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

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Abstract

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 an exogenous change in unemployment insurance rules in Austria that allowed workers in eligible regions to withdraw permanently from employment up to 3.5 years earlier than workers in non-eligible regions. We find that the reduction in the retirement age increases mortality for men but not for women. The effect is statistically significant and quantitatively large for men in blue collar occupations, for men with less work experience or low earnings, and for men with pre-existing health impairments. For blue collar men, one additional year in early retirement increases the probability of death before age 73 by 2.4 percentage points (or 8.8 percent) and reduces the overall life span by 0.2 years. For women,

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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 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. This unique policy change allowed older workers in eligible regions to withdraw up to 3.5 years earlier from employment than comparable workers in non-eligible regions. Exploiting regional differences in eligibility for extended UI benefits of otherwise comparable workers 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 and instrumental variable (IV) estimation strategy.

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 life span for men in blue collar occupations, men with less work experience or low earnings, and men who have some pre-existing health impairment. For these subgroups of the population the effect of early retirement on mortality is not only statistically significant but also quantitatively important. For example, among blue collar men we find that one additional year in early retirement increases the likelihood of dying before age 73 by 2.42 percentage points (equivalent to a relative increase of 8.8 percent) and reduces the overall life span by 0.2 years. The effects are even larger for men with a pre-existing health impairment. An additional year in early retirement increases their probability to die before age 73 by 3.69 percentage points and shortens their life span by 0.25 years. On the other hand, we find that for women early retirement is not associated with worse health outcomes, even within the same subgroups for whom we found a strong effect for men. 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.

Our findings that early retirement increases mortality and reduces the life span among certain subgroups of men is robust to a variety of placebo and other specification checks. First, we show that individuals in eligible and non-eligible districts are similar in observed characteristics. Second, we examine whether mortality and early retirement trends differ across eligible and non-eligible regions prior to the extension of UI benefits and find no evidence for any significant differences. Third, we repeat our analysis for a sample of men and women who are not eligible for the benefit extension because they have not contributed enough years to the UI system. For these samples the IV estimates are always insignificant, while the OLS estimates are quantitatively large and highly significant. 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. As consequence, the change in lifetime income associated with early retirement is negligible and cannot explain the increased mortality among certain groups of the population.

We argue that the differential mortality patterns between blue and while collar men on the one side and women and men on the other side are consistent with the view that the interaction of early retirement and health-related behaviours as driver of mortality. For male blue collars, work means physical exercise and workers may not compensate this exercise with other physical activities after withdrawing from work. This may result in health problems, which may be further enhanced by adverse health behaviours (smoking, drinking) and/or by pre-existing health problems. These problems are less prevalent among women to the extent that they engage less in adverse health-related behaviours and they reduce less their physical exercise (e.g. due to housework activities).

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 and arguably exogenous shift in the earliest age at which individuals can permanently leave the labor force. We find that on average the group of eligible individuals indeed retires between 7 and 10 months earlier than the group of non-eligible individuals. Moreover, the policy change took place in the late 80s, allowing us to follow individuals for up to 30 years after the change. Second, we use administrative data containing precise and reliable information on both the timing of retirement and the date of death. Austrian social security data are collected for the purpose of assessing individuals' eligibility to (and level of) old age social security benefits. Information on any individual's work history and the date of death is thus precise, and our estimates are therefore unlikely contaminated by measurement error. This is different from many previous studies which focused on subjective measures of health or well-being that are subject to non-negligible measurement problems.[1] Third, our data contains the universe of workers in the private sector in Austria. Hence there is a sufficiently large number of observations that help us to get reasonably precise estimates.

Given the serious difficulties in estimating the causal effect of early retirement on mortality, and given that there may exist countervailing effects from early retirement on health, it is perhaps not surprising that previous empirical studies have found conflicting evidence. Moreover, focusing on those papers that take the endogeneity issue seriously and that, at the same time, use mortality as the main outcome measure leaves us with a handful of studies only, all of them using large administrative data sets (though some of them focus on subsamples of the working population only). In their study based on register data from Norway, Hernaes *et al.* (2013) also use instrumental variable methods to estimate the effect of early retirement on mortality (more specifically, their dependent variable is an indicator variable for death before age 77). They focus on institutional changes in the access to early retirement to instrument for actual retirement age. They find negative OLS estimates, but no causal effect from early retirement

[1] 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 (e.g. Coe and Zamarro, 2011; Johnston and Lee, 2009) while the evidence is less consistent for objective health measures. 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.

on mortality once potentially endogenous retirement decisions are taken into account (though some of their additional estimates appear to be pretty large, while being imprecisely estimated).

A similar approach is used by Bloemen *et al.* (2017), who focus on Dutch civil servants working for the central government in their analysis and instrument individuals' actual retirement age using targeted retirement. In contrast to Hernaes *et al.* (2013), they find that early retirement leads to an improvement in health, measured by the risk of dying within the five years following the date of of retirement. Hallberg *et al.* (2015) also use targeted early retirement to estimate the effect of retirement on both mortality and inpatient care, but they use discrete-time duration models instead of instrumental variable methods. Moreover, they focus on a relatively specific subsample of the working population, namely Swedish army employees aged 55 or older. They find that early retirement had a beneficial effect on retirees' health, using either outcome measure. Using U.S. population data, a recent study by Fitzpatrick and Moore (2018) examines whether there is a change in aggregate mortality at age 62, the earliest eligibility age for social security benefits. They find a 1.5% increase in aggregate mortality in the month individuals turn 62. A further related paper Black *et al.* (2017) estimate the effect of Disability Insurance (DI) benefit receipt on US 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.[2]

The remainder of this paper is structured as follows. In the following 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 sections 5 and 6. Section 7 concludes.

[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.

2 Institutional Background

2.1 Retirement Pathways in Austria

Since our study focuses on workers retiring during the last 1980s and early 1990s, we start with a description of the Austrian pension and unemployment insurance (UI) system during that time period.[3] We define retirement as the date at which an individual withdraws permanently from the labor market (see section 3.2 below for details).

The Austrian public pension system covers almost all workers and provides old-age and disability pensions, which are the main source of income in retirement. The formula for computing the level of benefits 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 is average indexed capped earnings over the best 15 years.[4] 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.

The statutory retirement age is 65 for men and 60 for women, but workers with sufficient insurance years may claim an old-age pension at any age after 60 for men and 55 for women.[5] 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 claim "special income support," provided that they have contributed to the UI program for at least 15 out of the previous 25 years.[6] 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

[3]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.

[4]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).

[5]Generally, a worker must have accumulated at least 35 insurance years to claim a pension before the statutory retirement age; the insurance years requirement is only 25 years for workers who have drawn UI benefits for the past year.

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

income support.

A third retirement pathway is via the disability insurance (DI) program, which grants relaxed access to a disability 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.[7].

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. To protect older workers against adverse labor market conditions in the steel industry, the Austrian government enacted a change in the unemployment insurance law that granted access to unemployment benefits for up to 209 weeks for a subgroup of workers.[8] To become eligible, a worker had to fulfill each of the following criteria at the time of unemployment entry: (i) age 50 or older, (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 eligible regions.

The program was implemented in June 1988 in 28 out of about 100 labor market districts. 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 quite 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 January 1992, a reform became effective which abolished the benefit extension for new UI claims in 6 of the 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.

Figure 1 illustrates that the introduction of the REBP significantly increased the incentive

[7]In 1996 (2000), the age at which disability screening was raised to 57 for men (women) (see Staubli (2011))

[8]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).

to exit the labor force via the UI system. Due to the REBP, men (women) could retire already at age 55 (50) by claiming UI benefits for 4 years, followed by one year of special income support. In contrast, male and female workers not eligible for the REBP could only retire at age 58 and 53, respectively. Figure 1 also suggest that the REBP 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 a DI pension at age 55. With the introduction of the REBP, this option was already available at age 51.

Figure 1

3 Data and Sample

3.1 Data Source and Sample Selection

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 2016. 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 2016, as well as all of the 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 district where they live.[9]

[9]We can check the extent of measurement error introduced by this proxy because we can observe the place of

Selection of Workers. Our main sample consists of men born in July 1930 to July 1943 and women born in July 1935 to July 1943. This ensures that individuals eventually turn age 50 during the REBP and were younger than age 58 (men) or age 53 (women) when the REBP was introduced. These cohorts benefitted from the REBP, albeit to a varying extent. For example, men born between July 1933 and July 1943 could take full advantage of four years of UI benefits to permanently withdraw from the labor force because they were age 55 or younger when the REBP was introduced. In contrast, men born before July 1933 were too old to take full advantage (i.e. they were older than age 55 when the REBP started).

We drop from the steel sector because the REBP did not induce changes in the retirement age for these workers. 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 age and place of residence. We therefore do not find any difference in the retirement age between steel-workers in eligible and non-eligible regions.

We focus on workers who meet the experience criterion of the REBP (i.e. worker with at least 15 employment years in last 25 years), but we use the sample of 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. Because all selected individuals meet both the age and the experience criteria, the assessment of whether or not a worker is eligible for the REBP hinges entirely on individuals' region.

Selection of Regions. Our analysis contracts eligible and non-eligible districts that are adjacent to each other. More specifically, we use the common classification of territorial units for statistics (NUTS, for short). NUTS comes in three aggregation levels, of which we choose the most disaggregated one, NUTS-3.[10] We choose the eight NUTS-3 regions that contain both eligible and non-eligible districts. Since NUTS-3 regions comprise geographically adjacent districts and because these units are small, this procedure implies that regional differences in

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.

