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DISCUSSION PAPER SERIES

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ABSTRACT

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

We estimate the causal effect of permanent and premature exits from the labor force on mortality. To overcome the problem of negative health selection into early retirement, we exploit a policy change in unemployment insurance rules in Austria that allowed workers in eligible regions to exit the labor force 3 years earlier compared to workers in non-eligible regions. Using administrative data with precise information on mortality and retirement, we find that the policy change induced eligible workers to exit the labor force significantly earlier. Instrumental variable estimation results show that for men retiring one year earlier causes a 6.8% increase in the risk of premature death and 0.2 years reduction in the age at death, but has no significant effect for women.

JEL Classification: I10, I12, J14, J26

Keywords: early retirement, mortality, health behavior, instrumental variable

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1 Introduction

In many industrialized countries, demographic changes put governments under increasing pressure to implement major reforms to old age social security systems. A particular focus of many reforms is to increase the effective retirement age by restricting access to early retirement schemes. Workers and their political representatives often strongly oppose such reforms. Among the most important arguments is that, after having worked all their lives in physically demanding jobs, workers should have the option to retire early and thus avoid emerging health problems. While leaving an unhealthy work environment is, ceteris paribus, clearly conducive to good health, the health effects of permanently exiting the labor force may go in the opposite direction. Indeed, the empirical evidence suggests that retirement is not only associated with lower income and fewer resources to invest in one's health, but also with less cognitive and physical activity (Bonsang *et al.*, 2012; Mazzonna and Peracchi, 2012; Rohwedder and Willis, 2010) as well as with changes in daily routines and lifestyles which are potentially associated with unhealthy behavior (e.g. Balia and Jones, 2008; Henkens *et al.*, 2008).

This paper presents new evidence on the causal effect of early retirement, defined as a permanent exit from the labor force before the retirement age, on mortality. To solve the problem of negative health selection into retirement, we take advantage of a major change to the Austrian unemployment insurance (UI) system, which extended the maximum duration of UI benefits for workers living in certain regions of the country for a limited time period. This unique policy change allowed older workers in eligible regions to withdraw 3 years earlier from employment than comparable workers in non-eligible regions. Exploiting differences in eligibility for extended UI benefits across regions and birth cohorts allows us to overcome the problem of reverse causality. Since the program generates variation in the retirement age that is arguably exogenous to individuals' health status, we can estimate the causal impact of early retirement on mortality using an instrumental variable (IV) estimation strategy.

Our study has three features, which are helpful to identify the impact of early retirement on mortality. First, our empirical strategy is based upon a policy change that generates a large shift in the earliest age at which individuals can permanently leave the labor force. Indeed, we find that men in eligible regions exit the labor force almost 6 months earlier than men in non-eligible regions. The effect is even larger among eligible women who exit the labor market up to 9 months earlier, on average. Second, the policy change took place in the late 80s, allowing us to follow individuals for several decades after the change. Third, we use administrative data containing precise and reliable information on both the timing of retirement and the date of death for the universe of private-sector workers in Austria. Austrian social security data are collected for the purpose of assessing individuals' eligibility to (and level of) social security benefits. Information on any individual's work history and the date of death is thus precise, and our estimates are unlikely to be contaminated by measurement error.[1]

Our empirical analysis can be summarized by four main findings. First, an earlier exit from the labor force has a strong effect on mortality among men but not women. An additional year in early retirement increases the risk of men dying before age 73 by 1.85 percentage points (equivalent to a relative increase of 6.8 percent). The increased mortality risk reduces men's age at death by 0.2 years for each additional year in early retirement. On the other hand, we find that for women early retirement is not associated with worse health outcomes. Second, our IV estimates are always considerably smaller than the corresponding ordinary least squares (OLS) estimates, which is consistent with selection into early retirement based on poor health. Third, we also document heterogeneity in the mortality effects across subgroups of the population. Men in blue-collar occupations, men with low-work experience, and men who have some pre-existing health impairment display higher mortality effects than men in white-collar occupations, men with high-work experience, and men in good health. Fourth, we show that early retirement is associated with a significant reduction in lifetime earnings. However, individuals compensate most of this earnings loss with transfers from other government transfer programs. As a consequence, the change in lifetime income associated with early retirement is negligible and cannot explain the increased mortality among men.

Our findings that early retirement increases mortality and reduces the age at death among men are robust to a variety of placebo and other specification checks. First, we find no significant

[1]Measurement problems are non-negligible when based on self-reported health measures, rather than an "objective" health indicators such as mortality. The distinction between subjective and objective measures appears to be of special relevance, as even self-reported measures of physical health may be subject to considerable reporting error (Baker *et al.*, 2004). It is likely that truly subjective measures of health, i.e. individuals' assessment of their well-being, perform even worse because of ex-post justification bias (e.g. Bertrand and Mullainathan, 2001). Indeed, studies using subjective health measures tend to find beneficial effects of retirement (Charles, 2004; Coe and Zamarro, 2011; Eibich, 2015; Grip *et al.*, 2012; Insler, 2014; Johnston and Lee, 2009; Neuman, 2008) and only a few find negative effects of retirement on subjective health (Behncke, 2012; Dave *et al.*, 2008). It is also conceivable that there is considerable measurement error with respect to retirement age in survey data, whereas such error is arguably of minor importance in administrative data.

regional differences in early retirement and mortality trends across eligible and non-eligible regions prior to the adoption and after the abolishment of extended UI benefits. Second, we repeat our analysis for a sample of men and women who are not eligible for extended UI benefits, because they have not contributed enough years to the UI system. For theses samples the IV estimates are always insignificant, while the OLS estimates are quantitatively large and highly significant. These two checks suggest that our estimates are not biased by pre-existing regional differences in early retirement and mortality trends. Third, our estimates are also robust to the underlying statistical model: we find similar effects if we estimate the mortality effects using a proportional hazard model instead of a linear probability model.

Recent studies on retirement and health that use mortality as the main outcome and try to establish causality have found mixed results.[2] 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. The find no causal effect of early retirement on the incidence to die before age 77, although the OLS estimates are negative and statistically significant. Bloemen *et al.* (2017) focus on Dutch civil servants and instrument individuals' actual retirement age using targeted early retirement programs. They 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.[3]

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

[2]A related literature looks at the impact of job loss on mortality. Studies using administrative data include Sullivan and von Wachter (2009) who estimate the effects of job displacement on mortality matching earnings and employment registers of Pennsylvanian workers to death records from the US social security register. They find mortality rates increase substantially after displacement, particularly for displaced long-tenured workers. Evidence supporting the idea that job loss increases mortality is also found in Eliason and Storrie (2009) for Sweden and Browning and Heinesen (2012) for Denmark.

[3]Indeed, for men the IV estimates of early retirement on mortality in Hernaes *et al.* (2013) are positive across all specifications and in some case quite large, but imprecisely estimated.

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.

