born between January-May 1930 (1935) and between August-December 1943 with the birth cohort 1929 (1934) and 1944, respectively. Dashed lines show 90% confidence bands.

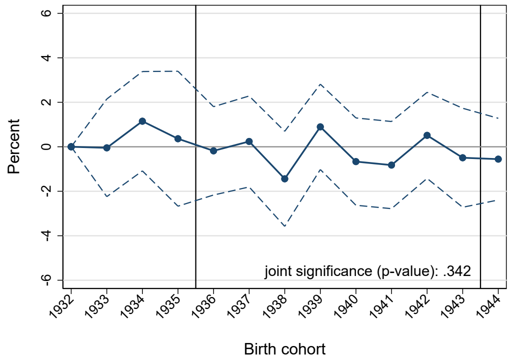
Figure 5: Effect of REBP eligibility on different measures of mortality

A. Probability to die before age 73

(a) Men

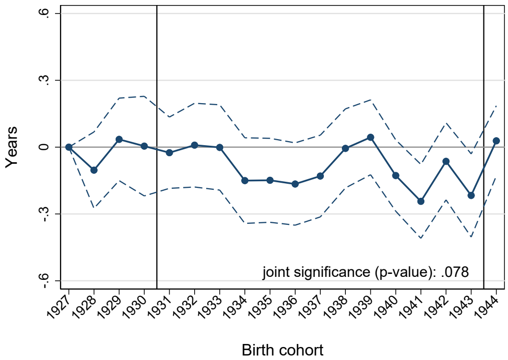


(b) Women

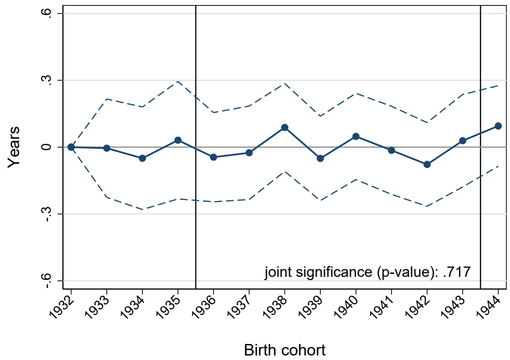


B. Age at death

(c) Men



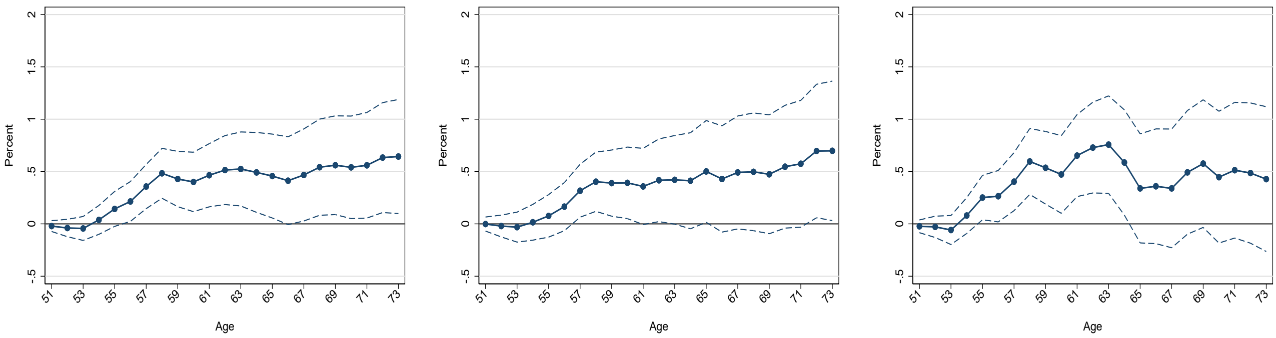
(d) Women



Notes: The figure plots the difference in the probability to die before age 73 (Panel A) and the age at death (Panel B), respectively, between TRs and CRs by annual birth cohorts for men and women. The reference category are men (women) born in 1927 (1932). Birth cohorts in between the two vertical lines could take advantage of the REBP. We lump men (women) born between January-May 1930 (1935) and between August-December 1943 with the birth cohort 1929 (1934) and 1944, respectively. Dashed lines show 90% confidence bands.

Figure 6: Effect of REBP on cumulative mortality at different ages, men

(a) All (b) Access to DI pathway[a] (c) Access to OA pathway[b]