[10]NUTS-3 units are defined in terms of the existing administrative units in the EU member states. An administrative unit corresponds to a geographical area for which an administrative authority has power to take administrative or policy decisions in accordance with the legal and institutional framework of the member state. There are 35 distinct NUTS-3 units in Austria, each consisting of one or more district(s).

access to health care and labor market conditions are unlikely to contaminate our estimates.[11] However, we also test the robustness of our results by contrasting all eligible and non-eligible regions.

3.2 Key Variables and Descriptive Statistics

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 2016, cohorts in our sample (men born in 1930-1943 and women born in 1935-1943) 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 age 50 and age 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 25.4 percent for men and 11.1 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 individuals 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.[12] Hence, a positive number on the treatment variable implies that an individual has retired before the statutory retirement age. A second measure for early retirement we use in the analysis is the incidence of early retirement, an indicator whether an individual exits the labor force before age 58 for men and age 53 for women. Without the REBP

[11] 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.

[12] This definition implies that an individual does not have to be retired in the legal sense of drawing an old age 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. In section xxxx, we examine the effect of the REBP on government transfers and income.

age 58 (53) is the earliest age men (women) can exit the labor force through the UI system, while during the REBP men and women could exit the labor force through the UI system before these ages (see Figure 1). In our analysis we stratify the sample by gender because early retirement rules and mortality patterns differ for men and women.

Table 1

Our final sample consists of 36,147 men and 10,555 women. Table 1 present summary statistics by gender and by eligibility status of the district. As shown in Panel A, the incidence of early retirement and the age at retirement are markedly lower in eligible districts compared to non-eligible districts. This pattern is consistent with the REBP inducing individuals to retire early. The probability to die before age 73 is also somewhat higher in eligible districts, while the age at death is the same across districts.

Panel B shows the background characteristics of individuals in eligible and non-eligible districts. These characteristics are measured at age 49 or before when individuals are not yet eligible for the REBP. Individuals in eligible districts tend to work more in blue-collar occupations and manufacturing, and less in construction and wholesale trade, but overall the differences in background characteristics are small. We empirically test for differences in background characteristics in the next section, which describes our empirical approach.

4 Empirical Strategy

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

$$y_i = \alpha + \mathbf{X_i}\theta + \beta ER_i + \epsilon_i, \tag{1}$$

where y_i is an indicator for death before age 73 (or the age at death), $\mathbf{X_i}$ denotes observed characteristics (e.g., birth year-month, NUTS region, previous earnings) that may influence mortality, ER_i denotes the years spent in early retirement, i.e. the difference between statutory age and the date of permanent exit from work, and ϵ_i is an error term.

In observational data, inference on the parameter of interest β is hampered if unobserved characteristics, such as health shocks, affect both mortality and early retirement. If $\beta > 0$ and if unobserved health shocks are positively correlated with early retirement, OLS overestimates the magnitude of the coefficient on early retirement.[13] 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, regional differences in REBP eligibility give rise to exogenous variation in the number of years an individual spends in early retirement.

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. As discussed previously, REBP eligibility is a function of age, previous work experience, and location of residence. Since we focus on workers who satisfy the age and experience criterion, this assumption boils down to whether the region of residence is correlated with unobserved characteristics such as health shocks.

One concern with this assumption is that workers may move from non-eligible to eligible districts in order to become eligible for the program. This is unlikely to be the case because eligibility rules require residence in a treated district for at least 6 months prior to claiming UI benefits. Moreover, mobility is rather 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 districts within states or across states within the last 5 years. A second, potentially more serious, problem may arise if a worker's location of residence has per se an effect on individuals' mortality risk. As previously discussed, to mitigate this concern, we focus on NUTS-3 regions that geographically adjacent eligible and non-eligible districts.

We test the validity of this assumption in two ways: First, we test whether REBP eligibility is correlated with observed individual characteristics. A failure of this test does not invalidate our estimation strategy, because independence only needs to hold conditional on observed characteristics. However, it seems plausible that strong differences in observed characteristics also imply differences in unobserved characteristics. Second, we examine early retirement and mortality trends for cohorts that had already reached the retirement age when the REBP started. If the independence assumption holds, then we should not find significant differences in early retirement or mortality between eligible and non-eligible districts.

While independence of REBP eligibility of unobserved characteristics is sufficient for a causal

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

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. This assumption is uncontroversial, as the REBP has a strong impact on early retirement behavior. Second, the monotonicity assumption requires that REBP eligibility would not result in individuals retiring later. One testable implication of the monotonicity assumption is that the first state estimates should be non-negative for all subsamples. Indeed, as we show below, first stage estimates for different subgroups of individuals based on their characteristics are consistently positive and sizeable, in line with the monotonicity assumption. The third assumption requires 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, as it is difficult to imagine that the mere eligibility for extended UI benefits should have a direct effect on mortality.

Under these assumptions, we can estimate the following first-stage equation:

$$ER_i = \mu + \mathbf{X_i}\lambda + \gamma Z_i + \nu_i, \qquad (2)$$

where ER_i corresponds to the number of years spent in early retirement, $\mathbf{X_i}$ denotes observed characteristics, Z_i measures whether an individual was eligible for the REBP to some degree, and ν_i is an error term.

Since not all birth cohorts could take advantage of the REBP to the same extent, we allow Z_i to vary by birth cohort. More specifically, Z_i denotes the years of additional UI benefits an individuals could draw due to the REBP, divided by the maximum years of additional UI benefits due to the REBP (3 years). For example, a male worker who is age 57 at the start of the REBP could draw one additional year of UI benefits until reaching age 58 and is therefore assigned a $Z_i = 1/3$. In contrast, men who were age 55 or younger at the start of the REBP could take full advantage of the REBP and are assigned a $Z_i = 1$. 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.

4.1 Assessing Instrument Validity

Before turning to our main estimates, we discuss our key identifying assumption that a worker's location of residence is exogenous with respect to unobserved characteristics such as the latent health status. We provide two pieces of evidence supporting the validity of our instrument.

First, Table 2 shows that differences in observed characteristics of individuals in eligible and non-eligible districts are small, consistent with the idea that REBP eligibility is almost as good as randomly assigned. All characteristics are measured before age 50 when individuals where not yet eligible for the REBP. To purge any differences across NUTS-regions or birth cohorts in the characteristics of individuals, we control always for NUTS and birth year-month fixed effects. Column 1 shows estimates of a linear probability model that tests whether observed characteristics of men in our sample are predictive for REBP eligibility. Men in eligible districts are more likely to work in blue-collar jobs and had slightly higher earnings in the past. There are also some difference with respect to the industry mix, but most coefficients are statistically insignificant and quantitatively small. We fail to reject the hypothesis that the coefficients on these 20 variables are jointly zero (p=0.168). Column 3 assess whether the same observed characteristics are predictive for REBP eligibility for women in our sample. Reassuringly, we also find only small differences.

Table 2

The second piece of evidence uses information on individuals who could not benefit from the REBP because they had already reached the eligibility age for an old-age pension (age 60 for men and age 55 for women) when the REBP started. For these individuals we should not find any significant differences in early retirement and mortality across districts if the identifying assumption holds.

Table 3 reports estimates of equation (2) with the incidence of early retirement, years in early retirement, incidence of death before age 73, and the age at death as dependent variables. Panel A considers the impact of residing in an eligible district on these four outcomes for men born in January 1922 to June 1928 who were too old for the REBP. Columns 1 and 2 shows no significant difference in the incidence of early retirement between eligible and ineligible districts, both with and without additional control variables. Columns 3 and 4 show that the years in early retirement is higher in eligible districts, but the point estimates are small, specially when compared to younger birth cohorts who were eligible for the REBP (Table below). Columns 5 to 8 further indicate that among these men there are no significant differences between eligible and non-eligible districts in the incidence of death before age 73 or the age at death.

Table 3

Panel B displays analogous estimates for women born between January 1927 and June 1933 who were at least 55 years old when the REBP was introduced in June 1988. We find no significant effects of residing in an eligible district on early retirement behavior or mortality. Taken together, we think that the evidence presented in Tables 2 and 3 provides strong support for the assumption that the selection of eligible labor-market districts was unrelated to underlying differences in early retirement or mortality in these districts.

5 Program Eligibility and Early Retirement

5.1 Estimates for the Full Sample

Table 4 presents first-stage estimates of equation (2). Panel A reports results for our main sample of men and women who have at least 15 employment years in the past 25 years, which was a necessary condition to be eligible for the REBP. Column 1 of Panel A reports a 7.88 percentage point increase in the incidence of early retirement among men in eligible districts. This represents represents a substantial 15 percent increase relative to the baseline in non-eligible districts. The second column shows that this estimate is robust when including additional variables capturing observed characteristics of individuals. Columns 3-4 of Panel A indicates the REBP eligibility increases the time spent in early retirement among men by 0.56 years or almost 7 months. Columns 4-8 of Panel A show that the analogous estimates are even larger for women. Being eligible for the REBP increases the incidence of early retirement among women by 18.5 percentage points and slightly less when additional controls are included. This translates into an increase in time spent in early retirement of 0.83 years, or about 10 months.

Panel B reports estimates for individuals who were born during the same years as individuals in Panel A, but who did not have enough employment years to qualify for the REBP. Thus, for these individuals we should not find any significant differences in early retirement behavior between eligible and non-eligible districts. Indeed, all point estimates are small and almost always statistically insignificant. For women, years in early retirement is statistically significant at the 10%-level, but the point estimate is about 7 times smaller relative to women with more than 15 employment years. These findings further support the idea that REBP eligibility drives the changes in early retirement behavior documented in Panel A.

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 86 or higher for all the estimates reported in Panel A, well above the threshold value of 10 above which 2SLS is not supposed to be subject to a weak instruments critique (Staiger and Stock, 1997). On the other hand, the F-statistic in Panel B is well below 10 for all specifications.

