Fatal Attraction?
Access to Early Retirement and Mortality

Andreas Kuhn, University of Zurich and IZA*
Jean-Philippe Wuellrich, University of Zurich
Josef Zweimüller, University of Zurich, CEPR, CESifo and IZA

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Abstract

We estimate the causal effect of early retirement on mortality for blue-collar workers. 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 retiring one year earlier causes a 13 percent increase in the risk of premature death for men. We do not find any significant effect for women. Excess mortality among males is concentrated among heart diseases, diseases related to excessive alcohol consumption, and vehicle injuries.

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Contact: University of Zurich, Department of Economics, Mühlebachstrasse 86, 8008 Zurich, Switzerland; andreas.kuhn@econ.uzh.ch; jean-philippe.wuellrich@econ.uzh.ch; josef.zweimueller@econ.uzh.ch

1 Introduction

In many industrialized countries, dramatic 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. However, 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. 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 (**??**) as well as with changes in daily routines and lifestyles which are potentially associated with unhealthy behavior (e.g. **????**).

This paper presents new evidence on the causal effect of early retirement on mortality for blue-collar workers. Blue-collar workers are an interesting group because they typically work in physically demanding jobs and because emerging health problems – and/or their prevention – often induce these workers to retire earlier. To solve the problem of negative health selection into retirement we take advantage of a major change to the Austrian unemployment insurance system that extended the maximum duration of unemployment benefits for workers living in certain regions of the country. This 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 to extended unemployment 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 instrumental variable techniques. We find that a reduction in the retirement age causes a significant increase in the risk of premature death (defined as death before age 67) for males but not for females. The effect for males is not only statistically significant but also quantitatively important: one additional year of early retirement causes an increase in the risk of premature death of 2.4 percentage points (about 13.4 percent). IV estimates are considerably smaller than the simple OLS estimate, for both men and for women, which is consistent with selection into early retirement based on bad health.

We consider several channels to understand why male early retirees die earlier. A first channel could be lower permanent income associated with early labor force exit. However, we find that earnings losses quantitatively too small to explain the observed impact on mortality. A second channel is based on adverse health-related behaviors (smoking, drinking, unhealthy diet, and lack of physical exercise). An analysis of death causes strongly supports this hypothesis. Male excess mortality is concentrated on three causes of deaths: (i) ischemic heart diseases (mostly heart attacks), (ii) diseases related to excessive alcohol consumption, and (iii) vehicle injuries. These death causes account for 78 percent of the causal retirement effect (while accounting for only 24 percent of all deaths in the sample). We calculate that 32.4 percent of the causal retirement effect can be directly attributed to smoking and excessive alcohol consumption. A third channel suggests that the male mortality effect arises from retirement following an involuntary job loss but not from voluntary quits. Exploiting severance payment rules to proxy the voluntariness of the retirement decision, we find that involuntary job losses are likely to cause excess mortality among blue-collar males, while voluntary transitions into early retirement do not.

Our study goes beyond the existing literature in several respects. First, our empirical strategy is based upon a policy change that generates huge exogenous variation in the potential minimum age of permanently leaving the labor force. While treated and control groups are ex-ante similar in observable characteristics, the group of eligible individuals retires between 9 and 12 months earlier than the group of non-eligible individuals. Second, we use an administrative data set 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 his or her death is thus precise so our estimates are 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 blue-collar workers in the private sector in Austria. Hence there is a sufficiently large number of observations that help us to get precise estimates. This is a particular advantage in the present context, because many previous studies (mostly those based on survey data) often face the problem of imprecise estimates due to small sample sizes.

We think that our results, while based on the experience of a small country, are of more general interest. The effect we estimate is unlikely to originate from the particular insti-

[1]The distinction between subjective and objective measures appears to be of special importance (?), as even self-reported measures of physical health may be subject to considerable reporting error (?). 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 and similar effects. Indeed, studies using subjective health measures tend to find beneficial effects of retirement 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, especially in survey data, whereas such error is arguably of minor importance in administrative data.

tutional framework. Treated and control workers are both covered by mandatory universal health insurance and by a rather generous old-age social security system. Hence our estimates are not driven by unequal access to health care or by major income losses after retirement. Instead we estimate a more direct effect of early retirement on mortality. (If early retirees had no access to health care or did suffer major income, the effects would be even larger. In this sense, our estimates provides a lower bound for the mortality effect of early retirement.) Another reason why we think that Austria in an interesting case is that early retirement is a very common phenomenon (the average retirement age is as low as 58). Hence the typical early retiree in our sample is quite similar (though clearly not identical) to the average blue-collar worker rather than a member of a highly selective group.

While there is a larger literature studying the nexus between health/mortality and retirement, studies adopting convincing empirical strategies are rare. **?** use institutional rules governing eligibility to public pensions to identify a causal effect of retirement. They do not find effects, once the possibility of endogenous entry into retirement is taken into account. **?** exploit unexpected "retirement windows" (i.e. early retirement opportunities offered by firms to groups of workers) in the US. They find no detrimental effects of early retirement on health and, if anything, even temporary improvements in health following retirement. **?** uses age discontinuities in retirement incentives and legal changes to these incentives and finds a positive effect of retirement on subjective well-being. Similar results on mental well-being are reported in **?** for the US. and **?** for the UK, and **?** in a cross-country study for Europe, all using survey data and a similar empirical design. **?** use panel-data methods to study the effects of labor market status on the health of Dutch elderly, finding that early retirement has a positive impact on self-assessed measures of health. **?** is one of the few studies finding that retirement increases both the risk of a cardiovascular disease and the risk of being diagnosed with cancer. Qualitatively similar results are reported in **?**, who analyze the effects of retirement using panel-data methods and relying on survey data from the US. They find negative effects of retirement on both mental health and measures of self-assessed physical health. Note, however, that conventional panel-data methods are vulnerable to time-varying unobserved confounders such as unobserved health shocks. Exploiting exogenous changes in social security benefits in the US. **?** find that retirees with lower benefits have lower mortality rates after age 65, a result that is in stark contrast to the broad consensus in this literature. They reconcile their counterintuitive results by showing that the lower benefits encouraged retirees to do more post-retirement part-time work off-setting the detrimental impact of lower social security benefits. In sum, the available evidence uses different outcome measures and different strategies to deal with endogenous entry into retirement and, consequently, yields no clear pattern regarding the causal impact of retirement on health.[2]

The remainder of this paper is structured as follows. In section **??** we discuss the institutional background for Austria. Section **??** discusses the data source as well as the selection of our sample and presents descriptive statistics. Details of our econometric framework are given in section **??**. The results are presented in sections **??** and **??**. In section **??**, we focus on potential channels explaining excess mortality among male retirees. Section **??** concludes.

2 Pathways to Early Retirement in Austria

In this section we describe the pathways into early retirement in Austria. We define as "early retirement" the date at which an individual withdraws permanently from the labor market. This definition does not require the individual to be a retiree in the legal sense of drawing regular old age social security benefits. Instead, our definition of early retirement hinges upon the last day of regular employment and does not refer to the particular transfer associated with permanent exit from work.

2.1 Unemployment Insurance and Early Retirement

Almost all workers in Austria are covered by the old age social security system. Income transfers from this system are the most important source of income for retirees (**?**). Old age social security benefits depend on retirement age, the contributions (i.e. earnings) made to the system in the years before retirement as well as on the number of contribution months (i.e. work experience).[3] The gross replacement rate for a worker retiring at the statutory retirement age in the year 1993 was at most 80% of his or her previous earnings, given a continuous work history with 45 insurance years before retiring. Social security benefits are subject to income tax and mandatory health insurance contributions. The regular statutory retirement age is 65 for men and 60 for women. For workers with long-insurance duration the statutory retirement age is 60 for men and 55 for women ("vorzeitige Alterspension wegen langer Versicherungsdauer"). Eligibility to statutory retirement with long-insurance duration is linked to an individual's previous work history: workers who paid social security

[2]Our paper is also related to a literature that focuses on the impact of involuntary job loss on mortality. An interesting recent study by **?** finds a strong impact of involuntary job loss on mortality for US workers, particularly for older (high-seniority) workers and for workers who suffer large earnings losses (i.e. low-wage workers). In a related study for Sweden, **?** find a strong increase in overall mortality among male job-losers, but no impact among females. However, an increase in suicides and alcohol-related mortality was found for both men and women. Adverse effects of involuntary job loss on mortality for Norwegian workers are reported by **?**.

[3]There were several changes to the pension system during our observation period. However, these changes affected both the treatment and the control group in the same way. See **?** for details.

contributions for at least 35 years and who worked at least 2 out of the 3 years prior to retirement have the option to retire early at age 60 for men and at age 55 for women.