Two recent studies by Fitzpatrick and Moore (2018) and Black *et al.* (2017) examine populations more similar to ours and also find a negative effect of retirement on mortality. Fitzpatrick and Moore (2018) study whether there is a change in aggregate mortality at age 62, the earliest eligibility age for social security benefits in the United States. Using population data, they find a 2% increase in male mortality immediately after age 62 and a much smaller and imprecisely estimated effect on female mortality. They argue that the increase in male mortality is connected to retirement from the labor force and associated lifestyle changes. Black *et al.* (2017) estimate the effect of Disability Insurance (DI) benefit receipt on U.S. mortality. To overcome the health selection problem, they exploit random DI assignment based on more or less lenient judges deciding on DI applications. It turns out that receiving DI slightly increases mortality within the first 10 years of benefit receipt. Because DI benefit receipt causes lower labor supply, Black *et al.* (2017) argue that this result is consistent with the claim that withdrawing from work increases mortality. This channel is likely to be important in our context as well, given that the extension in UI benefits is associated with a significant drop in labor supply.

The remainder of this paper is structured as follows. In section 2, we discuss the institutional background for Austria, focusing on the institutional features that underlie our IV strategy. Section 3 discusses the data source as well as the selection of our sample and presents some descriptive statistics. Details of our econometric framework are given in section 4. The results are presented in section 5. Section 6 concludes.

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 period.[4] 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 old age (OA) and disability insurance (DI) pensions, which are the main source of income in retirement. The formula for computing the level of benefits 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.[5] All pensions are subject to income taxation and mandatory health insurance contributions. The replacement rate after income and payroll taxes is on average 75% of the 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 sufficient insurance years may claim an OA pension at any age after 60 for men and 55 for women.[6] 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 percent of the prior net wage, subject to a minimum and maximum. On top of regular UI benefits, family allowances are paid. Individuals above age 50 can claim UI benefits for up to 52 weeks (30 weeks before August 1989). Individuals who exhaust the regular UI benefits can apply for unemployment assistance. These means-tested transfers last for an indefinite period and are about 70 percent of regular UI benefits. Unemployed men (women) aged 59 (54) or older can

[4]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.

[5]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).

[6]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.

claim "special income support," provided that they have contributed to the UI program for at least 15 out of the previous 25 years.[7] 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.[8] 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: (i) aged 50 and above, (ii) a continuous work history (15 employment years in the last 25 years), and (iii) at least 6 months of residence in one of the 28 eligible regions.[9]

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

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

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

[9] Previous evaluations of the REBP have found large effects of the program on realized unemployment durations (Lalive, 2008; Lalive and Zweimüller, 2004a,b), labor market exit among the unemployed (Inderbitzin *et al.*, 2016; Winter-Ebmer, 1998), and search spillovers (Lalive *et al.*, 2015).

new claims were no longer eligible for the benefit extension.

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 (SPÖ). 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 illustrates that the introduction of the REBP significantly increased the incentive to exit the labor force via the UI system. 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 shows that the REBP also increased the incentive to retire for men below age 55. More specifically, 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.[10]

Sample selection. Starting from the population data set, we impose four 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 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 49. Fourth, we concentrate on workers living

[10]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.

within 77 kilometer driving distance to the border between TRs and CRs.[11] This restriction implies that regional differences in access to health care and labor market conditions are unlikely to contaminate our estimates.[12] We choose 77 kilometers because the maximum driving distance from a treated region community to the nearest control region community is 76.5 kilometers. However, we also test the robustness of our results by contrasting all TRs and CRs.

Key variables. The key variables of our analysis are measures of early 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. Since workers in our sample have to be alive at age 50, this indicator measures whether or not an individual in our sample dies between ages 50 and 73. This is a meaningful indicator 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 early retirement on (i) the probability of premature death and (ii) the length of a life. We censor age at death at age 73 for those individuals who are still alive at age 73.

Our main treatment variable is the number of years an individual spends in early retirement. This variable measures the time span between the statutory retirement age (age 65 for men and age 60 for women) and the date when the individual permanently withdraws from working life. More precisely, we define the date of retirement as the day after the end of the individual's last regular employment spell.[13] Hence, a positive number on the treatment variable implies that an individual has retired before the statutory retirement age.

[11]The distance measure reflects the kilometers required to drive from the center of one community to the center of the closest community that is located on the other side of the border using the shortest road connection between these two communities (see Lalive (2008) for details).

[12]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.

[13]This definition implies that 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 permanently. Retired individuals in our sample can draw unemployment benefits, disability benefits, old-age benefits, some other type of benefit, or no transfer.

Descriptive statistics. In our analysis, we stratify the sample by gender because early retirement rules and mortality patterns differ for men and women. Our final sample consists of 310,440 men and 144,532 women. 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:

$$y_{icr} = \beta_0 + \beta_1 ER_{icr} + \mathbf{X'_{icr}}\beta_\mathbf{2} + \lambda_\mathbf{c} + \eta_\mathbf{r} + \epsilon_{icr}, \tag{1}$$

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 denotes the years spent in early retirement, i.e. the difference between the statutory retirement age and the date of permanent exit from work, $\mathbf{X}$ 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 is an error term.[14]

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.[15] To deal with unobserved health shocks, we instrument the years spent in early retirement by workers' eligibility for the REBP. Because the REBP induced individuals to retire early, as we document below, differences in REBP eligibility across regions and birth cohorts give rise to exogenous variation in the number of years an individual spends in early retirement.

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

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

where D_{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.[16] The corresponding reduced-form effect of REBP on mortality is estimated by the following equation:

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

in which y_{icr} is a mortality outcome.

We interpret the coefficient on ER from 2SLS estimation of equation (1) as a local average treatment effect of early retirement on mortality (Imbens and Angrist, 1994). In other words, the estimate of β_1 captures the causal effect of early retirement on mortality among the individuals who retire early with the REBP and who would not retire early without the REBP (i.e. the

[14]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.

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

[16]More specifically, D_{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 where age 56 when the REBP was introduced and we therefore set D equal to 2/3.

compliers). 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.

In order for REBP eligibility to be a valid instrument for early retirement, individuals' assignment of REBP eligibility must be 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 essentially boils down to whether there are unobserved region-specific shocks. One concern with this assumption is that workers may move from CRs to TRs in order to become eligible for the program. This is unlikely to be the case because eligibility rules require residence in a treated region for at least 6 months prior to claiming UI benefits. Moreover, mobility is low among older workers in Austria. For example, statistics from the Austrian census show that in 1991 only 3 percent (4 percent) of individuals aged 55-59 (50-54) moved across regions within states or across states within the last 5 years.

We test the validity of the identifying assumption in two 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, 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. Again, 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.

While the absence of unobserved region-specific shocks that are correlated with early retirement and mortality is sufficient for a causal interpretation of the reduced form effect of the REBP on mortality, we need three additional assumptions in order 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, 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.

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, in line with the monotonicity assumption, 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, a necessary condition to be eligible for the REBP. 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 during the REBP and could retire early via the OA pathway. Thus, contrasting these two subgroups is interesting to understand whether the mortality effects of early retirement depend on the choice of pathway.

Column 1 of Panel A shows that among men REBP eligibility increases the time spent in early retirement by 0.45 years or about 5.5 months. This represents a substantial 6.5 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). Column 4 of Panel A show that the estimate is significantly larger for women. Being eligible for the REBP increases the time spent in early retirement by 0.72 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 133 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. To further explore the impact of the REBP on early retirement, we estimate equation (2) for each age in the interval 49-63 for men and 49-60 for women separately. 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).