Table 4

To further explore the impact of the REBP on early retirement, we estimate equation (2) for each age in the interval 49-65 for men and 49-60 for women separately. We define three dependent variables to better understand 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 individuals retires at a given age by eventually claiming a DI pension (retirement with DI pension). The third is an indicator for whether an individuals retires at a given age by eventually claiming an old-age pension, without having claimed DI before (retirement with old-age pension).

Figure 2 illustrates the results for men with dots on the solid line showing the coefficient estimates. The 95 percent confidence interval is shown by dashed lines. As shown in Panel (a), the probability to retire does not change for men younger than 50, but is positive and statistically significant for men between 50 and 55, consistent with the retirement incentives created by the REBP. On the other hand, coefficient estimates turn negative and significant between 56 and 61. This suggests that the additional retirements between 50 and 55 are driven by men who, in the absence of the REBP, would have retired between 56 and 61. After age 61, we do not see significant differences between eligible and non-eligible men.

Panel (b) shows that men retiring between ages 50-54 do so by eventually claiming a DI pension (most claim the DI pension at age 55 when eligibility rules are relaxed). Retirement with DI pensions is lower between ages 56-59, suggesting that some eligible men substituted from a DI pension to an old-age pension or claimed their DI pension at an earlier age. Consistent with this idea, Panel (c) shows a significant spike at age 55 in the fraction of eligible men who retire with an old-age pension.

Figure 2

Figure 3 shows the corresponding estimates for women. As Panel (a) illustrates, the probability to retire in eligible and non-eligible districts is identical at age 49, but displays a large positive spike at age 50 in eligible districts. Age 50 is the earliest age women can retire through the UI system during the REBP (Figure 1). The probability to retire is also higher at age 51 for women in eligible districts. In contrast, the incidence of retirement in eligible districts is significantly lower in the age group 53-56 and becomes insignificant in the age group 57-60.

Panel (b) shows that the REBP lead to a reduction in retirements with a DI pension among eligible women in the age group 51-53. These women now use the extended UI benefits to bridge the time until age 55 when they become eligible for a regular old-age pension. However, this substitution effect from DI to old-age pension is quantitatively quit 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 then claim an old-age pension at age 55.

Figure 2

5.2 Heterogeneity in the Effect of the REBP on Early Retirement

We next examine whether the size of the estimated early retirement responses varies across birth cohorts. More specifically, we generalize equation (2) by replacing Z with a set of birth cohort times REBP eligibility interaction terms. Figure 4 plots the estimated coefficients of the interaction terms with the incidence of early retirement and the years in early retirement as dependent variables. Panel (a) shows that the coefficients for the incidence of early retirement are significantly higher for all birth cohorts of men in eligible districts. The estimates are higher for older birth cohorts and somewhat lower for younger birth cohorts. This pattern is consistent with the institutional setting: older birth cohorts could use the REBP to exit the labor market via a regular old-age pension, while younger birth cohorts needed to rely on a disability pension which is more difficult to get.

Panel (b) shows that men born in 1930h2 spent about 0.25 years more in early retirement in eligible relative to non-eligible districts. These cohorts were already close to 58 years old when the REBP was implemented and hence the REBP did not have a sizable impact on the date of permanent exit from the work force. Coefficient estimates become larger for younger birth cohorts (between 0.5-0.75 years) who could take full advantage of the REBP. The bottom panels of Figure 4 show that the effects are even more pronounced for women. The incidence of early retirement increase by as much as 20 percentage points for some birth cohorts (Panel c). The time women in eligible districts spend in early retirement increases by about 0.75 years for younger cohorts and about 0.25 years for older cohorts (Panel d).

Figure 4

There is no reason to expect that the effect of the REBP on early retirement is the same among subgroups of individuals, defined by observed characteristics other than birth date. In Table 5 we present first-stage estimates for groups defined by occupation (blue-collar vs white-collar), time spent on sick leave, work experience, earnings, and industry (manufacturing vs non-manufacturing). The coefficient on the years spent in early retirement is significantly positive for all groups, and its magnitude varies substantially across groups. The observed differences across subgroups show a similar pattern for men and women. For example, eligible men (women) in blue collar occupations spend an additional 0.66 (0.86) years in early retirement, while the estimate is only 0.28 (0.65) for eligible men (women) in white collar occupations.

Our instrumental variables estimation strategy also offers an opportunity to draw a probabilistic inference about the characteristics of the compliers of the REBP eligibility instrument. The compliers are those individuals who change their retirement behavior because of the REBP. More specifically, the relative likelihood that the complier has a particular observable characteristic is given by the ratio of the first-stage coefficient conditional on that characteristic relative to the overall first-stage coefficient.[14] Columns 3 and 6 present the estimated relative complier likelihoods for each group. We see that complier men (women) are 14 (19) percent more likely to work in blue-collar occupations, 19 (33) percent more likely to have spent time on sick leave before age 50, and 60 (46) percent more likely have worked in the manufacturing sector. Indi-

[14] To derive the relative complier likelihood, we need to express early retirement as a binary variable (Angrist and Pischke, 2009). We therefore use the first-stage estimates with the incidence of early retirement as the dependent variable, because this variable is binary.

viduals with previous earnings (or experience) below and above the median are almost equally responsive to REBP eligibility.

Table 5

6 The Effect of Early Retirement on Mortality

6.1 Estimates for the Full Sample

In this section we present our main estimates of the causal effect of early retirement on mortality. Table reports estimates for the equation (1) estimated by OLS and IV (2SLS) as well reduced-form effects of REBP eligibility on the dependent variable. We examine two mortality outcomes: the incidence of death before age 73, measured in percent, and the age at death. The OLS estimates in Panel A suggest that among men an additional year in early retirement is associated with a 3.38 percentage point increase in the probability to die before age 73. This corresponds to a relative increase in the risk of premature death of about 13.3% (=3.38/25.4). Similarly, we find that an additional year in early retirement reduced the life span by 0.5 years. The corresponding OLS estimates for women are about three and a half 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 and life longer than men.

In contrast, our IV estimates are statistically insignificant and much smaller than the OLS estimates, but quantitatively still sizeable. For example, among men the IV estimate implies that an additional year in early retirement increases the probability to die before age 73 by 1.53 percentage points. One potential explanation why the IV estimates are insignificant despite being large in magnitude in some cases is a lack of power. Since not all birth cohorts were treated in the same way by the REBP, one way to increase the statistical power is to interact the REBP eligibility indicator with the year-month of birth. This approach leaves us with 157 instruments for men (birth cohorts 1930/7-1943/7) and 97 instruments for women (birth cohorts 1935/7-1943/7). This over-identified case lends itself to a simple graphical representation, which may be understood as a plot of the reduced-form against the first-stage estimates.

Table 6

Specifically, Panel (a) of Figure 5 shows the relationship between the probability of dying before age 73 and the years spent in early retirement. The dots are average residuals by birth year-month and eligibility status obtained from a regression of the incidence of death before age 73 and the years in early retirement on birth year-month fixed effects, NUTS fixed effects, and additional controls as in the main specification. The slope is from a regression of the residuals of the probability of death before age 73 on the residuals of years spent in early retirement, weighted by the size of the cells spanned by birth year-month and REBP eligibility. It directly corresponds to the 2SLS-estimate of the effect of early retirement on the incidence of death before age 73 (Angrist, 1990). The figure clearly shows that among men there is a positive causal relation between the number of early retirement years and the probability of premature death (before age 73). The slope is equal to 1.646, close to point estimate in the exactly identified case (1.530), and statistically significant at the 1%-level.

Panel (b) shows the relationship between the age at death and the years spent in early retirement among men. In this case, the slope is equal to -0.18, with a standard error of 0.06, suggesting that an additional year in early retirement reduces the life span by 0.18 years. Panel (c) and (d) show the analogous relationships between our measures of mortality and early retirement for women. The slope estimates are about four times smaller then for men and statistically insignificant.

Figure 5

6.2 Heterogeneity in the Mortality Effects

The results for the full sample point into the direction that early retirement is likely associated with worse health outcomes. However, it is plausible that the effects vary considerably across subgroups of the population In a next step, we explore whether the effect of early retirement on mortality varies with observable characteristics. In Table 7 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 (Table 5). The OLS estimates are always positive and statistically significant, and there is relatively little variation across subgroups. For example, among men the OLS point estimate varies from 2.87 percentage points for white collar men to 3.74 percentage points for men with previous earnings below the median. The OLS estimates are 3-4 times smaller for women, but also show little variation across subgroups (from 0.71 for women in manufacturing to 1.16 for blue collar women).

The IV estimates are consistently smaller than the OLS estimates and there are striking differences among certain subgroups. For blue collar men we find that an additional year in early retirement increases the probability to die before age 73 by 2.42 percentage points. This represents a 8.8 percent increase relative to the baseline probability to die before age 73 (27.4%). The reduced-form effect is also statistically significant, but it is smaller in magnitude than the IV (1.59) because it is not scaled up by the first-stage estimate. On the other hand, the IV estimate for white collar men is negative, but it is also imprecisely estimated because the first-stage coefficient is small for this group. This explains why the IV estimate is quantitatively large even though the reduced-form estimate is small.

We also find striking differences when we split the sample depending on whether the number of days spent on sick leave before age 50 was positive. An additional year in early retirement increases their probability to die before 73 by 3.69 percentage points for men with positive sick leave days, while the IV estimate is close to 0 for men who have not spent any time on sick leave. This difference is also reflected in the corresponding reduced-form estimates.

We also test for heterogenous mortality effects by prior employment and earnings history. Our IV estimate imply that an additional year of early retirement causes a 3.53 percentage point increase in the incidence of death before age 73 for men with low work experience, but has no effect on men with high previous work experience. Similarly, we find a statistically positive effect of early retirement on mortality for men with low-earnings and no significant effect for high-earning men. Finally, the bottom two rows show that early retirement is associated with a higher mortality rates for men who have worked in the manufacturing sector. The IV estimate for workers in other sectors is also large but imprecisely estimated due to the small first stage estimate (as also reflected in the small reduced-form estimate).