Apart from direct transitions from employment, the most important pathway into early retirement is the indirect transition from employment to retirement via the unemployment system. Individuals with a continuous work history are eligible for regular old-age social security benefits at age 60 after having drawn unemployment benefits for at least 12 out of the previous 15 months ("vorzeitige Alterspension wegen Arbeitslosigkeit"). In addition, unemployed individuals with a continuous long-run work history (social security contributions for at least 15 out of the last 25 years) are also eligible to 12 months in special income support ("Sonderunterstützung").[4] The special income support programme essentially allows workers to exit the work force at age 58 and bridge the gap to regular old age social-security benefits via an unemployment spell of 52 weeks (30 weeks before August 1989) and special income support for another 12 months. A third pathway is via disability insurance. Access to disability is relaxed after age 55 when eligibility rules to disability benefits become significantly relaxed (**?**).[5]

2.2 The Regional Extended Benefit Program

To assess the causal effect of early retirement on mortality, we exploit a policy change to the Austrian unemployment insurance system that introduced a further pathway to retirement, the Regional Extended Benefit Program (REBP). The REBP was introduced in response to the steel crisis of the late 1980s which hit certain regions of the country particularly hard. To mitigate economic hardship in these regions, the Austrian government enacted a change in the unemployment insurance law that granted access to unemployment benefits (UB) for up to 209 weeks.[6] To become eligible, a worker had to fulfill the following three criteria at the time of unemployment entry: (i) age 50 or older, (ii) a continuous work history before becoming unemployed (i.e. 780 weeks of employment in the last 25 years preceding the unemployment spell), and (iii) at least 6 months of residence in one of the eligible regions. The program was enacted in June 1988 and remained in force until July 1993.[7] In contrast,

[4]Special income support is equivalent to a regular unemployment spell from a legal point of view, but grants a transfer that is 25% higher than regular unemployment benefits.

[5]After age 55, disability benefits could be drawn when an individual's work capacity within his or her main occupation is reduced by more than 50 percent of that of a healthy individual. Before age 55, a reduction of the individual's general work capacity, not restricted to a particular occupation, is required for eligibility to a disability pension.

[6]Previous econometric evaluations of the REBP have found large effects of the program on realized unemployment duration (**????**).

[7]Initially 28 out of about 100 labor market districts were eligible to extended unemployment benefits. The REBP underwent a reform in January 1992 that excluded 6 formerly eligible regions from the program. Moreover, eligibility criteria were tightened, as not only location of residence but also the individual's

workers aged 50 or older who were not eligible to the REBP were entitled to a maximum of 52 weeks of regular unemployment benefits (to only 30 weeks before August 1989). The REBP allowed eligible workers to withdraw permanently from employment as much as 3 years (3.5. years before August 1989) earlier than non-eligible workers.

The institutional setup implies that individuals eligible to the REBP could effectively withdraw from the labor force at age 55 (men) or 50 (women) by claiming unemployment benefits for the maximum duration of 4 years, followed by one full year of special income support. This is different for workers not eligible to the REBP. Male workers had the option of effective retirement at age 58 (58.5 before August 1989) and female workers at age 53 (53.5 before August 1989) by bridging the time until the regular early retirement age by exhausting the maximum duration of unemployment benefits of 52 weeks (30 weeks before August 1989) followed by one year of special income support.

3 Data and Sample

3.1 Data Source

We use individual register data from the Austrian Social Security Database (ASSD), described in detail in **?**. The data cover the universe of Austrian wage earners in the private sector and collects, on a daily basis, workers' complete labor market and earnings history up to the year 2006. The data also contain a limited set of socio-economic characteristics (year and month of birth, age, sex, general occupation) and contains a firm identifier. The administrative purpose of collecting these data is to provide all the information necessary for calculating old age social security benefits.

The data contain precise information on the date of retirement and on mortality (date of death). Information on mortality is observable up to the year 2008. Moreover, the data contain information necessary for determining an individual's eligibility to the REBP. This latter information is of crucial importance for the purpose of this study as we exploit the exogenous variation in the effective retirement age induced by the REBP (REBP-eligibility status is used as an instrument for the retirement age). Information on individuals' month of birth and employment history allows us to determine whether a worker meets the age and employment criteria set by the REBP. We do not observe the place of residence 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 this is not a major drawback, as most individuals work in the same labor market district

workplace had to be in a REBP region (see section **??** for details).

where they live.[8]

3.2 Sample Selection

Workers

First, we restrict the analysis to blue-collar workers.[9] The main reason for our focus on blue-collar workers is that the REBP was a program targeted towards regions with a high dominance of blue-collar workers. While the program was also available for all workers, take-up by white collars was weak.[10] We restrict the sample to workers who meet both the age- and the experience-criterion during the REBP-period. We consider males born between July 1929 and December 1941 and females born between July 1934 and December 1941, respectively.[11] This ensures that individuals in the sample eventually turn age 50 during the REBP and men (women) were aged 59 (54) or younger when the REBP was introduced. These cohorts benefitted from the REBP, albeit to a varying extent. For instance, males born between 1934 and 1938 could take full advantage of the REBP because they reached age 55 during the time the REBP was in place. In contrast, males born before 1934 were too old to take full advantage (i.e. they were already 56 when the REBP started) and cohorts born after 1939 too young (i.e. they were only 54 when the REBP was abolished). We further restrict the sample to workers how meet the work experience criterion of the REBP (i.e. workers with at least 15 employment years during the last 25 years). Furthermore, we only consider individuals with at least one employment year during the last two years at age 50, a requirement for being eligible to draw employment benefits. Because all selected individuals meet both the age and the experience criteria, the assessment of whether or not a worker is eligible to extended UB entitlement entirely hinges on individuals' region. This

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

[9] Because blue and white collar workers in Austria are partially subject to different social security rules (for example, there are differences in notice periods and the duration of sick leave benefits), we can determine workers' occupational status without any significant measurement error.

[10] In fact, eligibility status is a highly significant predictor of early retirement among blue-collar workers, but not among white collar workers. One potential explanation is that blue-collar (low income) workers face higher replacement rates than white collar (higher income) workers when unemployed and thus higher incentives for taking advantage of the program. Specifically, replacement rates (both with respect to unemployment benefits and early retirement benefits) are much lower for white collar workers due to earnings caps. Because the instrument is too weak, results remain inconclusive in the case of white collar workers.

[11] In principle, we could also consider the cohorts born from January 1942 to July 1943 as they (eventually) meet the age criteria as well. However, the data available to us from the ASSD only tracks individuals' labor market histories up to 2006. We omit cohorts born later than December 1941 in order to observe individuals' labor market histories at least until age 65 (i.e. men's statutory retirement age).

means that by using REBP eligibility as instrument for the retirement age, we basically compare individuals who work in eligible regions with those who work in non-eligible regions (section **??** provides the details).

Finally, we drop workers from the steel sector because our instrument does not induce changes in the retirement age for these workers. The reason is that, apart from the REBP, there was a nation-wide program to alleviate problems associated with mass redundancies in the steel sector, the "steel foundation". Firms in the steel sector could decide whether to join, in order to provide their displaced workers with state-subsidized re-training measures organized by the foundation. Member firms had to co-finance this foundation. Displaced individuals who decided to join this outplacement center were entitled to claim regular unemployment benefits for a period of up to 3 years (later 4 years), 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.

Regions

To make sure that potential differences in labor market conditions between treated and control regions do not contaminate our empirical estimates, we contrast only those eligible and non-eligible districts that are adjacent to each other and economically similar. We use the common classification of territorial units for statistics (NUTS). NUTS comes in three aggregation levels, of which we choose the most disaggregated one, NUTS-3.[12] We further confine our sample to those NUTS-3 regions that contain both eligible and non-eligible districts. Since NUTS-3 regions comprise geographically adjacent districts and because these units are quite small, this procedure implies that differences in labor market conditions between treated and control regions are unlikely to affect our analysis.[13]

[12]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).

[13]Even though we think that there is no strong a-priori reason for believing that individuals' health status was decisive in determining a given community's treatment status, we will return to this issue later (see section **??**). See also the discussion in **?** and **??** on how the regions were selected for eligibility in the first place. Importantly, **?** show that both employment and unemployment rates for (potentially) eligible workers were quite similar before the start of the program. However, they also show that the program significantly increased the risk of unemployment for older workers, suggesting that the program may have been used deliberately as a path into early retirement, especially for women (**?**). Indeed, our results on the first-stage effect of the program are perfectly in line with this finding (see section **??** below).

3.3 Key Measures and Descriptive Statistics

The key variables of our analysis are our measures of early retirement and mortality. As mentioned above our sample includes only cohorts born between 1929 and 1941 (men) and 1929 and 1934 (women), respectively. Because information on labor-market histories is only available until December 2006 and information on mortality only until July 2008, individual labor-market histories of workers included in the sample can be tracked (at least) up to age 65 and individuals' mortality-related information is available (at least) up to age 67. We use this to define our dependent variable indicating premature death, a dummy variable that indicates whether a worker died before reaching age 67.[14] Since workers in our sample have to be alive at age 50 and meet the REBP age and experience criteria our mortality indicator measures whether or not an individual in our sample dies between age 50 and age 67. This is a meaningful indicator in the present context. Since we are studying birth cohorts 1941 and older, we are considering individuals whose life expectancy is still quite low (see footnote **??**). Moreover, we look at blue-collar workers whose life expectancy is lower than that of white-collar workers. In our sample, the probability of death before age 67 is 18.0 percent for males and 7.2 percent for females.

Our treatment variable is the number of early retirement years. This variable measures the time span between the statutory retirement age at age 65 (for men) and 60 (for women), respectively, 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.[15] Hence a positive number on the endogenous variable denotes that an individual has retired before the statutory retirement age. Throughout the analysis, we will stratify the sample by gender because retirement and mortality patterns of men and women are very different.