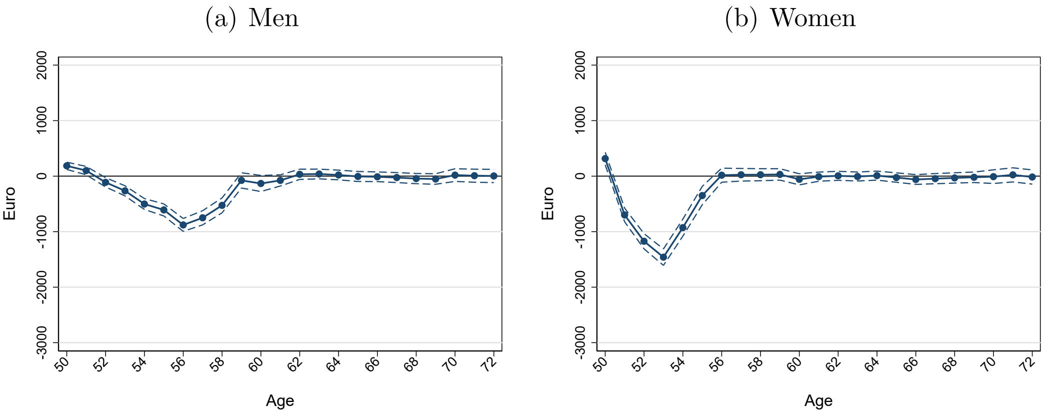


Notes: The figure shows reduced form estimates (and corresponding 90% confidence intervals) of the effect of the REBP on the probability to die before age 51, 52, ..., 73.

[a] Access to DI pathway: men born between January 1927-May 1930 and August 1938-December 1944. Treated birth cohorts were younger than age 55 during the REBP and could only use the REBP to exit the labor force via the DI pathway.

[b] Access to OA pathway: men born between January 1927-July 1938 and August 1943-December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

Figure 7: Effect of REBP on total income at different ages



Notes: The figure shows reduced form estimates (and corresponding 90% confidence intervals) of the effect of the REBP on the total income at age 50, 51, ..., 72.

A Additional Tables and Figures

Table A.1: Summary statistics for REBP-eligible individuals retiring via the DI or OA pathway

	Retiring via DI pathway	Retiring via OA pathway
A. Outcomes		
Retirement age (years)	55.9	59.6
Years of early retirement	9.42	5.38
Percent died by age 73	31.93	16.33
Age at death (years)	70.31	72.13
B. Background characteristics		
Years employed, ages 44-48	4.76	4.88
Years sick leave, ages 44-48	.040	.013
Years unemployed, ages 44-48	.159	.081
Daily wage at age 49 (euro)	90.7	99.2
Blue collar (%)	.733	.478
Agriculture (%)	.080	.107
Utilities (%)	.024	.043
Manufacturing (%)	.426	.341
Construction (%)	.183	.116
Wholesale trade (%)	.108	.123
Accommodation (%)	.010	.010
Transportation (%)	.044	.039
Finance and insurance (%)	.110	.191
Health care (%)	.010	.017
Arts and entertainment (%)	.006	.013
Observations	32,828	25,635

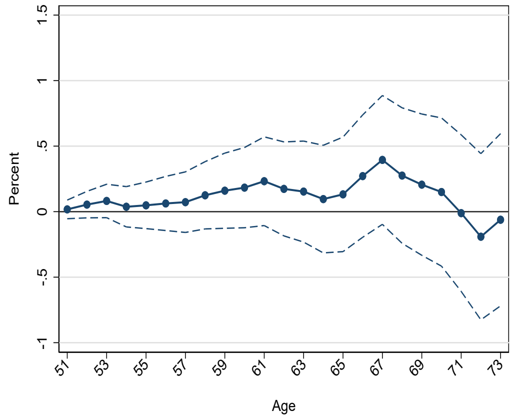
Notes: Sample consists of men born between June 1930 and July 1943 and women born between June 1935 and July 1943 who retire via the DI pathway (column 1) or via the OA pathway (column 2). Daily wage is adjusted for inflation.

Table A.2: Heterogeneity, women

	All	Blue-collar	White-collar	Past sick leave = 0	Past sick leave > 0	Low work experience	High work experience
A. Years in early retirement							
Coefficient	0.704***	0.866***	0.579***	0.623***	0.976***	0.706***	0.708***
	(0.042)	(0.052)	(0.062)	(0.051)	(0.069)	(0.060)	(0.056)
Equality test (p-value)			0.000		0.000		0.981
Mean in CRs	4.51	4.82	4.33	4.28	5.15	4.52	4.51
B. Died by age 73							
OLS	0.84***	0.96***	0.79***	0.78***	0.98***	0.86***	0.81***
	(0.02)	(0.04)	(0.02)	(0.02)	(0.05)	(0.03)	(0.03)
Reduced form	-0.11	-0.15	-0.24	-0.16	-0.25	-0.67	0.46
	(0.40)	(0.61)	(0.51)	(0.46)	(0.78)	(0.57)	(0.55)
IV	-0.16	-0.18	-0.41	-0.26	-0.25	-0.95	0.65
	(0.57)	(0.71)	(0.88)	(0.75)	(0.80)	(0.82)	(0.78)
Equality test IV (p-value)			0.834		0.993		0.152
Mean in CRs	13.8	16.16	12.38	12.53	17.24	14.85	12.81
C. Age at death							
OLS	-0.121***	-0.144***	-0.111***	-0.109***	-0.152***	-0.125***	-0.117***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.005)	(0.003)	(0.004)
Reduced form	-0.027	0.009	-0.047	-0.012	-0.034	0.011	-0.065
	(0.039)	(0.063)	(0.050)	(0.046)	(0.079)	(0.057)	(0.054)
IV	-0.039	0.010	-0.081	-0.019	-0.035	0.016	-0.091
	(0.056)	(0.073)	(0.086)	(0.074)	(0.081)	(0.081)	(0.076)
Equality test IV (p-value)			0.422		0.888		0.336
Mean in CRs	71.89	71.72	71.99	72.00	71.58	71.80	71.97
Observations	162,979	66,274	96,705	113,888	49,091	81,472	81,507