In contrast, columns 5 and 6 of Table 7 shows that in the case of women the IV and reduced-form estimates are statistically insignificant for all subgroups.

Table 7

Table 8 reports analogous estimates of the effect of early retirement on the age at death for the same subgroups. 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. For men they range from -0.42 to -0.57, while for women the estimates are smaller in absolute terms and range from -0.11 to -0.18.

The corresponding IV estimates are generally smaller and more heterogenous than the OLS estimates. The IV estimate imply that an additional year in early retirement reduces the life span by 0.20 years among blue collar men (statistically significant at the 10% level) and potentially increases the life span among white collar men (although we only find a positive effect for the reduced form).

We find an even larger negative effect for men who spent some time on sick leave in their 40s. Their life span is reduced by 0.35 years for an additional year in early retirement, while we find no effect for men who did not spend time on sick leave in the past. The next two panels show that men with little work experience and low earnings experience also experience a shorter life span of 0.33-0.34 years when retiring early. 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.

In contrast, we find no significant effects when we split the sample by industry (manufacturing vs non-manufacturing). The IV and reduced-form estimates are also never significant for women.

Table 8

6.3 Robustness

Our results suggests that early retirement has a negative health effect for certain subgroups of men (blue collar workers, unhealthy workers, workers with less work experience and low earnings) and no effect for other subgroups of men as well as women. In a next step, we explore the robustness of these results in two ways: (i) we perform the same heterogeneity analysis for men who lack employment years to be eligible for the REBP and (ii) we extend the analysis sample by including all regions in Austria and older birth cohorts who were not eligible for the REBP.

Table 9 shows coefficient estimates for men who have less than 15 employment years in the past 25 years and are not eligible for the REBP.[15] Specifically, the table reports estimate for the effect of the REBP on years in early retirement (first-stage) and our mortality outcomes (the reduced-form) as well as OLS estimates of the effect of early retirement on our mortality outcomes. The first column shows that the first-stage estimates are statistically insignificant and quantitatively small for all subgroups of men.[16]

As columns 2 of Table 9 shows, the OLS estimates of early retirement on the incidence of death before age 73 are always positive and statistically significant. In contrast, the reduced-form estimates (column 3) are statistically insignificant and either small or even have the opposite sign. These patterns are consistent with the idea that unobserved health of individuals who retire is worse compared to those who retire later. Columns 4 and 5 support this conclusion: retiring early is associated with a shorter life span, but residing in a REBP eligible district has no direct effect on the life span, even among subgroups of men for whom we find a large and significant IV effect in the eligible sample.

Table 9

Our main analysis focuses on NUTS regions that contained both eligible and non-eligible districts which are comparable in terms of labor market conditions and access to health care. A natural question to ask is whether our findings are robust when we extend the sample to include all regions in Austria. Table 10 reports estimates of equations (1) and (2) for men born 1927/7-1943/7 using all regions in Austria. This sampling restriction gives us variation in REBP eligibility within a NUTS region across birth cohorts, because men born before 1930/7 were too old to benefit from the REBP.

Column 1 of Table 10 shows that REBP eligibility significantly increases the number of years spent in early retirement among. The coefficient estimates tend to be smaller than compared to the estimates using the geographically restricted sample. For example, we find that REBP eligibility increased the time spent in early retirement by 0.4 years when using all regions compared to 0.56 years when using the restricted regions. Nevertheless, we find that the F-statistic is 50 or above in all cases, except for white collar men where the F-statistic is 14.4.

[15] We focus on men here because we did not find any significant effects among eligible women. However, we report estimates for women in Table A.1 in Appendix A. The conclusions are the same as for men.

[16] Since the first-stage is weak for all subgroups of non-eligible men, we do not report IV estimates in Table 9.

Column 4 and 5 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 shorter life span among blue collar men, men with positive sick leave days, and men with less work experience. The point estimates are somewhat smaller compared to the restricted sample, but economically still meaningful. For example, for blue collar men we find that an additional year in early retirement increases the incidence of death before age 73 by 2.02 percentage points, or 7.4% relative to the baseline, and reduces the life span by 0.23 years.

Table A.2 in Appendix A reports the analogous results for women born 1932/7-1943/7 using all regions in Austria. Consistent with the results in Table 7 and 8, we find that for women being eligible for the REBP leads to a large increase in the years spent in early retirement, but has no direct effect on the incidence of death before age 73 or the age at death.

Table 10

Another question of interest is what is the mechanism that is driving these results. In particular, exiting the labor force 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. Our data allow us to examine this question in detail because they contain information on whether individuals receive transfers from the unemployment insurance, sickness insurance, or the public pension program. Table 11 reports estimates of equation (2) using as dependent variables an individual's total earnings, total UI benefits, total SI benefits, total DI and OA benefits, all measured between ages 50-73. We also report estimates for the total income between ages 50-73, which is the sum of the different transfers and earnings. Panel A shows that among men REBP eligibility is associated with a loss in earnings of 14,834 euros, or about 7.7% relative to the baseline in non-eligible districts. However, it turns out that a large part of this earnings loss is compensated through benefits from the UI program. On average, men in eligible districts collect an additional 8,052 euros in UI benefits. Moreover, we also find a significant increase in the amount of DI benefits that people collect (4,333 euros) because they are more likely to exit the labor force at age 55 through the DI program. Summing all up all the transfers and earnings, we find that among REBP eligibility reduced the total income between ages 50-73 by 1,041 euros.

Panel B shows analogous estimates for women. We find that being eligible for the REBP reduces their earnings by -12,316 euros, but a large part of the earnings loss is covered by additional UI benefits (9,798). Interestingly, we find that eligible women collect less DI benefits and more old-age pension 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 the become eligible for an old-age pension. Summing up all the different income sources, we find that residing in a REBP eligible district did not affect women's total income. In sum, the estimates in Table 11 suggest that changes in income are likely cannot explain the increase in mortality associated with early retirement, even when scaled up by the first stage.

Table 11

7 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 up to 3.5 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 7 (10) months earlier than their non-eligible colleagues. This provides us with an empirical design that allows us to identify the causal impact of early retirement on mortality using instrumental variable techniques.

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 life span for men in blue collar occupations, men with less work experience or low earnings, and men who have some pre-existing health impairment. For these subgroups of the population the effect of early retirement on mortality is not only statistically significant but also quantitatively important. On the other hand, we find that for women early retirement is not associated with worse health outcomes, even within the same subgroups for whom we found a strong effect for men. 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.

Our findings that early retirement increases mortality and reduces the life span among certain subgroups of men is robust to a variety of placebo and other specification checks. We show that individuals in eligible and non-eligible districts are similar in observed characteristics and we also find no differences in morality and early retirement trends between eligible and non-eligible districts prior to the extension of UI benefits. We also repeat our analysis for a sample of men and women who are not eligible for the benefit extension because they have not contributed enough years to the UI system and find no 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 the health of certain groups of retirees. Flexible policies fostering employment of older workers can therefore generate a double dividend. Such policies would not only improve government budgets, they would also increase individuals' welfare by prolonging their lives. Labor market policies should therefore try to incentivize both firms to keep older workers in employment and workers to abstain from premature retirement.

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Table 1: Summary statistics

	Men		Women	
	Eligible districts	Non-eligible districts	Eligible districts	Non-eligible districts
A. Outcomes				
Incidence of early retirement (in %)	60.65	52.08	34.54	16.22
Retirement age (years)	56.72	57.33	53.93	54.80
Years in early retirement	8.28	7.67	6.07	5.20
Incidence of death before age 73 (in %)	26.59	25.98	11.42	11.21
Age at death (years)	70.80	70.80	72.09	72.08
B. Background characteristics				
Years tenure in last job at age 49	5.87	5.57	6.60	6.90
Years employed, ages 39-43	4.83	4.78	4.77	4.76
Years employed, ages 44-48	4.78	4.72	4.76	4.78
Years sick leave, ages 39-43	.041	.039	.019	.017
Years sick leave, ages 44-48	.029	.032	.028	.022
Years unemployed, ages 39-43	.094	.127	.108	.106
Years unemployed, ages 44-48	.162	.207	.153	.129
Last daily wage (euro)	88.0	84.2	64.1	64.0
Total earnings, ages 39-43	127,171	121,885	92,488	93,006
Total earnings, ages 44-48	147,508	139,036	104,995	105,151
Blue collar (in %)	72.1	70.9	54.5	50.2
Industry				
Agriculture (in %)	8.9	9.2	2.6	3.6
Utilities (in %)	2.6	2.0	0.9	0.6
Manufacturing (in %)	48.7	39.1	41.5	35.9
Construction (in %)	14.4	18.8	4.7	4.9
Wholesale trade (in %)	9.0	12.6	14.1	18.0
Accommodation and food services (in %)	0.7	1.5	4.4	6.3
Transportation (in %)	2.8	3.8	1.4	2.0
Finance and insurance (in %)	11.3	11.1	19.7	18.4
Health care and social assistance (in %)	1.2	1.5	9.8	9.4
Arts, entertainment, and recreation (in %)	0.5	0.6	0.7	0.8
NUTS-Region				
Nordburgenland (in %)	11.0	11.1	14.3	13.9
Mostviertel-Eisenwurzen (in %)	13.3	14.1	13.1	13.4
Waldviertel (in %)	18.5	13.6	26.3	16.9
Unterkaernten (in %)	7.1	11.4	5.4	9.1
Oststeiermark (in %)	8.9	14.7	6.6	14.5
West- und Suedsteiermark (in %)	8.8	9.9	5.8	9.1
Innviertel (in %)	6.4	19.0	7.1	17.9
Muehlviertel (in %)	26.0	6.1	21.3	5.3
Number of individuals	15,688	20,459	4,415	6,140

Notes: Sample consists of men born between July 1930 and July 1943 and women born between July 1935 and July 1943. There are 8 distinct NUTS-regions, 11 eligible districts, and 19 non-eligible districts. Last daily wage, blue collar, and industry are measured at age 49.