Table **??**

Our final sample consists of 17,590 blue-collar males and 3,283 blue-collar females of whom 18.0 percent and 7.2 percent die before age of 67, respectively. Both male and female

[14] One might object that this measure is ill-suited for studying mortality because it only covers deaths occurring between age 50 and age 67. Note, however, that life expectancy at birth was not yet very high for those birth cohorts considered in the analysis. In fact, according to the life table based on data from 1930/33, life expectancy at birth (at age 45) was 54.5 (24.7) years for men and 58.5 (27.0) years for women (figures taken from Statistics Austria).

[15] Recall that our indicator does not require the individual to be a retiree in the legal sense of drawing regular old age social security benefits. Instead, our definition of effective retirement hinges upon the last day of employment and does not refer to a particular transfer an individual gets after ceasing work permanently. Effectively retired individuals draw unemployment benefits, disability benefits, old-age social security benefits, some other type of benefit, or no transfer.

workers exit the labor force substantially earlier when eligible to the REBP. The difference in the effective retirement is as much 0.75 years (9 months) for males and 1.15 years (14 months) for females. Treated and control samples are well balanced. There is almost no difference in average (and variance of) age and marginally higher previous work experience before age 50 in non-eligible regions. Blue-collar workers in eligible regions were slightly less often on sick leave before age 50, had somewhat higher earnings before age 50 (average earnings at ages 43 to 49) than those in control regions. The industry mix of treated and control samples is also similar, though not identical. The two subsamples are similar, but they are not identical. Hence our empirical analysis below controls for observable characteristics.

4 Econometric Framework

OLS estimates of a regression of individuals' mortality risk on an indicator of early retirement will overestimate the true causal effect of early retirement on mortality when there negative health selection into retirement (e.g. **??**). Let $Death_i^{67}$ denote a dummy variable indicating prematue death (which takes value 1 when the worker dies before age 67, and 0 otherwise). ER_i denotes years spent in early retirement, i.e. the difference between statutory age and the date of exit from work. The model to be estimated is given by

$$Death_i^{67} = \beta_0 + \beta_1 ER_i + X_i \beta + \epsilon_i, \tag{1}$$

where X_i denotes additional control variables and ϵ_i is the error term. We are interested in estimating the parameter β_1, the causal effect of an additional year in early retirement on premature death (i.e. death before age 67). Since workers self-select into early retirement based on factors that are not observed in the data, e.g. unobserved health shocks, ER_i is endogenous and thus the simple OLS estimate of β_1 is biased.

4.1 Identification

To assess the causal relationship between early retirement and mortality, we instrument the years spent in early retirment by workers' REBP eligibility. Using this empirical strategy, we estimate the causal mortality-effect for those individuals whose labor force exit date is affected by REBP-eligibility. (e.g. **??**). The credibility of our empirical strategy hinges upon the assumption that our instrument is "as good as randomly assigned". In other words, REBP eligibility should be uncorrelated with unobserved variables that are associated with retirement age and that simultaneously affect the risk of premature death. REBP eligibility was not randomized but is a function of age, previous work experience, and location of residence. Hence REBP eligibility should be considered conditionally randomized on the eligibility age, experience, and region.[16] Since the age and experience criteria are fulfilled by construction of the sample, the question of whether our instrument is valid or not essentially boils down to the question whether the risk of premature death is correlated with individuals' regions of residence in the absence of the program (an issue that we take up in section **??** below).

An equivalent way of thinking about our empirical design is to consider the eligibility criteria Z_i as a deterministic function of a worker's age, work experience, and region. Age and previous work experience are unlikely to be endogenous in the present context.[17] However, endogenous mobility across regions may be an issue since workers may move from non-eligible districts to eligible districts in order to become eligible for the program. This latter problem is mitigated by the fact that eligibility rules require residence in a treated region of at least 6 months prior to claiming unemployment benefits. Moreover, mobility is rather uncommon among older workers in Austria. In 1991, for example, only 3 percent (4 percent) of individuals aged 55-59 (50-54) had moved across districts within states or across states within the last 5 years.[18] This suggests that the type of mobility that would cause worries for our empirical strategy is a rather negligible phenomenon.

Another related problem may arise if location of residence has per se an effect on individuals' mortality risk. Location of residence is a REBP eligibility criterion. Conditioning on place of residence at the district level is thus not feasible, since it is perfectly correlated with our instrument. To circumvent this potential problem, we include only those NUTS-3 regions in our sample that comprise both districts eligible to the REBP and districts that are not so. If neither mortality risk nor the duration of early retirement is governed by REBP-eligibility status within any NUTS-3 unit, the independence assumption likely holds, ensuring the validity of our instrument.[19]

[16]Introducing covariates into the heterogeneous effects model technically calls for the semi-parametric procedure proposed by **?**. However, no extension of this procedure for models with variable treatment intensity yet exists (i.e. age at retirement is a continuous variable). On the other hand, however, **?** argues that 2SLS is likely to give a good approximation to the causal relationship of interest in many cases (i.e. the **?** procedure is identical to 2SLS when the first stage is (approximately) linear).

[17]Age can clearly be considered as exogenous in our setting. The employment criteria may be subject to an endogeneity issue if individuals improve their work history in order to become eligible for the program. However, we restrict the sample to individuals with an almost continuous work history (recall from Table **??** that the workers in our sample have on average more than 20 employment years during the last 25 years). Since the REBP was only announced shortly before coming into force and was in place for only 5 years, the workers in our sample fulfilled the employment criteria even without altering their work behavior.

[18]The Austrian census asks individuals whether they moved in the past 5 years. According to these data, 88% did not move at all, 5% moved within communities, 1% moved across communities within district, and 2% immigrated from abroad (figures are from census data, Statistics Austria).

[19]Three additional assumptions are needed, and they are likely to be fulfilled. First, we have to assume that the only channel through which REBP eligibility has an impact on premature death is through its

Let us now turn to the first-stage regression. Assume that years in early retirement are determined by the following equation

$$ER_i = \alpha_0 + Z_i\alpha_Z + \sum_j C_{ij}\alpha_{Cj} + \sum_k E_{ik}\alpha_{Ek} + \sum_l N_{il}\alpha_{Dl} + X_i\alpha_X + \varepsilon_i, \qquad (2)$$

where, as before, the endogenous variable ER_i corresponds to the number of years spent in early retirement. Z_i is our binary instrument, denoting whether an individual was eligible ($Z_i = 1$) or not eligible ($Z_i = 0$) to the REBP. The variables C_{ij}, E_{ij}, and N_{il} refer, respectively, to the workers' date of birth, previous work experience, and NUTS-3 unit of residence, i.e. the three eligibility criteria of the program.[20] We also include additional control variables, denoted by X_i, in some specifications.[21] These additional controls increase the precision of our estimates and are helpful in underlining the credibility of our empirical strategy by showing that these additional controls do not have an effect on the 2SLS estimates.

Finally, notice that the REBP was in effect for a limited period of time. This implies that the various birth cohorts differ in the extent to which the REBP actually offered a pathway to early retirement. For instance, birth cohort 1930 was already 58 years old at the date when the REBP was implemented. In contrast, birth cohort 1933 was 55 years old when the REBP started. The former cohort could take only limited advantage of the program (retiring at age 58), whereas the latter cohort could take full advantage of the program (by already retiring at age 55), as the actual benefits stemming from the program depend on an individual's date of birth. To capture the heterogeneity in the effect of the instrument on the first-stage outcome, we allow for cohort-specific effects by including interaction terms

impact on the duration of early retirement. Thus the instrument must not have any direct effect on the dependent variable. We believe that this assumption holds in the present context, as it is difficult to imagine that the mere eligibility to extended benefits should have any direct effect on health and mortality. Second, we assume that the instrument has a monotone impact on the endogenous variable. In our context, we have to assume that REBP eligibility induced *some* individuals to retire earlier than in the absence of eligibility, and that *no* individual decided to retire later because of REBP eligibility. Although we cannot test this assumption, we think it is quite unlikely that this assumption fails in our application. Finally, the REBP eligibility must have an effect on the early retirement date (i.e. the date when individuals permanently leave the labor force). We show in some detail that this is indeed the case in section **??**.

[20]Specifically, j indexes half-year-of-birth and runs from 1929h2 to 1941h2 for men and from 1934h2 to 1941h2 for women; k refers to the past 1, 2, 5, 10, and 25 years (before age 50); and l indexes those 8 NUTS-3 units included in the analysis. For work experience, we also include squared terms.

[21]The list of additional control variables is as follows: Several terms counting the number of past days on sick leave (also indexed by k) and the corresponding squared terms, employers' industry affiliation (14 industries), the log of the average of yearly earnings between ages 43 and 49, and the log of the standard deviation of yearly earnings between ages 43 and 49.

between the eligibility indicator and year-semester of birth into the first-stage equation

$$ER_i = \alpha_0 + \sum_j (Z_i \cdot C_{ij})\alpha_{Zj} + \sum_j C_{ij}\alpha_{Cj} + \sum_k E_{ik}\alpha_{Ek} + \sum_l N_{il}\alpha_{Nl} + X_i\alpha_X + \varepsilon_i, \qquad (3)$$

which implies that we now have 25 instruments for our male cohorts (1929h2–1941h2) and 15 instruments for our female cohorts (1934h2–1941h2), respectively. 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 the cohort-region level in order to correct for group-level errors.