Figure 3 illustrates the results for men, with dots on the solid line showing the coefficient estimates and the dashed lines indicating 95 percent confidence intervals. As shown in Panel (a), 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. After age 61, we do not see significant differences in the probability to retire between TRs and CRs.

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 when turning age 55, the age at which DI eligibility rules are relaxed. Retirement via the DI pathway is lower between ages 56 and 58, suggesting that some men who, without the REBP, would have retired via the DI pathway between ages 56 and 58 use the REBP to retire via the OA pathway or via the DI pathway at an earlier age. Consistent with this idea, Panel (c) shows a significant spike at age 55 in the fraction of men in TRs who retire via the OA pathway.

Figure 3

Figure 4 shows the corresponding estimates for women. As Panel (a) illustrates, the difference in the probability to retire between TRs and CRs during the REBP relative to before/after the REBP is close to zero at age 49, but displays a large spike at age 50. Age 50 is the earliest age at which women can retire through the UI system during the REBP (Figure 2). The probability to retire is also significantly higher at ages 51 and 52 for women in TRs relative to CRs. In contrast, the incidence of retirement between ages 53 and 56 is significantly lower and becomes insignificant at ages 57 to 60.

Panel (b) shows that the REBP had almost no effect on retirements via the DI pathway among women. The point estimates are negative and statistically significant between ages 50 to 54, suggesting that some women who would have retired via the DI pathway use the REBP to bridge the time until age 55 when they become eligible for an OA pension. However, this substitution effect from the DI pathway to the OA pathway is quantitatively small. Instead, Panel (c) suggests that the REBP affected retirement behavior of women primarily through the possibility to exit the labor force at age 50 and receive UI benefits for 5 years, and then claim an OA pension at age 55.

Figure 4

Differential trends. We next examine whether the size of the early retirement response varies across birth cohorts. More specifically, we generalize equation (2) by replacing D_{icr} with a set of birth cohort (at half-year intervals) times TR interaction terms (where TR is an indicator taking the value one if an individual lives in a TR). These interaction terms also offer the possibility to test whether early retirement trends are the same across regions before and after the REBP. Evidence for differential trends in the absence of the REBP would suggest that the assumption of no unobserved region-specific shocks is violated, and estimates are biased.

Figure 5 plots the estimated coefficients of the interaction terms with years spent in early retirement as dependent variable. Several things can be observed from the figure. First, the time spent in early retirement is significantly higher for birth cohorts eligible for the REBP (those in between the two vertical lines) in TRs relative to CRs. Consistent with Table 2, the point estimates are larger for women (panel b) compared to men (panel a). 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 (age 58 for men and age 53 for women) 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. This finding supports the assumption that trends in early retirement between TRs and CRs would have been similar in the absence of the REBP and are not affected by unobserved region-specific shocks.

Figure 5

5.2 The Effect of Early Retirement on Mortality

Main results. In this section we present our main estimates of the causal effect of early retirement on mortality. Table 3 reports estimates for the equation (1) using OLS and IV (estimated by 2SLS) as well as the reduced form effects of the REBP on mortality. We examine two mortality outcomes: the incidence of death before age 73 (measured in percent) and the age at death in years (censored at age 73). The first row of Panel A presents the OLS estimates of equation (1). We find that among men an additional year in early retirement is associated with a 2.48 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% (=2.48/27.28). 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 estimates for women are about three times smaller, but also highly significant. These are still sizeable effects when expressed in relative terms because women are less likely to die before age 73.

The second row of Panel A presents reduced form estimates of the REBP on the probability to die before age 73. We find that being eligible for the REBP increases the probability to die before age 73 by 0.84 percentage points among men. The effect is slightly larger for men who have access to the DI pathway (0.96 percentage points) and slightly smaller (and statistically insignificant) for men who have access to the OA pathway (0.60 percentage points). In contrast, we do not find a statistically significant effect for women. The third row of Panel A presents the IV estimates of the impact of early retirement using the REBP as an instrument. The IV estimates are consistently smaller than the OLS estimates, but quantitatively still sizeable. The IV estimate implies that an additional year in early retirement increases the probability to die before age 73 by 1.85 percentage points (6.8%) among all men, 2.21 percentage points (8.8%) among men with access to the DI pathway, and by 1.30 percentage points (4.6%) among men with access to the OA pathway, although this estimate is not statistically significant. Consistent with the reduced form estimates, we do not find a significant effect for women.

Panel B of Table 3 reports analogous estimates of the effect of early retirement on the age at death. This outcome has not been examined in previous studies, even though it has the advantage to capture the effect of early retirement on the likelihood of death (extensive margin) and the length of a life (intensive margin). The estimates for the age at death 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 -0.330 years to -0.362 years, while for women the estimate is about three times smaller in absolute terms (-0.122 years).

The second row of Panel B presents reduced form estimates of the REBP on the age at death. We find that being eligible for the REBP reduces men's age at death by 0.095 years and this effect is statistically highly significant. The reduction in the age at death appears to be similar for men with access to the DI pathway (-0.090 years) or the OA pathway (-0.098 years). In contrast, the reduced form estimate is about four times smaller for women (-0.026 years) and statistically insignificant. The IV results (third row of Panel B) suggests that an additional year in early retirement is associated with a reduction in the men's age at death of 0.208 years, which is about two thirds of the corresponding OLS estimate, and has no effect on women's age at death. The estimates are similar for men with access to the DI pathway (-0.207 years) or the OA pathway (-0.213 years), suggesting that the effect of early retirement on the age at death does not depend on the choice of retirement pathway per se.

Table 3

Other studies that look at mortality as their main outcome variable have found effects that are broadly consistent with ours, although some are imprecisely estimated and thus not statistically significant. For example, Hernaes *et al.* (2013) find that a one-year reduction in the effective retirement age increases the probability to die before age 74 by 2.4 percentage points (not statistically significant). Black *et al.* (2017) find that the disability insurance benefit receipt increases the mortality within the first 10 years by 1.44 percentage points (statistically significant at the 10%-level). Fitzpatrick and Moore (2018) find that male retirement at the earliest eligibility age for social security benefits in the U.S. increases mortality by about 20%, which is larger than the 6.8% increase for men we estimate. One reason is that the baseline mortality at age 62 is much lower than at age 73. Two exceptions are Bloemen *et al.* (2017) and Hallberg *et al.* (2015) who find that early retirement reduces mortality among civil servants and army officers.

Differential trends. Figure 6 plots the estimated coefficients of the interaction terms with the probability to die before age 73 (Panel A) and the age at death (Panel B) as dependent variables. The figure shows that the probability to die before 73 and the age at death are not significantly different between TRs and CRs for birth cohorts who are not eligible for the REBP because they were too old when the REBP was introduced (those to the left of the first vertical line) or too young when the REBP was abolished (those to the right of the second vertical line). This finding again supports the claim that our estimates do not simply capture pre-existing difference in mortality trends between TRs and CRs. The point estimates are mostly insignificant for birth cohorts eligible for the REBP. For men they 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.