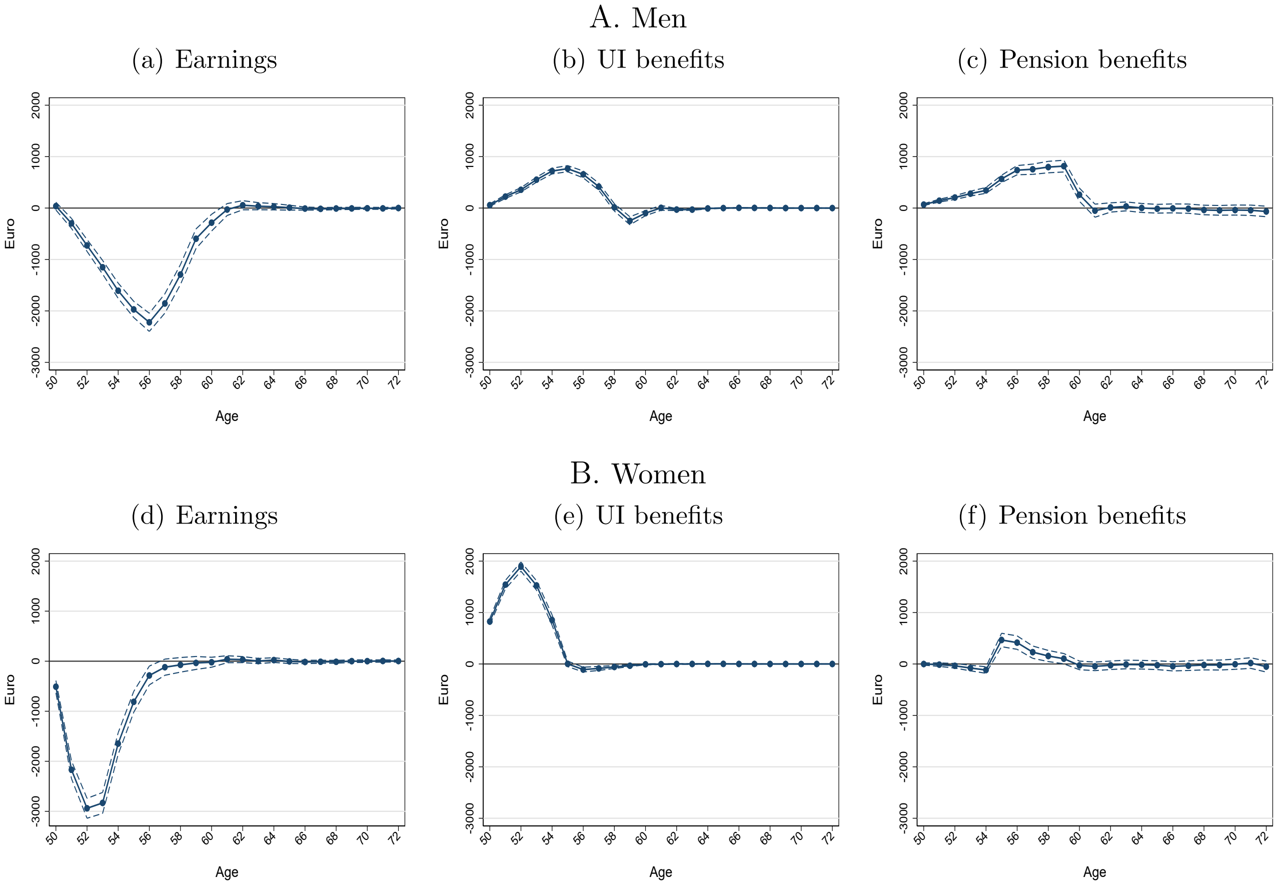
Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Blue/white collar measured at age 49. Past sick leave: years on sick leave between ages 39-48, low (high) experience: employment years last 15 years before age 49 below (above) median. Regressions include daily wage at age 49, number of years on sick leave, unemployed, and employed between ages 44-48, and dummies for birth year-month, region, industry at age 49, and blue-collar status at age 49. Reported means are for REBP-eligible birth cohorts in CRs. Equality tests in column 3, 5, and 7 test the null hypothesis that coefficients for white-collar, past sick leave > 0, and high work experience are identical to coefficients for blue-collar, past sick leave = 0, and low work experience, respectively.

Figure A.1: Effect of REBP on cumulative mortality at different ages, women



Notes: The figure shows reduced form estimates (and corresponding 90% confidence intervals) of the effect of the REBP on the probability to die before age 51, 52, ..., 73.

Figure A.2: Effect of REBP on earnings, UI benefits, and pension benefits at different ages



Notes: The figure shows reduced form estimates (and corresponding 90% confidence intervals) of the effect of the REBP on earnings, UI benefits, and pension benefits at age 50, 51, ..., 72 for men and women.