4.2 Assessing Instrument Validity

Let us now discuss our key identifying assumption that location of residence is exogenous with respect to individuals' health status. We provide two pieces of evidence supporting the validity of our instrument. First, we show estimates from a regression of standardized mortality rates at the district level for the years 1978–1984, a period well before the REBP was implemented (see Table **??**). Differences in standardized mortality rates are available at the district level for four different age groups, separately for men (columns (1) to (4)) and women (columns (5) to (8)). The table shows estimates from a simple regression of (district-specific) log standardized mortality rates on a dummy indicating a REBP region. It turns out that standardized mortality rates did not differ between eligible and non-eligible districts before the REBP started. The relevant point estimate turns out to be both statistically and quantitatively insignificant.

Table **??**

The second piece of evidence makes use of individual-level information on workers' days on sick leave before the individual turns age 50. To assess whether eligible and non-eligible individuals have ex-ante similar health conditions, we regress the number of sick leave days on our binary instrument Z_i while controlling for cohort fixed-effects, experience, NUTS-3 fixed-effects, industry fixed-effects, and earnings. Table **??** shows results for four different counts of sick leave days, separately for males and females. It turns out that workers' health conditions do not systematically differ between eligible and non-eligible individuals within the same NUTS-3 units, and this is valid for both men and for women. Taken together, we think that the evidence presented in Tables **??** and **??** provides strong support for our claim that the selection of eligible labor-market districts was unrelated to mortality in these districts.

Table ??

5 Program Eligibility and Early Retirement

We proceed by presenting first-stage estimates of equations (??) and (??), respectively. Results are given in Table ??, for men and women separately. We show estimates for four different specifications for each gender. For men and women alike, specifications (1) and (2) estimate one common effect of the instrument on the endogenous variable, while specifications (3) and (4) allow for a varying effect across birth cohorts. Specifications (1) and (3) control for cohort fixed-effects, past work experience, and NUTS-3 fixed-effects; specifications (2) and (4) additionally include past sick leave days, the average as well as the standard deviation of yearly earnings (during ages 43 to 49), and industry fixed-effects.

Table ??

We start with the just-identified case, shown in the first two columns of each panel. For males, the common first-stage effect of the instrument amounts to 0.71 years. This means that REBP-eligibility lowers the effective age of retirement by roughly 8.5 months. If we add further controls in specification (2), the effect of the instrument is somewhat reduced to 0.59 years (roughly 7 months). For women, first stage effect averaged across birth cohorts amounts to 1.01 years in the first specification and is only slightly reduced to about 0.94 years when additional controls are included.[22]

Next, we turn to the over-identified case, given by equation (??) above. The overall pattern becomes more apparent in a graph. Figure ?? displays the relevant parameter estimates, $\hat{\alpha}_{Zj}$, per year-semester cohort (these estimates correspond to those displayed in column (3) of Table ??). The underlying regressions control for cohort fixed effects (one for each year-semester cohort), work experience, and NUTS-3 fixed-effects. Panel (a) shows that the first-stage effect is small for older cohorts and becomes increasingly larger for younger cohorts. This is exactly what we expect, given the REBP rules. Cohorts born in 1929 were already close to 60 years old when the REBP was implemented. Consequently, the REBP cannot have had a sizable impact on the date of permanent exit from the work force for them. The figure shows that the strongest impact is observed for cohorts born in 1934 or later, who could take full advantage of the REBP. This strongly suggests that the REBP

[22]A closer look at the distribution of the effective retirement age reveals that the fraction of male workers still at work changes drops very strongly at age 55 in REBP regions but in non-REBP regions. Similarly for females at age 51. This supports that idea that REBP eligibility drives differences in the distribution of early retirement ages. This issue is explored further in Appendix Figure ??.

entitlement strongly drives the pattern of permanent labor force exit. For female workers, the pattern is similar and the size of the first-stage effect is even more pronounced (see Panel (b)).

Figure **??**

Specification (3) of Table **??** reports the estimates from Panel (a) of Figure **??**. The first stage effect ranges from 0.031 years (birth cohort 1931h1) to 1.36 years (birth cohort 1937h2). Beginning with birth cohort 1931h2, all estimates are statistically significant at least at the 5%-level (except for birth cohorts 1933h2 and 1938h2). Statistical significance is also reflected in the relevant F-statistic, calculated for the excluded instruments only and reported at the bottom of the table. It amounts to above 9, i.e. it is very close to the threshold value of 10 above which 2SLS is not supposed to be subject to a weak instruments critique as proposed by **?**. Adding further controls again reduces the magnitude of the first-stage effect somewhat, but the F-statistic for the excluded instruments is still larger than 9.[23]

Column (7) of Table **??** shows the corresponding point estimates for women, displayed graphically in Panel (b) of Figure **??**. The first-stage effect varies across birth cohorts, ranging from about 0.33 years (birth cohort 1935h1) to about 1.63 years (birth cohort 1939h2). Starting with birth cohort 1936h1, all coefficients are statistically significant at least at the 5%-level. Adding further controls in column (8) hardly changes anything. The F-statistic for the excluded instruments exceeds the value of 10 in both column (7) and column (8). This again suggests that we do not run into any weak-instruments issues.

6 The Effect of Early Retirement on Mortality

Tables **??** and **??** report our main results for blue-collar males and females, respectively. Column (1) of Table **??** shows the OLS estimates of a regression of the number of early retirement years on mortality for blue-collar males. The regression controls for birth-cohort fixed-effects, work experience, and NUTS-3 fixed-effects. The OLS estimate is highly significant and amounts to 0.0322 (with a standard error of about 0.0012). Taken literally, this would imply that the probability of dying before age 67 increases by 3.22 percentage points

[23] Appendix Table **??** provides evidence on whether the REBP really causes the contrast in the retirement age, or whether this is simply due to regional differences between eligible and non-eligible districts. It shows the first-stage for cohorts who are not eligible to the REBP (i.e. workers aged less than 50 when the REBP ends). It turns out that no systematic difference emerges between eligible and non-eligible districts for cohorts too young for extended UB entitlement. This strengthens our claim that the contrast in the effective retirement age is causally linked to the REBP.

for each year of early retirement. In terms of the average probability of dying before age 67 (equal to about 18.0%), this corresponds to a relative increase of about 17.9%. The inclusion of additional controls does not change the OLS estimate. However, as argued before, OLS estimates are likely plagued by endogeneity bias due to non-random selection into early retirement.

Table **??**

Columns (3) to (6) show our 2SLS results. In the just-identified case (i.e. columns (3) and (4)), we get a much smaller point estimate than the corresponding OLS estimate. Using the minimal (extended) set of control variables yields an IV estimate of 0.0078 (0.0122) compared to the corresponding OLS estimate of 0.0322 (0.0324). Moreover, the IV estimate turns out to be statistically insignificant in both cases. In the over-identified case shown in column (5), we get a point estimate of about 0.016 (standard error of 0.0079), a decrease in magnitude of about 50% compared to the corresponding OLS estimate. Even though the standard error of this estimate is much larger than that in the corresponding OLS regression, the effect remains statistically different from zero at the 5%–level. Adding further controls in column (6) leads to an even larger point estimate of 0.0242. This estimate is slightly larger than that from column (3), but it is still about a quarter smaller than the OLS estimate. The estimated standard error is 0.0084, resulting in statistical significance at the 1%–level. Based on the 2SLS estimate in column (5) and (6), respectively, one additional year spent in early retirement increases the risk of dying before age 67 by 0.0162 (0.0242) percentage points. Evaluated at the sample mean of the dependent variable (equal to 0.18), this means a relative increase in the risk of premature death of about 9% (13.4%). Moreover, the comparison between OLS and 2SLS estimates clearly shows that the OLS estimates are contaminated by reverse causality and tend to be too big, which implies that there is selection into early retirement based on ill health. We chose column (6) of Table **??** as our preferred estimate and refer to it as such in the following.

Furthermore, as proposed by **?**, we compare the 2SLS estimates with those produced by the limited information maximum likelihood (LIML) estimator in the over-identified case.[24] Column (7) corresponds to column (5) except for the fact that the parameters are estimated by LIML rather than 2SLS. LIML estimation yields a point estimate of 0.0144 (standard error of 0.0087). Analogously, column (8) is the LIML estimate that corresponds to the 2SLS estimate shown in column (6). Here we get an estimate of 0.0231 (standard error of 0.0095). In both cases, the LIML estimates are very similar to the 2SLS estimates (though,

[24]The more instruments there are, the more relevant issues with weak instruments eventually become. LIML is less biased than 2SLS in finite samples with many instruments, but also has a higher variance.

as expected, less precise than 2SLS). However, both are still statistically significant at least at the 10%-level. Overall, the comparison between 2SLS and LIML estimates does not suggest that finite-sample bias is a problem (this is not a surprise taking into account that this estimate is based on 17,590 observations).[25]

Our IV-estimates suggest that early exit from the labor force strongly increases mortality.[26] Our preferred estimate of 0.0242 implies that one additional year of early retirement increases the probability of dying before age 67 by as much as 2.4 percentage points. Evaluated at the average probability of dying before the age of 67 (which is equal to 18.0 percent), this corresponds to a relative increase of about 13.4%.