Table 2: Testing for differences between REBP and non-REBP regions before age 50

	Eligible district, men		Eligible district, women	
	coefficient	s.e.	coefficient	s.e.
Years tenure in last job	.0003	(.0012)	-.0010	(.0016)
Years employed, ages 39-43	-.0057	(.0100)	.0137	(.0098)
Years employed, ages 44-48	.0051	(.0107)	.0236*	(.0125)
Years sick leave, ages 39-43	.0125	(.0222)	.0205	(.0587)
Years sick leave, ages 44-48	-.0382	(.0257)	.0566	(.0528)
Years unemployed, ages 39-43	.0055	(.0160)	-.0051	(.0189)
Years unemployed, ages 44-48	.0019	(.0130)	.0639***	(.0185)
Last daily wage (euro)	.0003	(.0003)	.0006	(.0004)
Total earnings, ages 39-43 (in 1,000 euro)	.0004**	(.0002)	-.0002	(.0002)
Total earnings, ages 44-48 (in 1,000 euro)	.0001	(.0001)	-.0001	(.0003)
Blue collar	.0473***	(.0087)	.0177*	(.0102)
Agriculture	-.0036	(.0334)	-.0540	(.0485)
Utilities	.0626	(.0455)	.0930	(.0753)
Manufacturing	.0259	(.0301)	.0292	(.0495)
Construction	-.0457	(.0299)	.0114	(.0461)
Wholesale trade	-.0545*	(.0311)	-.0148	(.0441)
Accommodation and food services	-.0908***	(.0336)	-.0499	(.0448)
Transportation	-.0527	(.0328)	-.0402	(.0578)
Finance and insurance	.0224	(.0325)	.0398	(.0443)
Health care and social assistance	-.0219	(.0356)	.0373	(.0427)
F-statistic for joint significance	.23		.37	
p-value	(.629)		(.546)	
Obs.	36,147		10,555	
R^2	.168		.168	

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Sample consists of men born between July 1930 and July 1943 and women born between July 1935 and July 1943. Columns 1 and 3 display OLS estimates from separate regression of whether an individual resides in an eligible district on individual characteristics prior to age 50. All regressions include fixed effects for birth year-month and NUTS-region. Years tenure in last job, last daily wage, blue collar, and industry are measured at age 49.

Table 3: Placebo regressions for pre-REBP cohorts

	Incidence of early retirement (in %)		Years in early retirement		Death before age 73 (in %)		Age at death	
	Base controls	Full controls	Base controls	Full controls	Base controls	Full controls	Base controls	Full controls
A. Men								
Eligible district	-.74	-.86	.120*	.107*	.526	.601	-.022	-.028
	(1.44)	(1.37)	(.067)	(.063)	(.721)	(.718)	(.068)	(.070)
Mean of Dep. Var.	29.08	29.08	6.25	6.25	28.62	28.62	71.01	71.01
First stage F-stat.	.27	.39	3.18	2.87				
Obs.	12,266	12,266	12,266	12,266	12,266	12,266	12,266	12,266
R^2	.085	.138	.095	.187	.008	.019	.011	.022
B. Women								
Eligible district	-.922	-.723	.118	.115	.474	.349	-.018	.014
	(.842)	(.874)	(.095)	(.072)	(1.059)	(1.091)	(.104)	(.104)
Mean of Dep. Var.	10.31	10.31	4.54	4.54	14.68	14.68	71.87	71.87
First stage F-stat.	1.20	.68	1.55	2.59	.20	.10	.03	.02
Obs.	4,930	4,930	4,930	4,930	4,930	4,930	4,930	4,930
R^2	.029	.060	.073	.200	.018	.023	.018	.021

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Sample consists of men born between January 1922 and June 1928 and women born between January 1927 and June 1933. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively. Reported means are for non-eligible districts.

Table 4: First Stage: Effect of REBP one early retirement

	Men				Women			
	Incidence of early retirement (in %)		Years in early retirement		Incidence of early retirement (in %)		Years in early retirement	
	Base controls	Full controls	Base controls	Full controls	Base controls	Full controls	Base controls	Full controls
A. $\geq$ 15 employment yrs								
Eligible district	7.88***	7.64***	.559***	.556***	18.52***	17.86***	.831***	.782***
	(.84)	(.72)	(.057)	(.049)	(1.99)	(1.60)	(.111)	(.080)
Mean of Dep. Var.	52.3	52.3	7.7	7.7	16.2	16.2	5.2	5.2
First stage F-stat.	88.07	112.20	96.98	131.29	86.43	125.12	56.05	94.75
Obs.	36,147	36,147	36,147	36,147	10,555	10,555	10,555	10,555
R^2	.065	.159	.059	.191	.070	.141	.055	.175
B. < 15 employment yrs								
Eligible district	-.15	.12	-.099	-.101	.53	-.03	.119*	.117*
	(.89)	(.85)	(.066)	(.064)	(.62)	(.57)	(.068)	(.063)
Mean of Dep. Var.	66.0	66.0	8.3	8.3	12.0	12.0	4.1	4.1
First stage F-stat.	.03	.02	2.26	2.55	.73	.00	3.12	3.47
Obs.	16,235	16,235	16,235	16,235	16,520	16,520	16,520	16,520
R^2	.066	.139	.060	.190	.013	.077	.013	.159

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Sample consists of men born between July 1930 and July 1943 and women born between July 1935 and July 1943. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively. Reported means are for non-eligible districts.

Table 5: Heterogeneity: First-stage regressions for men and women

	Men				Women			
	Years of early retirement	F-stat	Relative complier likelihood	Obs.	Years of early retirement	F-stat	Relative complier likelihood	Obs.
White collar	0.284*** (0.076)	14.1	0.63	10,343	0.654*** (0.093)	49.5	0.77	5,071
Blue collar	0.656*** (0.050)	170.0	1.14	25,804	0.858*** (0.093)	84.5	1.19	5,484
Past sick leave=0	0.442*** (0.057)	60.8	0.88	21,261	0.731*** (0.082)	80.1	0.90	7,737
Past sick leave>0	0.729*** (0.070)	108.7	1.19	14,886	0.962*** (0.140)	47.0	1.33	2,818
Experience<median	0.571*** (0.063)	81.6	0.91	17,978	0.833*** (0.107)	61.2	1.11	5,268
Experience≥median	0.524*** (0.064)	67.0	1.04	18,169	0.730*** (0.093)	61.3	0.90	5,287
Earnings<median	0.436*** (0.052)	69.4	0.91	18,073	0.820*** (0.103)	63.3	1.06	5,277
Earnings≥median	0.680*** (0.071)	92.0	1.13	18,074	0.750*** (0.089)	70.6	0.93	5,278
Manufacturing	0.915*** (0.069)	173.4	1.61	15,632	1.112*** (0.111)	100.7	1.46	4,037
Non-manufacturing	0.291*** (0.056)	26.7	0.55	20,515	0.513*** (0.086)	35.5	0.63	6,518

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. Relative likelihood is the ratio of the first-stage coefficient on the incidence of early retirement for a subgroup (not reported) relative to the overall first-stage coefficient (columns 2 and 6 in Table 4). Blue/white collar status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Table 6: Effects of early retirement on mortality

	Men				Women			
	OLS	IV	Reduced form	Mean	OLS	IV	Reduced form	Mean
A. ≥ 15 employment yrs								
Death before age 73	3.38***	1.53	0.85	25.4	0.92***	0.25	0.19	11.1
	(0.09)	(1.01)	(0.57)		(0.14)	(0.86)	(0.68)	
Age at death	-0.50***	-0.06	-0.03	70.8	-0.15***	0.02	0.01	72.1
	(0.01)	(0.1)	(0.06)		(0.01)	(0.08)	(0.07)	
B. < 15 employment yrs								
Death before age 73	1.92***		-0.64	26.4	0.87***		-0.41	10.0
	(0.11)		(0.88)		(0.08)		(0.57)	
Age at death	-0.31***		0.06	70.7	-0.09***		0.05	72.3
	(0.02)		(0.88)		(0.01)		(0.05)	

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, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively. We do not report IV estimates for individuals with <15 employment years due to the lack of a first stage (see Table 4)

Table 7: Heterogeneity: Effect of early retirement on the incidence of death before age 73

	Men				Women			
	OLS	IV	Reduced form	Mean	OLS	IV	Reduced form	Mean
White collar	2.87***	-2.78	-0.79	20.3	0.77***	-0.81	-0.53	10.8
	(0.15)	(3.63)	(0.99)		(0.21)	(1.58)	(1.03)	
Blue collar	3.69***	2.42**	1.59**	27.4	1.16***	1.07	0.92	11.3
	(0.11)	(1.05)	(0.71)		(0.23)	(1.17)	(1.01)	
Past sick leave=0	3.10***	-0.48	-0.21	22.2	0.90***	-0.03	-0.02	10.7
	(0.12)	(1.59)	(0.12)		(0.17)	(1.03)	(0.76)	
Past sick leave>0	3.72***	3.69***	2.69***	29.7	0.97***	0.85	0.82	12.1
	(0.14)	(1.27)	(0.93)		(0.28)	(1.48)	(1.46)	
Experience<median	3.45***	3.53**	2.01**	29.2	1.09***	0.11	0.09	11.5
	(0.12)	(1.41)	(0.83)		(0.21)	(1.34)	(1.13)	
Experience≥median	3.28***	-0.64	-0.34	21.7	0.71***	0.60	0.44	10.6
	(0.13)	(1.49)	(0.78)		(0.19)	(1.34)	(1.00)	
Earnings<median	3.74***	3.39*	1.48*	29.2	0.97***	0.41	0.33	11.0
	(0.12)	(1.81)	(0.81)		(0.20)	(1.05)	(0.88)	
Earnings≥median	3.04***	-0.03	-0.02	21.8	0.81***	-0.18	-0.13	11.4
	(0.12)	(1.16)	(0.80)		(0.21)	(1.27)	(0.97)	
Manufacturing	3.07***	1.45*	1.33*	23.6	0.71***	0.72	0.80	11.0
	(0.13)	(0.85)	(0.80)		(0.25)	(1.06)	(1.20)	
Non-manufacturing	3.61***	1.83	0.53	26.7	1.06***	-0.10	-0.05	11.0
	(0.12)	(2.55)	(0.76)		(0.16)	(1.62)	(0.84)	