Figure 6

Cumulative mortality. Our estimates above suggest that early retirement increases the probability of a premature death. However, these estimates are not informative at which age mortality rates start to accelerate. To examine this issue in more detail, we separately estimate the reduced form effect of the REBP on the probability to die before age 51, 52, 53, ..., 73 using regression specification (3). Panel A of Figure 5 plots the estimated δ_1-coefficients for the full sample of men. It shows that the REBP had no effect on the probability to die before age 55 for eligible men in TRs relative to CRs. After this age the probability to die starts to increase among eligible men in TRs relative to CRs up to age 58 – the age at which men in CRs can start to retire early via the OA pathway. The difference in the probability to die across TRs and CRs continues to increase after age 58, albeit at a slower rate, and reaches a maximum of 0.84 percentage points at age 73.

Panel B of Figure 5 plots the estimated δ_1-coefficients when we restrict the during REBP cohorts to those who had access to the DI pathway. The same pattern is visible as for the full sample, and even more pronounced. The difference in the probability to die across TRs and CRs starts to increase continuously after age 55. At age 73 the difference in the cumulative mortality for men in TRs relative to CRs reaches 0.96 percentage points. This finding suggests that among men with access to the DI pathway excess mortality in TRs relative to CRs continuously increases with age.

Panel C of Figure 5 plots the estimated δ_1-coefficients when we restrict the during REBP cohorts to those who had access to the OA pathway. We see that the difference in the cumulative mortality rate across TRs and CRs starts to increase quite steeply at around age 55 and reaches a peak at age 63 after which it is quite stable at a rate slightly below the rate at the peak. This finding suggests that most of the excess mortality in TRs occurs between the ages 55 to 63, while mortality rates follow a similar pattern in TRs and CRs after age 63. This pattern also explains why Table 3 shows a significant effect of early retirement on the age at death but not on the probability to die before age 73 among men who had access to the OA pathway.

Figure 7

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, defined by observed characteristics. 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. The first column in each panel displays the results of our baseline sample for comparison. We focus on men here because for women we did not find a significant causal effect of early retirement on mortality.[17]

Panel A of Table 4 presents first-stage estimates of the effect of the REBP on the number of years spent in early retirement. The coefficient is significantly positive for all groups, and its magnitude varies substantially across groups. For example, eligible men in blue collar occupations spend an additional 0.559 years in early retirement, while the estimate is only 0.231 years for eligible men in white collar occupations. We also find a stronger early retirement effect among men who have spent some time on sick leave in the 10 years before age 50 (0.525 years) relative to those who have not (0.419 years). Lastly, we find that eligible men with low work experience are more responsive to the REBP incentives than those with high work experience.

In Panel B of Table 4 we present estimates of the effect of early retirement on the incidence of death before age 73 for the same subgroups for which we have estimated the first stage. The OLS estimates are always positive and statistically significant, and they vary from 2.00 percentage points (white collar workers) to 3.11 percentage points (blue collar workers). The IV estimates are consistently smaller than the OLS estimates, except for men who have spent some time on sick leave before age 50, and there are striking differences among certain subgroups. We find that an additional year in early retirement increases the probability to die before age 73 by 1.91 percentage points for blue collar men, 3.45 percentage points among men who have spent some time on sick leave, and by 2.42 percentage points among men with low work experience. The reduced form estimates are also statistically significant for all these subgroups and smaller in magnitude than the IV estimates because they are not scaled up by the first stage estimate. By contrast, the IV and reduced-form estimates are statistically insignificant for white collar men, men who have not spent time on sick leave, and men with high work experience.

Panel C of Table 4 reports analogous estimates of the effect of early retirement on the age at death. An advantage of this measure is that it captures the effect of early retirement on both the likelihood of death and the length of a life. The estimates for the age at death show a similar pattern as the estimates for the incidence of death. The OLS estimates are large and statistically significant in all cases, they range from -0.267 years to -0.464 years. The corresponding IV estimates are generally smaller and more heterogenous than the OLS estimates.

[17]Indeed, performing a similar heterogeneity analysis for women yields insignificant IV estimates of the effect of early retirement on mortality for all subgroups, despite the fact that the first stage is always large and highly significant.

The IV estimate imply that an additional in early retirement reduces the age at death by 0.227 years among men in blue-collar jobs, by 0.39 years among men who have spent some time on sick leave, and by 0.326 years among men with low 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.

Table 4

5.3 Robustness and Mechanisms

Placebo and Robustness. Table 5 presents several placebo and robustness 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. 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. The estimates confirm our findings when using the age at death or the probability of death before age 73 as mortality outcomes. Being eligible for the REBP is associated with a 3.5% 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 DI pathway compared to men with access to the OA pathway, but the difference is not statistically significant.

Our main analysis focuses on individuals living within 77 kilometers driving distance to the border between TRs and CRs. A natural question to ask is whether our findings are robust when we extend the sample to include all regions in Austria. Panel B reports estimates for the same birth cohorts as in our main sample when we include all regions in Austria. The first row of Panel B shows that men and women in TRs spend significantly more years in early retirement during the REBP than their counterparts in CRs. The point estimates are very similar to those using the geographically restricted sample. The second and third rows show the corresponding IV estimates for the incidence of death before age 73 and the age at death. As for the restricted sample, we find that early retirement is associated with a higher incidence of death before age 73 and a lower age at death for men but not for women. The point estimates are somewhat smaller compared to the restricted sample, but economically still meaningful. Specifically, we find that among men an additional year in early retirement increases the incidence of death before age 73 by 1.45 percentage points and reduces that age at death by 0.18 years.

Panel C reports estimates from a placebo test in which we estimate the same set of regressions for the sample of individuals who have less than 15 employment years. These individuals are not eligible for the REBP and we should therefore not find any significant effects of the REBP on early retirement and mortality for this sample. Indeed, all point estimates are statistically insignificant and much smaller in magnitude than for the main sample. 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

Mechanisms. Another question of interest is what is the mechanism that is driving these results. One potentially important determinant of mortality is income. In particular, retiring (early) is associated with a loss 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 whether early retirement is associated with change in lifetime income, i.e. the sum of lifetime earnings and government transfers. Specifically, 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 total earnings, UI benefits, SI benefits, DI benefits, and OA benefits between ages 50-73 (measured in 2016 euros).[18]

Table 6 reports estimates of equation (2) using as dependent variable our measure for either total earnings or total transfers from a specific program. We also report estimates for the total income between ages 50-73, which is simply the sum of earnings and all transfers. Panel A shows that among men the REBP is associated with a loss in earnings of 11,837 euros, or about 4.9% relative to the baseline in CRs. However, it turns out that about 40% of the earnings loss is

[18]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 life 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%).

compensated through an increase in UI benefits. On average, men in TRs collect an additional 4,860 euros in UI benefits. Moreover, we also find a significant increase in the amount of DI benefits that people collect (4,794 euros) because they are more likely to exit the labor force at age 55 through the DI program. On the other hand, we find no significant changes in SI benefits or OA benefits. Summing all up all transfers and earnings, we find that the REBP reduces men's total income between ages 50-73 by 1,893 euros.

Panel B shows analogous estimates for women. We find that the REBP reduces women's total earnings between ages 50-73 by 11,189 euros, but about 80% of the loss in earnings is compensated by additional UI benefits (8,959 euros). Interestingly, we find that eligible women collect slightly less DI benefits and more OA benefits. The reason is that some women who in the absence of the REBP would have entered the DI program, now use the extended UI benefits to bridge the time until age 55 when they become eligible for an OA pension. Summing up all the different income sources, we find that the total income of women in TRs slightly declines during the REBP, but the point estimate is not statistically significant.