Table **??**

Table **??** shows the corresponding results for female blue-collar workers. The first two columns again report OLS results first. Female workers have a probability of dying before the age of 67 that is increased by about 0.81–0.85 percentage points for each year spent in early retirement. The magnitude of this conditional correlation is roughly a third smaller than the corresponding effect found for their male counterparts, but this is still a non-negligible correlation (in relative terms this is an effect of 11.8%, a magnitude comparable to their male counterparts). However, and in contrast to our results for men, this effect vanishes completely once we apply the 2SLS estimation (see columns (3)–(6)). The 2SLS estimates tell us that female workers' earlier exit from the work force has no impact on mortality. Again, the corresponding LIML estimates (columns (7) and (8)) do not indicate that the 2SLS estimates in columns (5) and (6) suffer from small sample bias since LIML yields estimates very close in magnitude to 2SLS coefficients.[27]

Figure **??**

[25] We further assess the sensitivity of the results to the number of instruments used. We re-estimate the specification in column (6) of Table **??** using only 4 (triennial cohort interactions), 8 (one-and-a-half-annual cohort interactions), and 12 (annual cohort interactions) instruments. The estimates are somewhat smaller ranging from 0.16 to 0.19, but all statistically significant at least at the 10%-level. This is further evidence that our estimate in column (6) of Table **??** is not flawed by a weak and/or many instruments bias.

[26] One might argue that our estimates are be driven by individuals dying while still working, a situation that is in principle possible. Indeed, this may bias our results if death at work occurs with different probability in eligible versus non-eligible districts. To investigate this issue in more detail, we constructed a subsample in which all workers are excluded who die within three months after leaving employment (about 270 male individuals) and then re-estimated our main models. The results remain quantitatively very similar to those presented in Table **??**.

[27] We also assess the sensitivity to the number of instruments used for blue–collar women (see footnote **??**). Irrespective of the number of instruments used, the retirement effect on mortality is very close to and statistically not different from zero.

Our IV strategy in the over-identified case lends itself to a simple graphical representation, which is given by Figure ??. The visualization builds on the equivalence of 2SLS using a set of dummy instruments and GLS on grouped data, where the grouping is done over the dummy instruments (this equivalence is elaborated in ?). Briefly, the left-hand panel of Figure ?? shows the relationship between the probability of being eligible to the REBP on the horizontal axis and the probability of dying before age 67 on the vertical axis (which in turn may be understood as a plot of the reduced form against the first-stage). The figure plots average residuals by year-semester date of birth and eligibility status from a regression of the dependent variable (the endogenous variable, respectively) on cohort fixed-effects, NUTS-3 fixed effects, and controls for past work experience (using corresponding cell sizes as weights). The right-hand panel of Figure ?? shows average residuals from regressions that include additional control variables (corresponding to regression specification shown in column (6) in Table ??). The figure clearly shows that there is a positive causal relation between the number of early retirement years and the probability of premature death (before age 67) for male workers. In contrast, Panel (b) of Figure ?? shows that no such relation exists for female workers.

7 Why Is There Excess Mortality Among Males?

We now explore several potentially important channels that might help explain the observed increased mortality among male blue-collar early retirees. We first show that losses in earnings associated with early retirement are quite small and thus cannot be the main explanation of the evident excess mortality among male workers. Second, we use ancillary information to investigate whether the detrimental impact of early retirement on mortality can be ascribed to specific death causes. Third, we provide some suggestive evidence on the impact of retirement voluntariness on the estimated effect of early retirement on premature death. As the preceding section has shown no causal effect of early retirement on premature death for women, the analysis in this section is confined to male blue-collar workers only.

7.1 Loss of Earnings

Earnings losses may contribute to an explanation of excess mortality among early retirees. To check the relevance of this channel, we first estimate the reduction in permanent earnings for individuals aged 50 or older if they retire one year earlier. We find that the reduction in permanent income for individuals aged 50 or older is only about 2.5 percent.[28] Taken at face value, the estimated OLS estimate of -0.10 for the effect of average earnings before the age of 50 on mortality would imply that we expect an increase in the probability of dying before age 67 of about 0.25 percentage points.[29] We therefore conclude that at most 10% of our preferred estimate of the causal effect of retirement on premature mortality can be explained by the reduction in permanent income associated with early retirement.[30]

The income channel in our case is much less important than that in a recent study by **?**, who find that this specific channel accounts for as much as 50%–75% of the overall effect of involuntary job loss on mortality in the US. The fact that there is compulsory and universal health insurance coverage in Austria reconciles this difference, however. Moreover, the reduction in income after retiring early is mitigated by relatively high income replacement rates in the Austrian pension system. In sum, we conclude that earnings losses associated with early retirement are too small to provide a credible explanation for our finding of excess mortality among males.

7.2 Changes in Health-Related Behavior

This section investigates whether changes in individuals' health-related behavior (such as excessive drinking and/or smoking, an unhealthy diet, and a low level of physical activity) can explain the increased risk of premature death among male blue-collar workers. In fact, there is considerable – though not conclusive – medical research on the relation between retirement and smoking (e.g. **???**), retirement and (excessive) alcohol use (e.g. **??**), as well as between retirement and changes in diet and physical activity (e.g. **?????**). **?** find that involuntary job loss among older workers increases both the risk of heavy drinking and an unhealthy diet (i.e. a high body mass index), though only among individuals already pursuing unhealthy behaviors.

We shed light on this channel by investigating whether early retirement increases the risk of specific causes of death that are directly or indirectly attributable to changes in health related behavior.[31] For this analysis we additionally rely on individual data on mortality

[28]See Table **??** in the appendix. Note further that the volatility of income is a minor issue only in our context because income streams are constant as soon as an individual draws pension benefits.

[29]The OLS estimate is taken from column (2) of Table **??**. Based on this estimate, a reduction in permanent income of 2.5% implies an increase in the probability of death before age 67 of approximately $-(-0.010/100)\cdot 0.025 = 0.0025$. This figure is likely to overestimate the effect of earnings on mortality because the OLS estimate of the effect of earnings on premature death is arguably biased upward.

[30]This number results from dividing the estimated effect of the reduction in permanent income of 0.0025 by our preferred 2SLS estimate of 0.0242, taken from column (6) of Table **??**.

[31]This is similar to **?** who use cause-specific mortality rates to investigate excess mortality among World War II and Korean War Veterans in the US. They find that military-induced smoking drives most of the observed excess mortality.

provided by Statistics Austria which contains the universe of death cases in Austria. It contains information about the detailed causes of death according to the 9th and 10th revision of the International Classification of Diseases and Related Health Problems (ICD-9, ICD-10). While information on causes of death from Statistics Austria cannot be linked directly with the ASSD (there is no common person identifier), it is nonetheless possible to exactly match information on the basis of four characteristics that are available in both data sets: year and month of birth, year and month of death, NUTS-3 unit, and eligible/non-eligible district. It turns out that cause of death can be unambiguously matched for 2,454 observations (among those 3,172 blue-collar workers in our sample who died before age 67) which implies a matching rate of 77.4%. For 147 observations the matching is ambiguous and for 571 observations in the ASSD there is no corresponding observation in the data from Statistics Austria.

In the following we concentrate on the following causes of death: (i) Alcohol-related causes, (ii) ischemic heart diseases, (iii) smoking-related causes (other than ischemic heart disease), (iv) vehicle accidents, and (v) other causes. The assignment of particular diseases to "alcohol-related causes" and "smoking-related causes" is based on the procedure applied by the U.S. Department of Health and Human Services (Table **??** in the appendix details this classification procedure). We assign deaths to alcohol-related and smoking-related causes if at least 40% of deaths in an ICD category are attributable to excessive consumption of alcohol or smoking, respectively. "Ischemic heart diseases" (mostly heart attacks) are also highly attributable to smoking, and, in addition, to overweight and obesity which are related to an unhealthy diet and a low level of physical activity.[32] "Vehicle accidents" are also to a non-negligible extent attributable to alcohol abuse.[33] "Other causes" are the residual category which contains all remaining death causes as well as those deaths for which the cause of death is unknown due to the failure to link the causes of death statistics with the ASSD.

Table **??**

[32]Ischemic heart diseases are indicated by ICD-9 codes 410-414, 429.2 and ICD-10 codes I20-I25. According to the Smoking-Attributable Mortality, Morbidity, and Economic Costs (SAMMEC) application provided by the Centers for Disease Control and Prevention (CDC), one of the major operating components of the U.S. Department of Health and Human Services, the proportion of deaths due to ischemic heart diseases for US males aged 35–64 (65 and above) in the year 2001 attributable to smoking amounts to 40% (15%). For obesity see the study by the **?**. There is a broad consensus in the medical literature that there are only a few main risk factors associated with cardiovascular infarction and coronary heart disease. Among the most important risk factors are smoking, hypertension, diabetes, obesity, and psychosocial factors, while a healthy diet (e.g. eating fruit and vegetables) and regular physical exercise appear to be protective (**???**)

[33]Vehicle accidents are indicated by ICD-9 codes 800-848 and ICD-10 codes V00-V99. According to the U.S. National Highway Traffic Safety Administration (2002) 28% (13%) of all motor-vehicle accidents of US males aged 55-64 (65 and older) are related to alcohol.