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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. Regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Table 8: Heterogeneity: Effect of early retirement on the age at death

	Men				Women			
	OLS	IV	Reduced form	Mean	OLS	IV	Reduced form	Mean
White collar	-0.42***	0.65	0.19*	71.2	-0.15***	-0.04	-0.02	72.2
	(0.02)	(0.43)	(0.10)		(0.02)	(0.14)	(0.10)	
Blue collar	-0.55***	-0.20*	-0.13*	70.7	-0.17***	0.06	0.05	71.9
	(0.02)	(0.11)	(0.07)		(0.02)	(0.12)	(0.10)	
Past sick leave=0	-0.46***	0.23	0.10	71.1	-0.16***	0.02	0.01	72.1
	(0.02)	(0.17)	(0.07)		(0.02)	(0.10)	(0.07)	
Past sick leave>0	-0.56***	-0.35**	-0.25**	70.5	-0.15***	0.01	0.01	71.9
	(0.02)	(0.14)	(0.11)		(0.03)	(0.15)	(0.15)	
Experience<median	-0.51***	-0.33**	-0.19**	70.5	-0.16***	0.09	0.07	72
	(0.02)	(0.15)	(0.09)		(0.02)	(0.13)	(0.11)	
Experience≥median	-0.48***	0.22	0.12	71.1	-0.15***	-0.08	-0.06	72.2
	(0.02)	(0.16)	(0.08)		(0.02)	(0.13)	(0.10)	
Earnings<median	-0.57***	-0.34*	-0.15*	70.5	-0.16***	0.11	0.09	72.0
	(0.02)	(0.19)	(0.09)		(0.02)	(0.11)	(0.09)	
Earnings≥median	-0.43***	0.18	0.12	71.1	-0.15***	-0.06	-0.04	72.1
	(0.02)	(0.13)	(0.08)		(0.02)	(0.13)	(0.10)	
Manufacturing	-0.44***	-0.11	-0.10	71	-0.11***	-0.01	-0.01	72.1
	(0.02)	(0.09)	(0.09)		(0.03)	(0.11)	(0.12)	
Non-manufacturing	-0.54***	0.02	0.01	70.7	-0.18***	0.00	0.00	72.1
	(0.02)	(0.29)	(0.09)		(0.02)	(0.17)	(0.09)	

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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Table 9: Robustness: First-stage and reduced-form regressions for men with < 15 employment years

	Years in early retirement	Death before age 73 (in %)		Age at death		Obs.
		OLS	Reduced form	OLS	Reduced form	
White collar	-0.11	1.22***	-1.39	-0.16***	0.09	6,762
	(0.10)	(0.14)	(1.08)	(0.02)	(0.10)	
Blue collar	-0.05	2.44***	-0.11	-0.43***	0.04	9,473
	(0.08)	(0.17)	(1.21)	(0.03)	(0.13)	
Past sick leave=0	-0.09	1.83***	-0.48	-0.28***	0.02	14,080
	(0.06)	(0.11)	(0.92)	(0.01)	(0.09)	
Past sick leave> 0	-0.23	1.68***	-1.70	-0.37***	0.49	2,155
	(0.22)	(0.36)	(2.94)	(0.06)	(0.39)	
Experience=0	-0.05	0.71***	-0.98	-0.08***	0.11	5,240
	(0.10)	(0.15)	(1.17)	(0.01)	(0.09)	
Experience>0	-0.09	2.43***	-0.44	-0.41***	0.03	10,995
	(0.08)	(0.14)	(1.08)	(0.02)	(0.12)	
Avg. earnings=0	-0.05	1.62***	-0.44	-0.23***	-0.01	11,619
	(0.07)	(0.12)	(0.97)	(0.01)	(0.09)	
Avg. earnings>0	-0.19	2.30***	-0.51	-0.42***	0.22	4,616
	(0.14)	(0.21)	(1.73)	(0.03)	(0.23)	
Manufacturing	0.02	2.58***	-1.75	-0.44***	0.10	2,285
	(0.18)	(0.31)	(2.05)	(0.05)	(0.26)	
Non-manufacturing	-0.11	1.80***	-0.34	-0.28***	0.04	13,950
	(0.07)	(0.11)	(0.99)	(0.02)	(0.09)	

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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar. Additional controls are the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Table 10: Robustness: Using variation from all regions and pre-REBP cohorts, men

	First stage			IV		Obs.
	Years in early retirement	F-stat	Relative complier likelihood	Death before age 73 (in %)	Age at death	
Full sample	0.40***	145.1		1.54	-0.15	331,686
	(0.03)			(0.99)	(0.10)	
White collar	0.17***	14.4	0.47	-0.37	0.18	147,436
	(0.04)			(3.56)	(0.36)	
Blue collar	0.50***	171.4	1.18	2.02**	-0.23**	184,250
	(0.04)			(1.02)	(0.10)	
Past sick leave=0	0.38***	91.7	1.34	-0.13	0.04	209,287
	(0.04)			(1.25)	(0.12)	
Past sick leave> 0	0.47***	114.3	0.86	3.43***	-0.37***	122,399
	(0.04)			(1.31)	(0.14)	
Experience< median	0.45***	128	0.99	2.50**	-0.27**	165,647
	(0.04)			(1.20)	(0.12)	
Experience≥median	0.33***	63.2	1	0.15	0.00	166,039
	(0.04)			(1.56)	(0.15)	
Avg. earnings<median	0.32***	81.4	0.92	0.39	-0.07	165,841
	(0.04)			(1.57)	(0.16)	
Avg. earnings≥median	0.40***	60.4	0.7	1.84	-0.13	165,845
	(0.05)			(1.52)	(0.14)	
Manufacturing	0.62***	141.2	1.55	1.46*	-0.14	114,046
	(0.05)			(0.88)	(0.09)	
Non-manufacturing	0.24***	49.7	0.59	1.87	-0.22	217,640
	(0.03)			(2.03)	(0.21)	

Notes: Sample consists of men born between July 1927 and July 1943 in all regions of Austria. ***, **, * 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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar as well as controls for the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

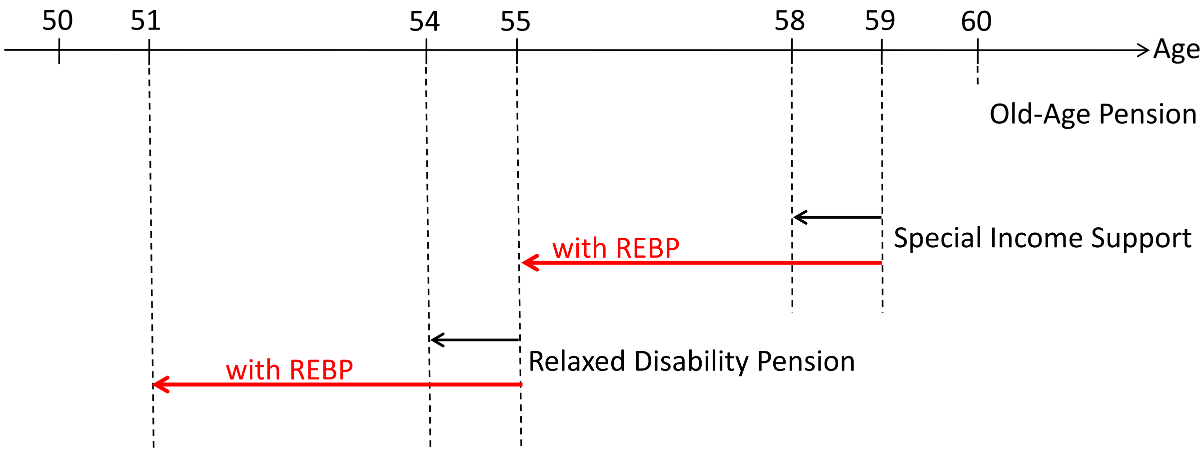
Table 11: Effect on program costs and earnings between ages 50-73

	Earnings benefits	UI	DI benefits	Old-age benefits	Sick leave	Total income
A. Men						
Eligible districts	-14,834***	8,052***	4,333***	1,398	10	-1,041
	(1,556)	(692)	(610)	(952)	(96)	(1,421)
Mean of Dep. Var.	191,463	16,581	27,091	180,434	4,306	419,876
Obs.	36,147	36,147	36,147	36,147	36,147	36,147
R^2	.481	.098	.128	.384	.075	.618
B. Women						
Eligible districts	-12,316***	9,798***	-2,633***	6,037***	-574***	312
	(1,773)	(997)	(501)	(1,524)	(106)	(1,628)
Mean of Dep. Var.	101,863	13,119	8,091	189,721	1,954	314,748
Obs.	10,555	10,555	10,555	10,555	10,555	10,555
R^2	.435	.144	.052	.577	.063	.760