In sum, the estimates in Table 6 suggest that changes in lifetime income cannot explain the increase in mortality associated with early retirement. We find that the total income between ages 50-73 declines by only 0.4% among men and by less than 0.3% among women. Another potential explanation is that reduced employment and retirement-related lifestyle changes drive the increase in mortality. Two recent studies by Fitzpatrick and Moore (2018) and Black *et al.* (2017) find a negative effect of retirement on mortality and provide additional evidence that the increase in mortality is likely connected to retirement from the labor force and associated lifestyle changes. This channel may also play an important in our context, given that the extension in UI benefits led to a significant drop in employment. Moreover, the differential mortality patterns we find 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.

Table 6

6 Conclusions

In this paper, we estimate the causal effect of early retirement on mortality. To resolve the problem of negative health selection into early retirement, we exploit a policy change to the Austrian unemployment insurance system, which allowed workers in eligible regions to withdraw permanently from employment 3 years earlier than workers in non-eligible regions. The program generated substantial exogenous variation in the effective early retirement age: eligible male (female) workers retired, on average, almost 6 (9) months earlier than their non-eligible counterparts. This provides us with an empirical design to identify the causal impact of early retirement on mortality using an instrumental variable approach.

We find that the reduction in the retirement age causes a significant increase in the risk of dying before age 73 and a significant reduction in the age at death among men. Specifically, an additional year in early retirement increases men's probability to die before age 73 by 1.85 percentage points (equivalent to a relative increase of 6.8 percent) and reduces the age at death by 0.2 years. On the other hand, we find that for women 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. Our IV estimates are consistently smaller than the corresponding ordinary least squares estimates, which highlights the importance of controlling for selection into early retirement based on poor health.

Our finding that early retirement has a negative effect on male mortality is robust to a variety of placebo and other specification checks. We find no differences in mortality and early retirement trends between eligible and non-eligible regions prior to the extension of UI benefits and after its abolishment. We also run a placebo analysis for a sample of men and women who are not eligible for the benefit extension and find no statistically significant effects. Finally, we show that early retirement is associated with a significant reduction in lifetime earnings. However, individuals compensate most of this earnings loss with transfers from other government transfer programs. Thus, the change in lifetime income associated with early retirement is negligible and cannot explain the increased mortality among certain groups of the population.

From a policy perspective, our results suggest that policies forcing older workers into early retirement may be detrimental for their health. Policies that foster employment of older workers can therefore generate a double dividend. They not only improve government budgets, they would also increase individuals' welfare by prolonging their lives, particularly for men. Labor market policies should therefore try to incentivize both firms to keep older workers in employment and workers to abstain from premature retirement.

However, one needs to exercise caution: 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. Moreover, 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. This may limit the generalizability of our findings, 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)	58.3	57.3	58.8	58.1	55.1	54.4	55.6	55.4
Years in early retirement	6.75	7.75	6.23	6.94	4.87	5.60	4.41	4.60
Percent died by age 73	27.69	26.25	29.29	27.28	13.20	11.85	15.27	14.10
Age at death (years)	70.89	70.83	70.72	70.71	72.00	72.06	71.75	71.86
B. Background characteristics								
Years employed, ages 44-48	4.84	4.81	4.82	4.79	4.78	4.80	4.78	4.79
Years sick leave, ages 44-48	0.050	0.029	0.047	0.031	0.029	0.028	0.034	0.030
Years unemployed, ages 44-48	0.076	0.127	0.080	0.130	0.110	0.111	0.105	0.109
Daily wage at age 49 (euro)	89.1	94.3	92.1	97.0	65.6	68.7	71.1	74.5
Share blue collar at age 49	.621	.622	.526	.541	.517	.461	.418	.381
Share industries at age 49								
Agriculture	.110	.092	.044	.040	.042	.033	.015	.013
Utilities	.035	.032	.031	.028	.006	.009	.007	.008
Manufacturing	.391	.387	.358	.338	.319	.297	.259	.250
Construction	.148	.153	.148	.156	.030	.039	.026	.030
Wholesale trade	.105	.114	.143	.147	.174	.190	.208	.213
Accommodation	.012	.010	.018	.018	.044	.044	.066	.065
Transportation	.036	.042	.049	.055	.022	.024	.038	.035
Finance and insurance	.141	.147	.175	.183	.256	.259	.278	.282
Health care	.014	.013	.016	.015	.086	.086	.081	.082
Arts and entertainment	.007	.009	.018	.019	.019	.019	.022	.024
Observations	21,438	60,452	57,803	170,747	10,317	20,815	36,523	76,877

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

Table 2: Effect of REBP on years in early retirement

	Men			Women
	Full sample	Access to DI pathway[a]	Access to OA pathway[b]	
A. Base Controls				
Coefficient	0.449***	0.445***	0.423***	0.716***
	(0.030)	(0.039)	(0.036)	(0.044)
Mean in CRs	6.94	6.92	6.88	4.60
F-statistic	224.9	132.5	139.5	270.0
B. Full controls				
Coefficient	0.455***	0.435***	0.460***	0.698***
	(0.028)	(0.036)	(0.034)	(0.042)
Mean in CRs	6.94	6.92	6.88	4.60
F-statistic	261.3	143.3	187.3	282.2
Observations	310,440	190,802	222,628	144,532

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 control variables are 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. Reported means are for REBP-eligible birth cohorts in CRs. Full sample consists of men born between January 1927 and December 1944 and women born between January 1932 and December 1944.

[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.

Table 3: Effects of early retirement on mortality

	Men			Women
	Full sample	Access to DI pathway[a]	Access to OA pathway[b]	
A. Died by age 73				
OLS	2.48***	2.36***	2.58***	0.84***
	(0.02)	(0.03)	(0.03)	(0.02)
Reduced form	0.84**	0.96**	0.60	-0.16
	(0.33)	(0.41)	(0.43)	(0.40)
IV	1.85**	2.21**	1.30	-0.23
	(0.73)	(0.93)	(0.91)	(0.57)
Mean in CRs	27.28	25.17	28.41	14.10
B. Age at death				
OLS	-0.349***	-0.330***	-0.362***	-0.122***
	(0.003)	(0.004)	(0.004)	(0.003)
Reduced form	-0.095***	-0.090**	-0.098**	-0.026
	(0.035)	(0.042)	(0.045)	(0.039)
IV	-0.208***	-0.207**	-0.213**	-0.037
	(0.074)	(0.094)	(0.095)	(0.056)
Mean in CRs	70.71	70.85	70.64	71.86
Observations	310,440	190,802	222,628	144,532

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, dummies for region, 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. Reported means are for REBP-eligible birth cohorts in CRs. Full sample consists of men born between January 1927 and December 1944 and women born between January 1932 and December 1944.

[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.