The results for the cause-specific mortality are displayed in Table **??**. Because the results without and with the inclusion of additional controls are very similar, the table only reports the results with additional controls. Column (1) repeats the estimate of column (6) of Table **??** that shows that premature death (before age 67) increases by 2.4 percentage points for each additional year spent in early retirement. The causes of death displayed in the table are exhaustive and mutually exclusive, thus the estimates from columns (2) to (6) add up to the overall estimate from column (1) (and the mortality rates for the particular death causes sum up to the total mortality rate). Column (2) shows that one year spent in early retirement increases the probability of dying from alcohol-related diseases by 0.71 percentage points. In other words, the risk of dying from diseases (partially) caused by excessive alcohol consumption contributes 29% (=0.0071/0.0242) to the overall effect. Column (3) shows that that the risk of dying before age 67 due to ischemic heart diseases is increased by 0.94 percentage points, and therefore ischemic heart diseases account for 39% (=0.0094/0.0242) of the total effect. The contribution of vehicle injuries (column (5)) amounts to another 0.24 percentage points (or 10% in terms of the total effect). Interestingly, the risk of dying from smoking-related diseases (other than ischemic heart diseases) does not significantly increase due to early retirement (column (4)). The risk of dying from other causes is not significantly affected by early retirement (column (6)).[34] Taken together, alcohol-related causes, ischemic heart diseases, and vehicle injuries account for as much as 78% of the overall causal effect of early retirement. This implies a strong concentration of excess mortality among blue-collar males to three causes (which account for 24% of all deaths in our male sample).[35]

Clearly, not all those deaths can directly be attributed to underlying changes in health-related behavior. For instance, only 40% of all deaths caused by portal hypertension can directly be attributed to alcohol abuse (Table **??** shows the respective attributable fractions). To account more directly for excessive alcohol consumption and smoking as causes of excess mortality, we multiply the estimated contribution of each cause to the overall effect by their respective fraction of these deaths that are attributable to alcohol consumption and smoking behavior. The fractions we use for this calculation are as follows: 58% of diseases classified

[34] We also investigate several subsets of the remaining causes in Table **??**. This Table shows analogous results for the following subcategories: Alcohol-unrelated digestive system diseases, non-ischemic heart diseases, smoking-unrelated respiratory diseases, smoking-unrelated cancer, self-inflicted injuries, other injuries, cerebrovascular diseases, and all remaining causes. It turns out that none of those cause specific deaths are affected by early retirement and thus do not contribute to the overall impact of early retirement on premature death. This strongly supports the notion that the alcohol-related causes, ischemic heart diseases, and vehicle injuries are the driving force for the detrimental impact of early retirement on premature death.

[35] We also did the analysis of the cause-specific mortality for blue-collar women. Results show that none of the causes-of-death categories were governed by the number of years spent in early retirement. This strongly supports the result that blue–collar women's health seems not to be affected by their retirement decision.

as "alcohol-related causes" are directly attributable to excessive alcohol consumption;[36] 34% of ischemic heart diseases are caused by smoking;[37] and roughly 26% of vehicle injuries are caused by alcohol consumption. This suggests that the contribution of smoking and excessive alcohol consumption amounts to as much as 32.4% ($= (0.58 \cdot 0.0071 + 0.34 \cdot 0.0091 + 0.26 \cdot 0.0024)/0.0242$) of total excess mortality. Clearly, unhealthy practices are not only confined to smoking and drinking but also to other dimensions such as unhealthy diet and lack of physical activity. Unhealthy diet and lack of physical activity result in overweight and obesity which are themselves important underlying reasons for ischemic heart diseases (see **?**). Hence the contribution of unhealthy behaviors to excess mortality among blue-collar males is likely to be much higher than the 32.4% we derived from smoking- and drinking-attributable causes only.

7.3 Voluntary or Involuntary Retirement?

Another hypothesis is related to firing decisions of firms. Since the REBP mitigated economic hardships associated with unemployment of older workers, the implementation of this program made it easier for firms to release older workers. If these firm decisions underlie the estimated treatment effects, we should see a larger effect among released workers as opposed those who voluntarily quit their jobs (**??**).[38]

While it is not possible to directly distinguish between quits and layoffs in our data, we can exploit the institutional particularity that there are sharp discontinuities in eligibility for severance pay in Austria. After 3 years of continuous work history with the same employer, a worker becomes eligible for severance payments. Severance payments amount to twice the monthly salary and increase to three salaries after 5 years, to four after 10 years, to six after 15 years, to nine after 20 years, and to twelve monthly salaries after 25 years of continuous work history with the same employer. Given that the financial stakes involved are quite high, one might argue that a comparison of workers just above and below any given threshold may

[36]This corresponds to the weighted average of the attributable fractions (the weights are the share of individuals dying of the specific alcohol-related diseases listed in Table **??**).

[37]34% corresponds to the weighted average of the age dependent smoking attributable fractions regarding ischemic heart diseases (see footnote **??**; the weights are the share of individuals dying of ischemic heart diseases before and after age 65).

[38]Of course, there are other potential sources of treatment effect heterogeneity. One especially interesting dimension is workers' ex-ante health status because it is easily imaginable that mortality effects be predominantly driven by workers with weak ex-ante health. Appendix table **??** sheds light on this issue. The mortality effect is strong and highly significant ex-ante among workers who are unhealthier. This suggests that early retirement causes premature death by adding to already existing health problems. In contrast, we see that the mortality effect is small and insignificant among workers who are ex-ante healthier. **?** report similar results regarding the role of behavioral predisposition for the risk of excessive drinking and obesity following involuntary job loss among elderly workers.

be informative about the degree of retirement voluntariness. More specifically, it may be reasonable to assume that the probability of a voluntary quit is higher, ceteris paribus, if a worker has just crossed any of the tenure thresholds above, and thus received severance pay, compared to the situation that he just failed to cross the threshold and thus had to forego (increased) severance pay. Before the threshold around 10, for example, the worker only gets three months of severance pay and might be sorely tempted to wait around to get six. If he goes before ten years, he does not lose severance pay, but receives a reduced amount.

Table **??**

Table **??** shows the resulting estimates using two different subsamples. The first (second) subsample contains only male workers with job tenure in a range of up to 6 (12) months around any tenure threshold relevant for severance pay (i.e. 3, 5, 10, 15, 20, or 25 years of job tenure). We then re-estimate, for each of the two subsamples, our main models of columns (5) and (6) of Table **??** for those workers below or above any existing tenure threshold relevant for severance pay. The first four columns show estimates based on the subsample including only workers with job tenure within 6 months of any threshold. The first column shows a significant effect of retirement on premature death for workers below the tenure threshold, while the third column only shows a small, and statistically, insignificant effect for workers just above the tenure threshold. A similar result is obtained if additional controls are used (compare columns (2) and (4)) and if the subsample considered includes workers within 12 months of any tenure threshold (remaining columns of Table **??**).

Even though we cannot directly distinguish between voluntary and involuntary entry into early retirement, we find suggestive evidence that retirement voluntariness may indeed be related to the health effects of early retirement and the potentially underlying behavior. Early retirement followed by voluntary quits seem to be unrelated to mortality, while early retirement caused by involuntary layoffs is so.

8 Conclusions

This paper estimates the causal effect of early retirement on mortality for blue-collar workers. 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) blue-collar workers retired on average 9 (12) months earlier than their non-eligible colleagues. This provides us with an empirical design which allows us to identify the causal impact of early retirement on mortality using instrumental variable techniques.

For male blue-collar workers, we find that early retirement age causes a significant increase in the risk of premature death (death before age 67). The effect for males is not only statistically significant but also quantitatively important. One additional year in early retirement causes an increase in the risk of premature death of 2.4 percentage points (a relative increase of 13.4%). Our results suggest that lower earnings of early retirees cannot explain male excess mortality because these losses are quantitatively too small to have a substantial impact on mortality. In contrast, we find that changes in health-related behavior (in particular, smoking and excessive alcohol consumption) contribute to a large extent to excess mortality. Male excess mortality is concentrated among three causes of deaths: (i) ischemic heart diseases (mostly heart attacks), (ii) diseases related to excessive alcohol consumption, and (iii) vehicle injuries. These three causes of death account for 78 percent of the causal retirement effect, while accounting for only 24 percent of all deaths in the sample. Some simple calculations suggest that 32.4 percent of the causal retirement effect is directly attributable to smoking and excessive alcohol consumption. Our empirical results also suggest that early retirement following an involuntary job loss is likely to cause excess mortality among blue-collar males, while retirement after a voluntary quit does not.

While the retirement-effect on mortality is highly significant and quantitatively important for males, we do not find such an effect for females. There are several reasons why male but not female workers suffer from higher mortality following early retirement (e.g. women may be more able to cope with major life events, they may be more health-conscious and adopt less unhealthy behaviors; they may be more active due to their higher involvement in household activities; and they may suffer less from a loss of social status). In line with prior expectations and previous evidence, we also find that IV-estimates are smaller than the simple OLS estimate, both for men and for women. This is consistent with negative health selection into retirement and underlines the importance of a proper identification strategy when estimating the causal impact on mortality.

From a policy perspective, our results suggest that policies fostering employment of older workers may generate a double dividend. They not only improve government budgets, they also increase individuals' welfare by prolonging their lives, particularly for males. Labor market policies should therefore incentivize both firms to keep older workers in employment and workers to abstain from premature retirement. Preventive health policies that support individuals in adopting healthy, or avoiding unhealthy, behaviours may have disproportionately positive health consequences for older males who are about to permanently withdraw from the labor market.