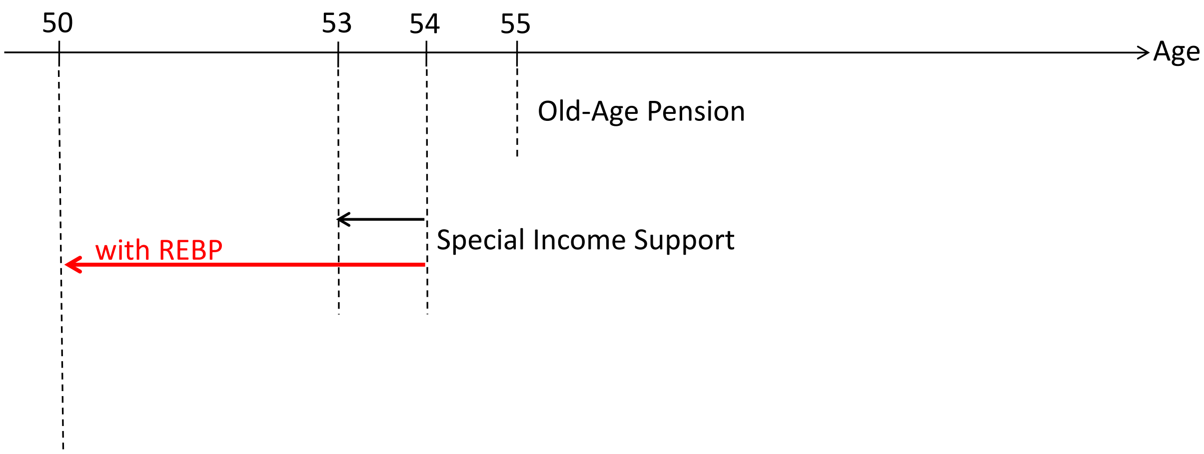
Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are adjusted for clustering on the cohort-region level. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar. Additional controls are the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Figure 1: Retirement Pathways with/without REBP Eligibility

(a) Men



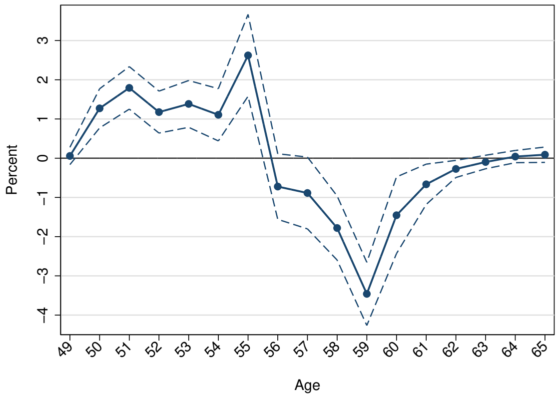
(b) Women



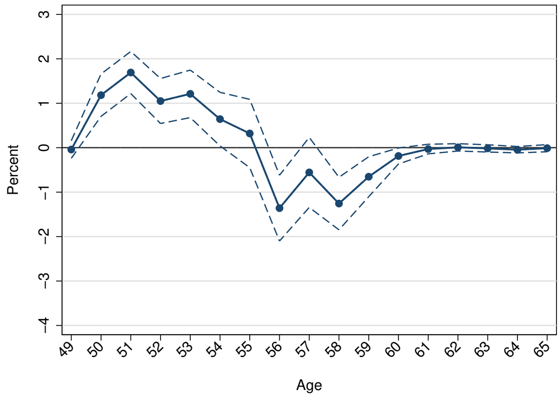
Notes: Black arrows denote maximum duration of regular UI benefits without REBP (1 year) and red arrows denote maximum duration of regular UI benefits with REBP (4 years). See text for details.

Figure 2: Effects on retirement at different ages, men

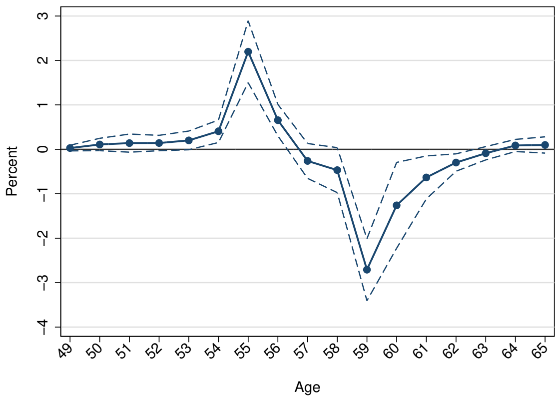
(a) Retirement



(b) Retirement with DI pension



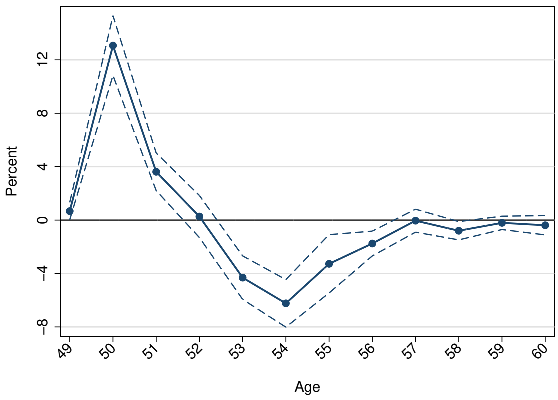
(c) Retirement with old-age pension



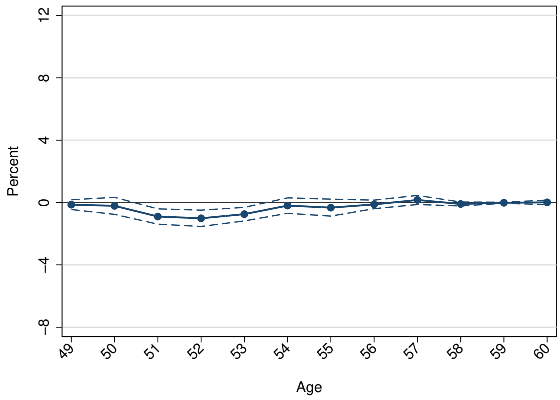
Notes: The figures plot the difference in the probability to retire at a certain age between eligible and non-eligible districts for men (panel a) and women (panel b), respectively. Dashed lines show 95% confidence bands.

Figure 3: Effects on retirement at different ages, women

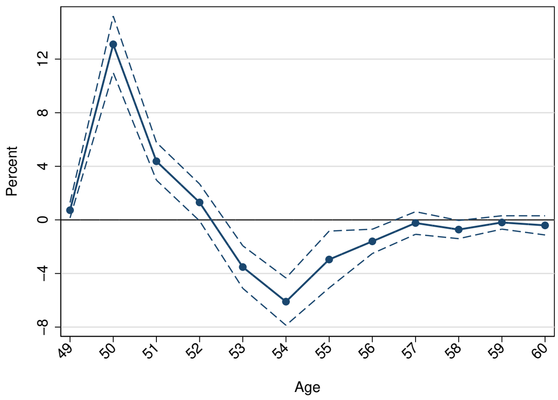
(a) Retirement



(b) Retirement with DI pension

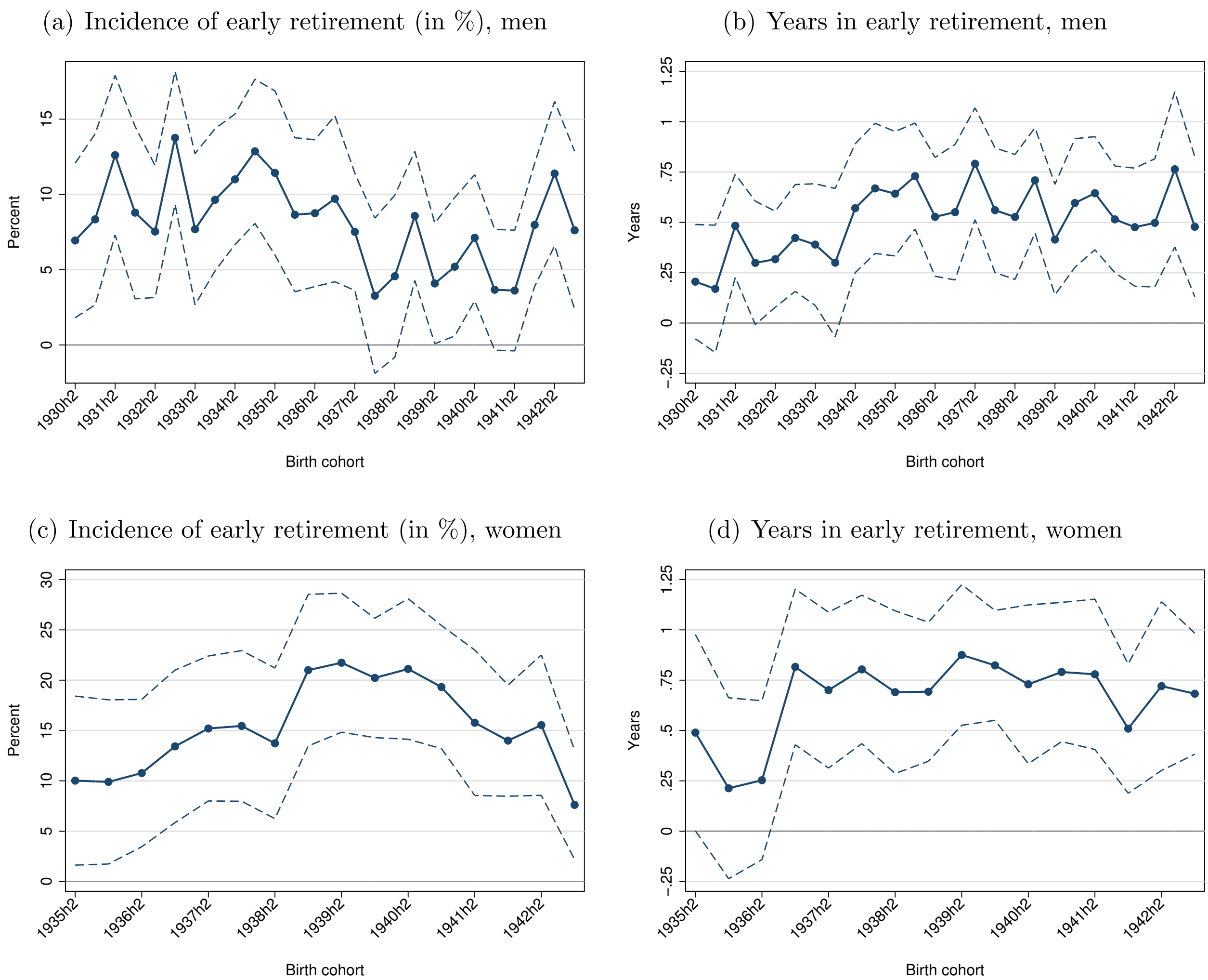


(c) Retirement with old-age pension



Notes: The figures plot the difference in the probability to retire at a certain age between eligible and non-eligible districts for men (panel a) and women (panel b), respectively. Dashed lines show 95% confidence bands.