Table 4: Heterogeneity, men

	Full sample	Blue-collar	White-collar	Past sick leave = 0	Past sick leave > 0	Low work experience	High work experience
A. Years early retirement							
Coefficient	0.455***	0.559***	0.231***	0.419***	0.525***	0.480***	0.418***
	(0.028)	(0.033)	(0.050)	(0.037)	(0.041)	(0.038)	(0.039)
Mean in CRs	6.94	7.91	5.8	6.21	8.29	7.71	6.18
B. Died by age 73							
OLS	2.48***	3.11***	2.00***	2.15***	3.11***	2.71***	2.20***
	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)
Reduced form	0.84**	1.07**	0.35	0.23	1.81***	1.16**	0.43
	(0.33)	(0.45)	(0.50)	(0.40)	(0.57)	(0.49)	(0.43)
IV	1.85**	1.91**	1.53	0.54	3.45***	2.42**	1.03
	(0.73)	(0.79)	(2.12)	(0.95)	(1.07)	(1.00)	(1.02)
Mean in CRs	27.28	32.12	21.58	22.98	35.23	32.64	21.97
C. Age at death							
OLS	-0.349***	-0.459***	-0.267***	-0.294***	-0.464***	-0.390***	-0.304***
	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)
Reduced form	-0.095***	-0.127***	-0.028	-0.022	-0.205***	-0.156***	-0.023
	(0.035)	(0.048)	(0.048)	(0.040)	(0.064)	(0.052)	(0.044)
IV	-0.208***	-0.227***	-0.121	-0.051	-0.390***	-0.326***	-0.056
	(0.074)	(0.084)	(0.203)	(0.095)	(0.117)	(0.106)	(0.104)
Mean in CRs	70.71	70.26	71.24	71.14	69.93	70.2	71.21
Observations	310,440	173,686	136,754	194,109	116,331	155,074	155,366

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 dummies for birth year-month, dummies for region, 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. Reported means are for REBP-eligible birth cohorts in CRs.

Table 5: Robustness and placebo checks

	Men			Women
	Full sample	Access to DI pathway	Access to OA pathway	
A. Hazard rate (reduced-form)				
Coefficient	0.035**	0.039**	0.027	-0.023
	(0.015)	(0.020)	(0.020)	(0.035)
Observations	310,440	190,802	222,628	144,532
B. All regions				
Years early retirement	0.454***	0.439***	0.462***	0.705***
	(0.028)	(0.036)	(0.033)	(0.042)
Died by age 73	1.47**	1.71*	0.98	-0.17
(IV)	(0.72)	(0.92)	(0.90)	(0.57)
Age at death	-0.18**	-0.173*	-0.189**	-0.038
(IV)	(0.074)	(0.092)	(0.094)	(0.056)
Observations	356,417	219,168	256,461	162,263
C. < 15 employment years				
Years early retirement	0.024	0.147	-0.092	0.141
	(0.083)	(0.106)	(0.112)	(0.091)
Died by age 73	-0.19	0.39	-0.66	0.15
(reduced form)	(0.57)	(0.70)	(0.75)	(0.38)
Age at death	-0.006	-0.067	0.058	-0.034
(reduced form)	(0.066)	(0.080)	(0.089)	(0.039)
Observations	94,080	60,037	70,183	93,464

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, dummies for region, 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. Reported means are for REBP-eligible birth cohorts in CRs. Full sample consists of men born between January 1927 and December 1944 and women born between January 1932 and December 1944.

[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.

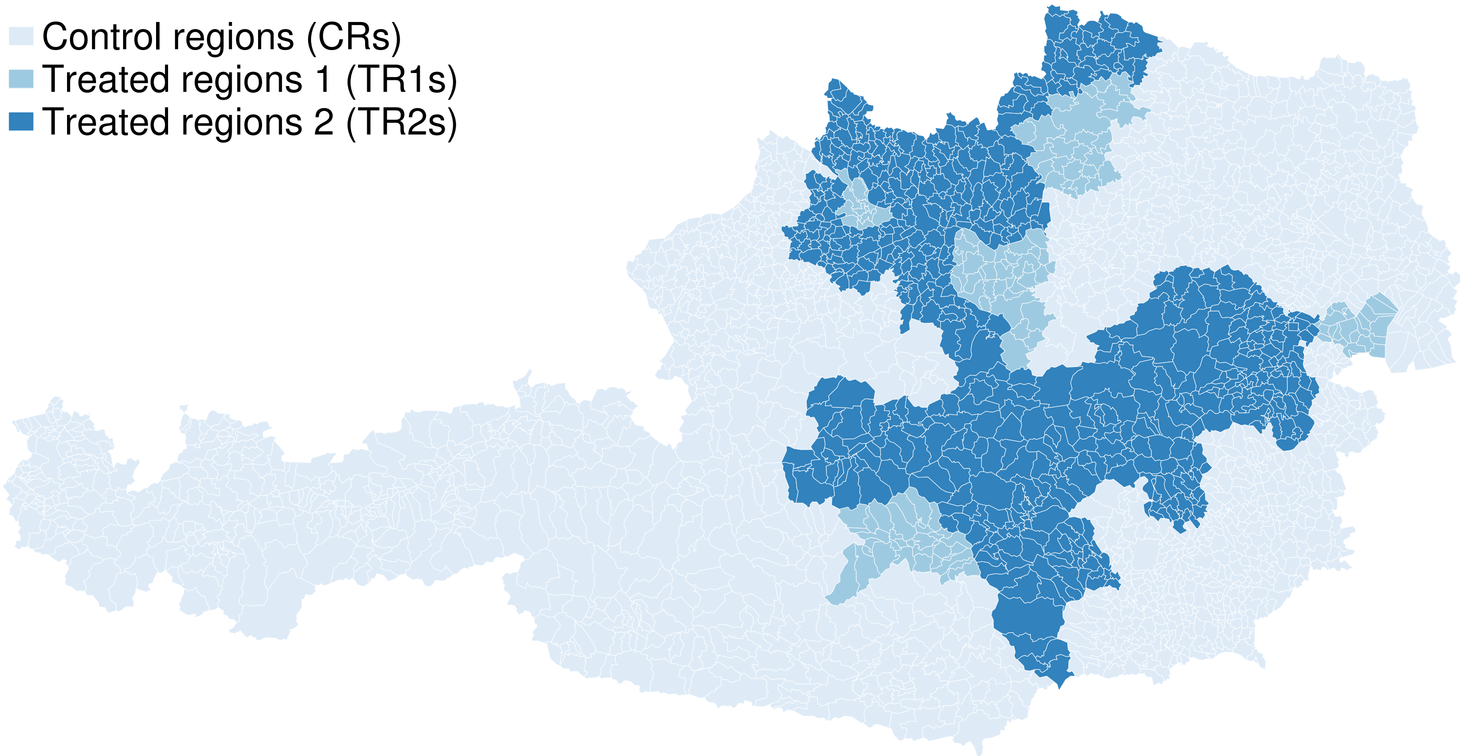
[b] Access to OA pathway: men born between January 1927-July 1938 and August 1943-December 1944. Treated birth cohorts reached age 55 during REBP and could also use the REBP to exit the labor force via the OA pathway.