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Table 1: Summary statistics

	Men				Women			
	Eligible districts		Non-eligible districts		Eligible districts		Non-eligible districts	
Retirement age	55.9411	(2.9171)	56.6912	(2.9046)	52.9410	(2.2170)	54.0937	(2.1722)
Retirement years before statutory retirement age	9.0589	(2.9171)	8.3088	(2.9046)	7.0590	(2.2170)	5.9063	(2.1722)
Age on June 1, 1988	52.8384	(3.7220)	52.7136	(3.7584)	50.0198	(2.1792)	49.9301	(2.1420)
Work experience before age 50 (in years)								
Within the last year	0.9623	(0.1102)	0.9490	(0.1217)	0.9598	(0.1303)	0.9585	(0.1232)
Within the last 2 years	1.9290	(0.1727)	1.9038	(0.1949)	1.9362	(0.1640)	1.9258	(0.1821)
Within the last 5 years	4.8163	(0.4108)	4.7572	(0.4616)	4.8302	(0.3794)	4.7856	(0.4817)
Within the last 10 years	9.6263	(0.7444)	9.5108	(0.8410)	9.5507	(0.8224)	9.4492	(1.0747)
Within the last 25 years	23.8921	(1.7633)	23.4873	(2.0540)	21.1592	(3.1414)	20.6869	(3.2646)
Number of sick leave days								
Within the last year	4.6800	(21.9293)	5.3438	(23.2272)	6.4036	(25.8762)	7.6630	(31.6948)
Within the last 2 years (in days)	7.7357	(29.1986)	8.4643	(30.2857)	10.1690	(33.7910)	11.1199	(37.7866)
Within the last 5 years (in days)	15.9685	(43.5918)	17.8007	(47.6949)	19.7249	(50.2334)	19.8257	(50.7874)
Within the last 10 years (in days)	38.3429	(70.4117)	40.1225	(74.8618)	31.8233	(64.5720)	31.9317	(63.8951)
Log(average yearly earnings (age 43-49))	9.9181	(0.2968)	9.8219	(0.2995)	9.4013	(0.3885)	9.3497	(0.4001)
Log(std. dev. of yearly earnings (age 43-49))	7.3430	(0.6966)	7.3534	(0.7363)	7.0143	(0.8072)	6.9082	(0.8533)
Industry affiliation (dummy indicators)								
Agriculture, fishery, forestry	0.0576	(0.2330)	0.0898	(0.2860)	0.0283	(0.1658)	0.0741	(0.2620)
Electricity, gas, heat, and water supply	0.0078	(0.0882)	0.0102	(0.1007)	0.0032	(0.0566)	0.0023	(0.0481)
Mining	0.0820	(0.2743)	0.0519	(0.2218)	0.0084	(0.0911)	0.0052	(0.0720)
Manufacturing	0.5793	(0.4937)	0.4468	(0.4972)	0.7404	(0.4386)	0.6306	(0.4828)
Construction	0.1600	(0.3666)	0.2414	(0.4280)	0.0174	(0.1306)	0.0156	(0.1241)
Retail, wholesale, stockkeeping	0.0571	(0.2321)	0.0851	(0.2790)	0.0514	(0.2209)	0.0811	(0.2730)
Tourism	0.0056	(0.0745)	0.0087	(0.0930)	0.0450	(0.2073)	0.0718	(0.2582)
Transport	0.0327	(0.1778)	0.0441	(0.2052)	0.0019	(0.0439)	0.0058	(0.0759)
Financial services, insurance	0.0081	(0.0895)	0.0102	(0.1007)	0.0135	(0.1154)	0.0324	(0.1772)
Personal hygiene	0.0030	(0.0544)	0.0031	(0.0552)	0.0212	(0.1441)	0.0307	(0.1725)
Arts, entertainment, sports	0.0010	(0.0308)	0.0001	(0.0104)	0.0013	(0.0358)	0.0012	(0.0340)
Health care	0.0051	(0.0713)	0.0064	(0.0800)	0.0617	(0.2407)	0.0382	(0.1918)
Educational system, research industry	0.0007	(0.0267)	0.0015	(0.0390)	0.0045	(0.0669)	0.0093	(0.0958)
Domestic servicing and maintenance	0.0001	(0.0109)	0.0005	(0.0233)	0.0019	(0.0439)	0.0017	(0.0417)
NUTS-3 units (dummy indicators)								
Nordburgenland	0.0259	(0.1588)	0.0750	(0.2634)	0.0437	(0.2045)	0.1164	(0.3208)
Mostviertel-Eisenwurzen	0.1290	(0.3352)	0.1403	(0.3474)	0.1298	(0.3362)	0.1060	(0.3079)
Waldviertel	0.1936	(0.3952)	0.1142	(0.3180)	0.3927	(0.4885)	0.1285	(0.3348)
Unterkaernten	0.0742	(0.2622)	0.1361	(0.3429)	0.0476	(0.2129)	0.1065	(0.3086)
Oststeiermark	0.1013	(0.3018)	0.1343	(0.3410)	0.0598	(0.2371)	0.1419	(0.3490)
West- und Suedsteiermark	0.1294	(0.3356)	0.1091	(0.3118)	0.0411	(0.1987)	0.1042	(0.3056)
Innviertel	0.0544	(0.2268)	0.2178	(0.4127)	0.0566	(0.2311)	0.2021	(0.4017)
Steyr-Kirchdorf	0.2922	(0.4548)	0.0732	(0.2604)	0.2288	(0.4202)	0.0944	(0.2924)
Number of observations	8,419		9,171		1,556		1,727	

Notes: Sample means and standard deviations (in parentheses).

Table 2: Standardized mortality rates, 1978-1984

	Men				Women			
Age group	<45	45-65	65-75	>75	<45	45-65	65-75	>75
Mean	5.0557	7.1355	8.3811	9.5872	4.2331	6.3441	7.7217	9.2952
Standard deviation	0.1491	0.1222	0.0856	0.0630	0.1494	0.1130	0.0975	0.0828
Eligible district	0.0432	−0.0489	−0.0045	0.0008	−0.0370	−0.0427	−0.0078	0.0069
	(0.0417)	(0.0318)	(0.0220)	(0.0139)	(0.0543)	(0.0447)	(0.0233)	(0.0215)
Number of Districts	93	93	93	93	93	93	93	93
R^2	0.0159	0.0303	0.0005	0.0000	0.0116	0.0270	0.0012	0.0013
p-value (F-statistic)	0.3022	0.1272	0.8379	0.9554	0.4968	0.3417	0.7380	0.7502

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. The dependent variable is the log of the number of deceases per 100,000 residents. All regressions are weighted by districts' resident population in 1981. Standardized mortality rates account for variation in age distribution across regions. Based on data from Statistics Austria.

Table 3: Reduced form effect on sick leave days during the last k year(s) before age 50

	Men				Women			
Sick leave days during	last year	last 2 years	last 5 years	last 10 years	last year	last 2 years	last 5 years	last 10 years
Mean	5.0261	8.1156	16.9238	39.2707	7.0661	10.6692	19.7779	31.8803
Standard deviation	22.6170	29.7718	45.7849	72.7692	29.0850	35.9460	50.5180	64.2070
Eligible district	−0.2817	−0.0266	−0.9548	−0.8047	−1.4623	−1.0209	0.0717	−0.6694
	(0.3532)	(0.4271)	(0.6945)	(1.1337)	(0.9940)	(1.2533)	(1.7075)	(2.1933)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	3,283	3,283	3,283	3,283
R^2	0.2742	0.2721	0.2388	0.2576	0.3655	0.3800	0.3291	0.2719

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, and employers' industry affiliation (14 industries).

Table 4: First-stage results

	Retirement years before the statutory retirement age							
	Men				Women			
Mean	8.6678	8.6678	8.6678	8.6678	6.4526	6.4526	6.4526	6.4526
Standard deviation	2.9346	2.9346	2.9346	2.9346	2.2675	2.2675	2.2675	2.2675
Eligible district	0.7100***	0.5895***			1.0104***	0.9399***		
Eligible district · 1929h2			0.3652	0.0900				
Eligible district · 1930h1			0.1658	−0.0254				
Eligible district · 1930h2			0.0879	−0.1167				
Eligible district · 1931h1			0.0307	−0.0617				
Eligible district · 1931h2			0.7390***	0.5466***				
Eligible district · 1932h1			0.6021***	0.4285**				
Eligible district · 1932h2			0.8066***	0.6584***				
Eligible district · 1933h1			0.6284**	0.4503**				
Eligible district · 1933h2			0.3868	0.2533				
Eligible district · 1934h1			0.6323***	0.4812**				
Eligible district · 1934h2			0.9923***	0.8322***			0.6717***	0.5006**
Eligible district · 1935h1			0.9849***	0.7802***			0.3345	0.3267
Eligible district · 1935h2			0.7494***	0.5207**			0.4140	0.2519
Eligible district · 1936h1			1.2162***	1.1637***			0.7349**	0.4873
Eligible district · 1936h2			0.6622***	0.6336***			0.7092***	0.6789***
Eligible district · 1937h1			1.0500***	1.0469***			1.1734***	0.9597***
Eligible district · 1937h2			1.3570***	1.2406***			1.2091***	1.0138***
Eligible district · 1938h1			0.9968***	0.9708***			1.1244***	0.9275***
Eligible district · 1938h2			0.5397*	0.3333			0.9883***	1.1578***
Eligible district · 1939h1			1.1068***	0.9968***			0.8349***	0.8560***
Eligible district · 1939h2			0.7041***	0.5939***			1.6288***	1.6173***
Eligible district · 1940h1			0.8803***	0.9863***			1.1276***	1.1186***
Eligible district · 1940h2			0.9150***	0.8897***			1.3271***	1.2925***
Eligible district · 1941h1			0.9651***	0.9921***			1.2916***	1.3071***
Eligible district · 1941h2			0.6944**	0.5212*			1.1064***	1.0060***
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	17,590	17,590	17,590	17,590	3,283	3,283	3,283	3,283
R^2	0.1326	0.1980	0.1357	0.2021	0.1721	0.2489	0.1779	0.2558
First Stage F-statistic (Instruments)	137.0057	116.8295	9.0148	9.3887	109.2957	116.9665	10.7651	12.2499