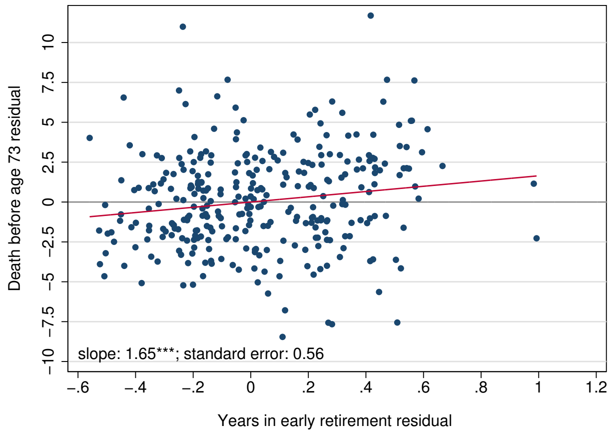
Figure 4: First-stage effects of REBP eligibility



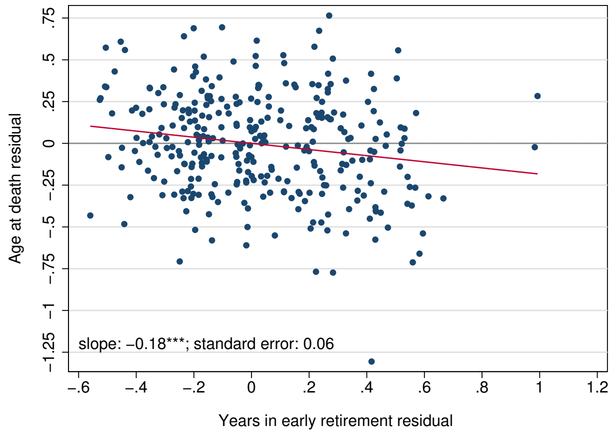
Notes: The figures plot the difference in the incidence of early retirement (panels a and c) and the number of years spent in early retirement (panels b and c) between eligible and non-eligible districts by year-semester birth-cohort for men and women, respectively. Dashed lines show 95% confidence bands.

Figure 5: Visual representation of 2SLS using month-of-birth variation

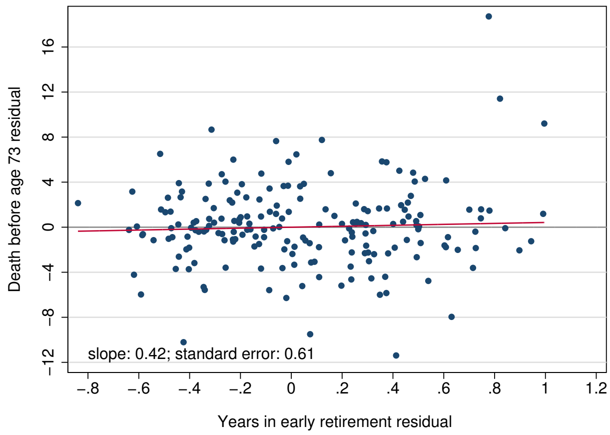
(a) Death before age 73 (in %), men



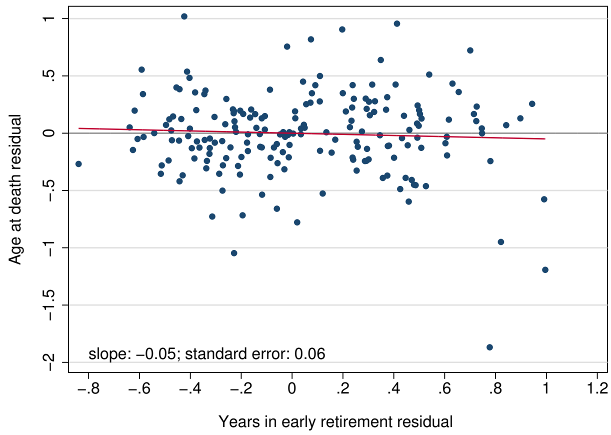
(b) Age at death, men



(c) Death before age 73 (in %), women



(d) Age at death, women



Notes: The figures plot the probability of death before age 73 (panels a and c) and age at death (panels b and d) against the years in early retirement. See text for details.

A Additional Tables and Figures

Table A.1: Robustness: First-stage and reduced-form regressions for women with < 15 employment years

	Years in early retirement	Death before age 73 (in %)		Age at death		Obs.
		OLS	Reduced form	OLS	Reduced form	
White collar	0.07	0.83***	-0.11	-0.07***	0.09	8,055
	(0.10)	(0.11)	(0.76)	(0.01)	(0.06)	
Blue collar	0.17**	0.91***	-0.59	-0.11***	-0.01	8,465
	(0.07)	(0.11)	(0.91)	(0.01)	(0.10)	
Past sick leave=0	0.12*	0.81***	-0.72	-0.08***	0.11**	15,042
	(0.07)	(0.08)	(0.60)	(0.01)	(0.05)	
Past sick leave> 0	0.14	1.52***	2.7	-0.21***	-0.55**	1,478
	(0.20)	(0.29)	(2.29)	(0.03)	(0.27)	
Experience=0	0.08	0.76***	-0.83	-0.06***	0.13**	8,336
	(0.1)	(0.11)	(0.70)	(0.01)	(0.06)	
Experience>0	0.15*	0.97***	0.06	-0.12***	-0.04	8,184
	(0.08)	(0.12)	(0.88)	(0.01)	(0.09)	
Avg. earnings=0	0.10	0.79***	-0.90	-0.07***	0.10*	12,522
	(0.07)	(0.09)	(0.62)	(0.01)	(0.05)	
Avg. earnings>0	0.20	1.04***	0.98	-0.13***	-0.09	3,998
	(0.14)	(0.15)	(1.39)	(0.02)	(0.16)	
Manufacturing	0.50**	1.09***	0.06	-0.09***	-0.06	1,742
	(0.21)	(0.20)	(1.60)	(0.02)	(0.19)	
Non-manufacturing	0.07	0.83***	-0.46	-0.09***	0.07	14,778
	(0.07)	(0.09)	(0.60)	(0.01)	(0.06)	

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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar. Additional controls are the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.

Table A.2: Robustness: Using variation from all regions and pre-REBP cohorts, women

	First stage			IV		Obs.
	Years of early retirement	F-stat	Relative complier likelihood	Death before age 73 (in %)	Age at death	
Full sample	0.47***	206.7		-0.87	0.03	140,057
	(0.03)			(0.85)	(0.09)	
White collar	0.3***	36.5	0.70	-3.78**	0.14	80,319
	(0.05)			(1.78)	(0.18)	
Blue collar	0.69***	204.3	1.35	0.31	0.01	59,738
	(0.05)			(0.9)	(0.09)	
Past sick leave=0	0.38***	87.9	0.81	-2.07*	0.13	98,283
	(0.04)			(1.19)	(0.13)	
Past sick leave>0	0.78***	222.5	1.57	0.17	-0.04	41,774
	(0.05)			(1.05)	(0.10)	
Experience$<$ median	0.51***	135.6	1.12	-1.15	0.08	69,975
	(0.04)			(1.15)	(0.11)	
Experience$\geq$median	0.44***	106.6	0.89	-0.90	0.01	70,082
	(0.04)			(1.18)	(0.12)	
Avg. earnings$<$median	0.49***	127.4	1.07	-0.49	0.04	70,028
	(0.04)			(1.14)	(0.11)	
Avg. earnings$\geq$median	0.47***	103	0.93	-1.60	0.06	70,029
	(0.05)			(1.17)	(0.12)	
Manufacturing	0.75***	184.8	1.62	0.14	0.05	37,771
	(0.06)			(0.94)	(0.09)	
Non-manufacturing	0.36***	87.0	0.75	-1.47	0	102,286
	(0.04)			(1.26)	(0.14)	

Notes: Sample consists of women born between July 1932 and July 1943 in all regions of Austria. ***, **, * 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 status and industry are measured at age 49. Past sick leave: years on sick leave between ages 39-48, experience: employment years past 15 years measured at age 49, earnings: cumulative earnings between ages 44-48. All regressions include dummies for birth year-month, NUTS-region, industry, and blue collar. Additional controls are the last daily wage at age 49, years of tenure at age 49, earnings between ages 39-43 and ages 44-48, and the number years on sick leave, unemployed, and employed between ages 39-43 and 44-48, respectively.