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

	Earnings benefits (A)	UI (B)	DI benefits (C)	OA benefits (D)	SI benefits (E)	Total income (A+B+C+D+E)
A. Men						
Coefficient	-11,837***	4,860***	4,794***	334	-44	-1,893*
	(837)	(399)	(324)	(620)	(74)	(987)
Mean in CRs	239,610	22,084	21,175	199,654	4,983	487,506
Observations	310,440	310,440	310,440	310,440	310,440	310,440
B. Women						
Coefficient	-11,189***	8,959***	-237*	2,072**	-478***	-872
	(858)	(380)	(121)	(836)	(64)	(1,077)
Mean in CRs	124,821	13,059	1,959	23,2425	2,162	374,425
Observations	144,532	144,532	144,532	144,532	144,532	144,532

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, SI benefits received, DI benefits received, OA benefits received, and total income received between ages 50 and 73. All amounts are in 2016 euros. All regressions include dummies for birth year-month, dummies for region, 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. Reported means are for REBP-eligible birth cohorts in CRs. Sample consists of men born between January 1927 and December 1944 and women born between January 1932 and December 1944.

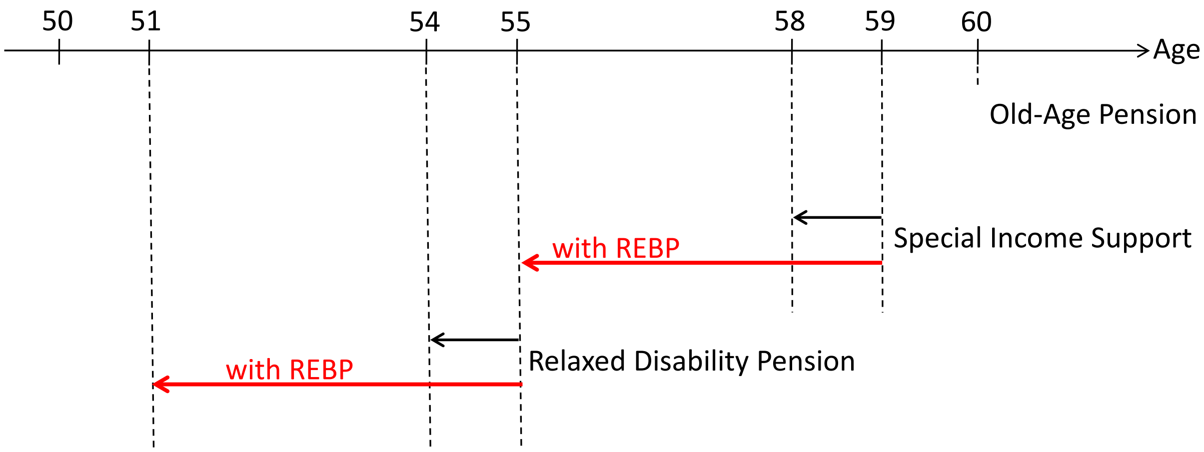
Figure 1: Distribution of REBP Treatment and Control Regions



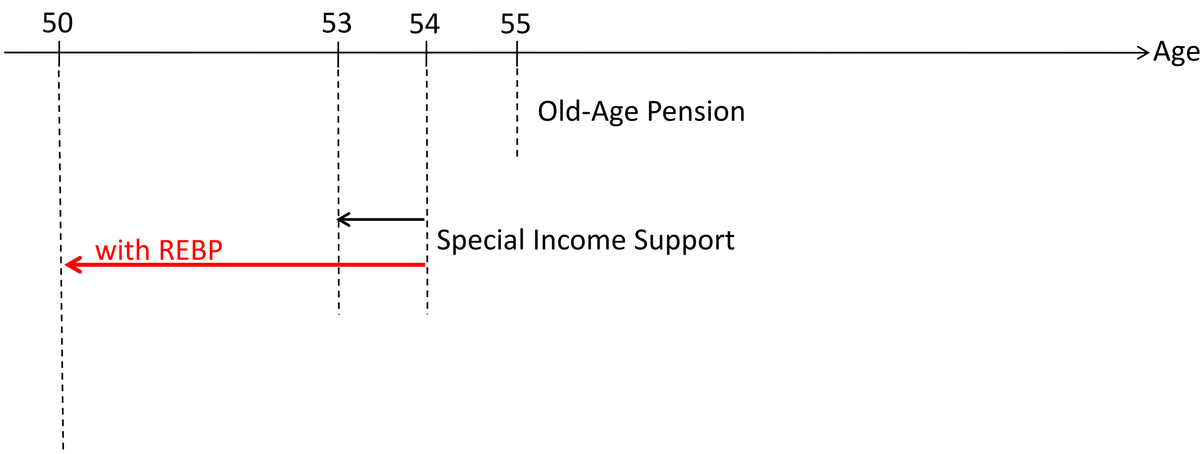
Notes: The figure shows the REBP and non-REBP regions. The REBP was introduced in June 1988 in TR1s and TR2s. The REBP was in effect until December 1991 (July 1993) in TR1s (TR2s).

Figure 2: Retirement Pathways with/without REBP Eligibility

(a) Men

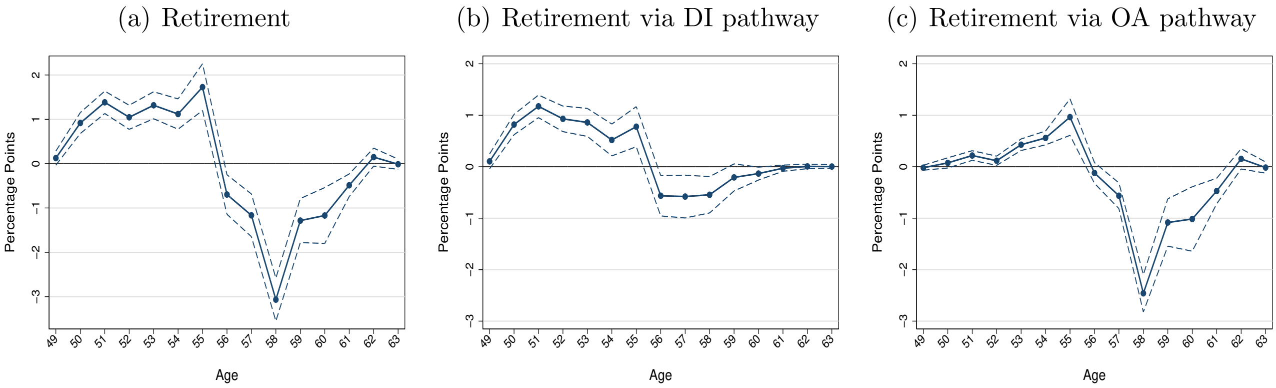


(b) Women



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). See text for details.

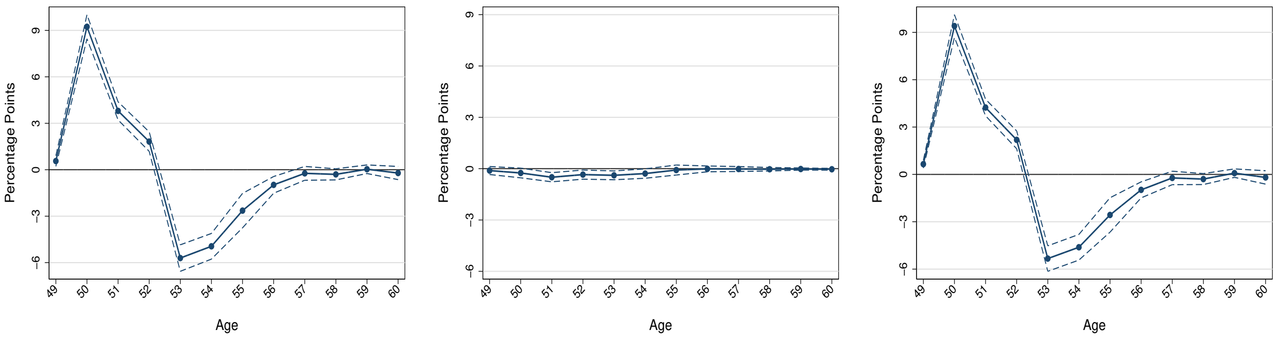
Figure 3: Effects on retirement at different ages by pathway, men



Notes: The figure plots the difference between TRs and CRs in the age-specific probability that men retire (panel a), retire via the DI pathway (panel b), and retire via the OA pathway (panel c), respectively. Dashed lines show 95% confidence bands.

Figure 4: Effects on retirement at different ages by pathway, women

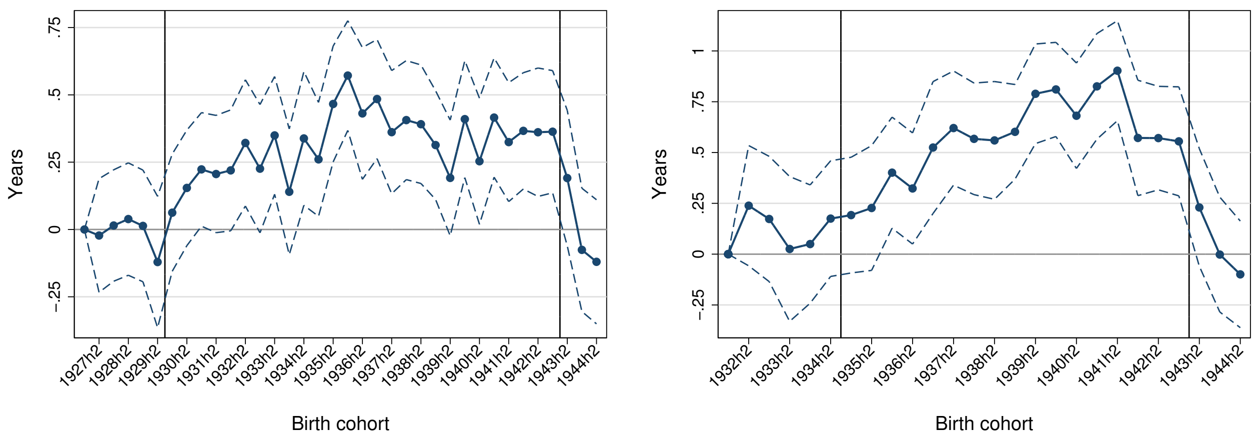
(a) Retirement (b) Retirement via DI pathway (c) Retirement via OA pathway



Notes: The figure plots the difference between TRs and CRs in the age-specific probability that women retire (panel a), retire via the DI pathway (panel b), and retire via the OA pathway (panel c), respectively. Dashed lines show 95% confidence bands.

Figure 5: Effects of REBP on years in early retirement

(a) Men (b) Women



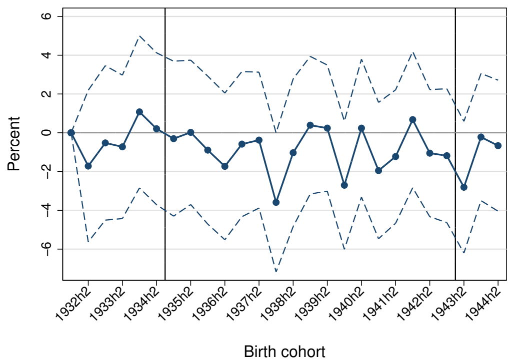
Notes: The figure plots the difference in the number of years spent in early retirement between TRs and CRs by half-year birth cohorts for men and women. The reference category are men and women born in 1927h1. Birth cohorts in between the two vertical lines could take advantage of the REBP. Dashed lines show 95% confidence bands.

Figure 6: Effects of REBP eligibility on different measures of mortality

A. Probability to die before age 73

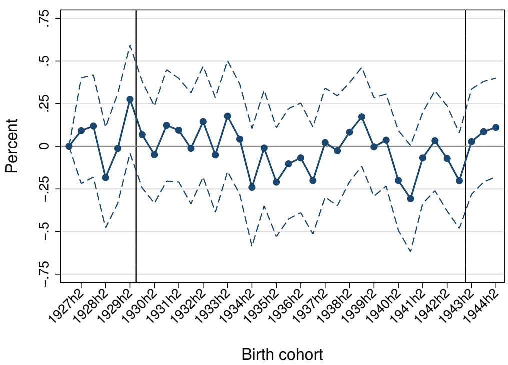
(a) Men

(b) Women

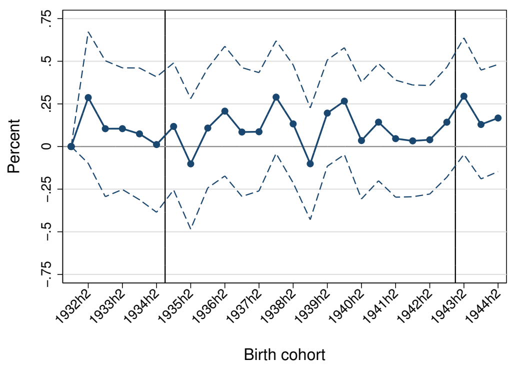


B. Age at death

(c) Men



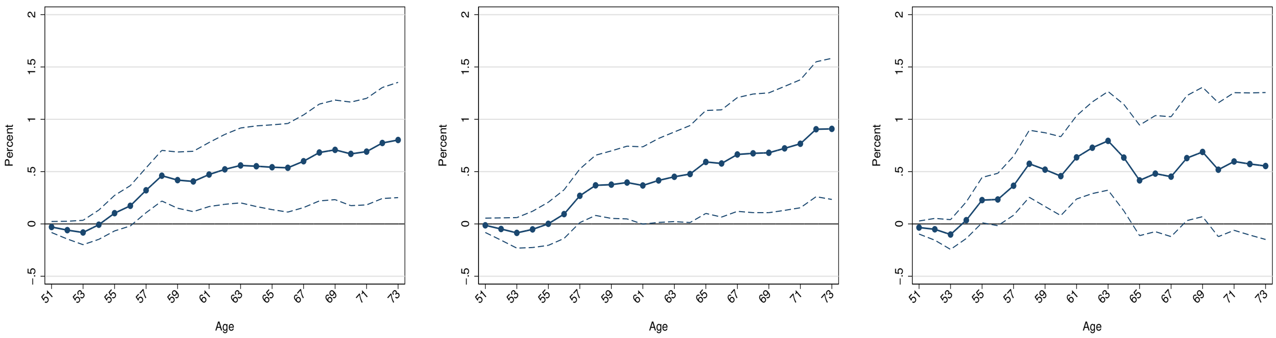
(d) Women



Notes: 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 half-year birth cohorts for men and women. The reference category are men and women born in 1927h1. Birth cohorts in between the two vertical lines could take advantage of the REBP. Dashed lines show 95% confidence bands.

Figure 7: Effects on cumulative mortality at different ages, men

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



Notes: The figure shows reduced form estimates (and corresponding 95% 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.