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table 5: Second stage results, men

	Death before age 67							
	OLS		2SLS				LIML	
Mean	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803
Standard deviation	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845
Retirement years before age 65	0.0322***	0.0324***	0.0078	0.0122	0.0162**	0.0242***	0.0144*	0.0231**
	(0.0012)	(0.0012)	(0.0090)	(0.0109)	(0.0079)	(0.0084)	(0.0087)	(0.0095)
Log(average yearly earnings (age 43-49))		−0.1001***		−0.1003***		−0.1001***		−0.1001***
		(0.0141)		(0.0142)		(0.0140)		(0.0141)
Log(std. dev. of yearly earnings (age 43-49))		0.0172***		0.0174***		0.0173***		0.0173***
		(0.0045)		(0.0045)		(0.0045)		(0.0045)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes
Instrument interacted with year-semester of birth	–	–	No	No	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590
R^2	0.0686	0.0744	0.0380	0.0551	0.0553	0.0712	0.0522	0.0703

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table 6: Second stage results, women

	Death before age 67							
	OLS		2SLS				LIML	
Mean	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719
Standard deviation	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583
Retirement years before age 60	0.0081***	0.0085***	−0.0051	−0.0016	−0.0032	0.0002	−0.0038	−0.0003
	(0.0023)	(0.0025)	(0.0085)	(0.0093)	(0.0077)	(0.0084)	(0.0081)	(0.0088)
Log(average yearly earnings (age 43-49))		0.0152		0.0212		0.0202		0.0205
		(0.0144)		(0.0154)		(0.0151)		(0.0152)
Log(std. dev. of yearly earnings (age 43-49))		−0.0016		−0.0026		−0.0024		−0.0025
		(0.0062)		(0.0061)		(0.0061)		(0.0061)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes
Instrument interacted with year-semester of birth	–	–	No	No	Yes	Yes	Yes	Yes
Number of observations	3,283	3,283	3,283	3,283	3,283	3,283	3,283	3,283
R^2	0.0184	0.0280	0.0069	0.0219	0.0099	0.0238	0.0090	0.0234

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table 7: Causes of death, men only

	Death before age 67	Alcohol-related causes	Ischemic heart disease	Other smoking-related causes	Vehicle injury	Other causes
Mean	0.1803	0.0138	0.0271	0.0271	0.0023	0.1101
Standard deviation	0.3845	0.1165	0.1623	0.1623	0.0482	0.3130
Retirement years before age 65	0.0242***	0.0071***	0.0094***	−0.0027	0.0024**	0.0079
	(0.0084)	(0.0026)	(0.0035)	(0.0036)	(0.0010)	(0.0071)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590
R^2	0.0712	0.0118	0.0097	.	.	0.0292

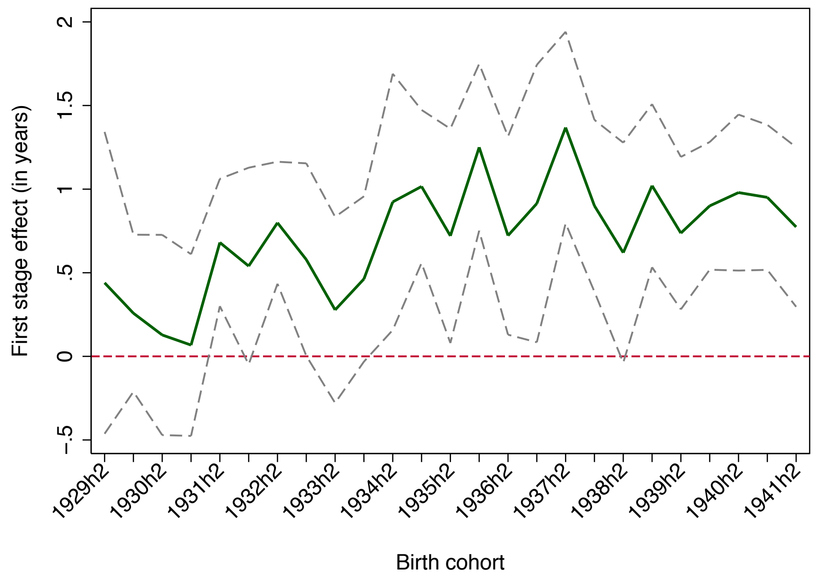
Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. A missing R^2 denotes a negative R-squared. The causes of death are classified by means of ICD-9 and ICD-10. See Table **??** for the ICD codes of *alcohol- and smoking-related causes*. *Ischemic heart diseases* include the the ICD codes 410-414, 429.2 (ICD-9) and I20-I25 (ICD-10). *Vehicle injuries* include the ICD codes 800-848 (ICD-9) and V00-V99 (ICD-10). *Other causes* include all remaining causes as well as those causes that are unknown due to failure of the match between data on causes of death and the ASSD.

Table 8: Retirement (in)voluntariness, individuals close the severance-pay threshold

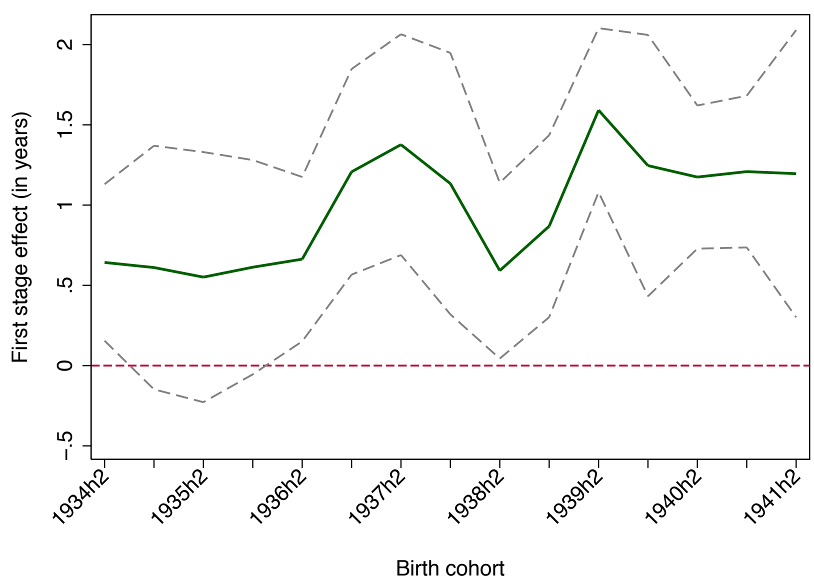
	Death before 67							
Window around threshold	1 to 6 months				1 to 12 months			
	Below threshold		Above threshold		Below threshold		Above threshold	
Mean	0.1862	0.1862	0.1811	0.1811	0.1899	0.1899	0.1622	0.1622
Standard deviation	0.3894	0.3894	0.3852	0.3852	0.3923	0.3923	0.3687	0.3687
Retirement years before age 65	0.0225	0.0334*	0.0051	0.0104	0.0194	0.0314**	0.0117	0.0190
	(0.0168)	(0.0173)	(0.0154)	(0.0169)	(0.0140)	(0.0159)	(0.0129)	(0.0149)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	1,332	1,332	1,458	1,458	2,938	2,938	2,866	2,866
R^2	0.0890	0.1176	0.0614	0.1100	0.0605	0.0823	0.0505	0.0779

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Figure 1: First-stage results



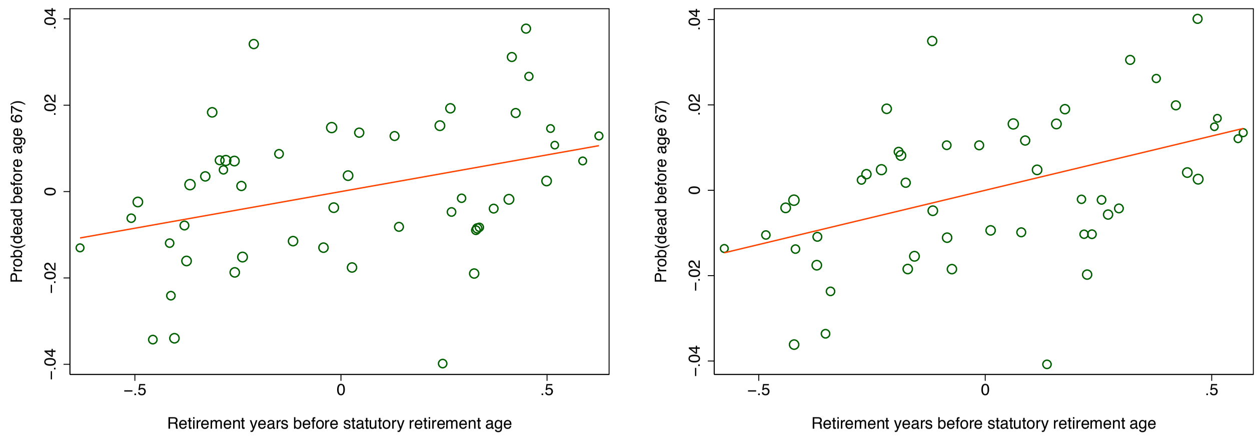
(a) Men



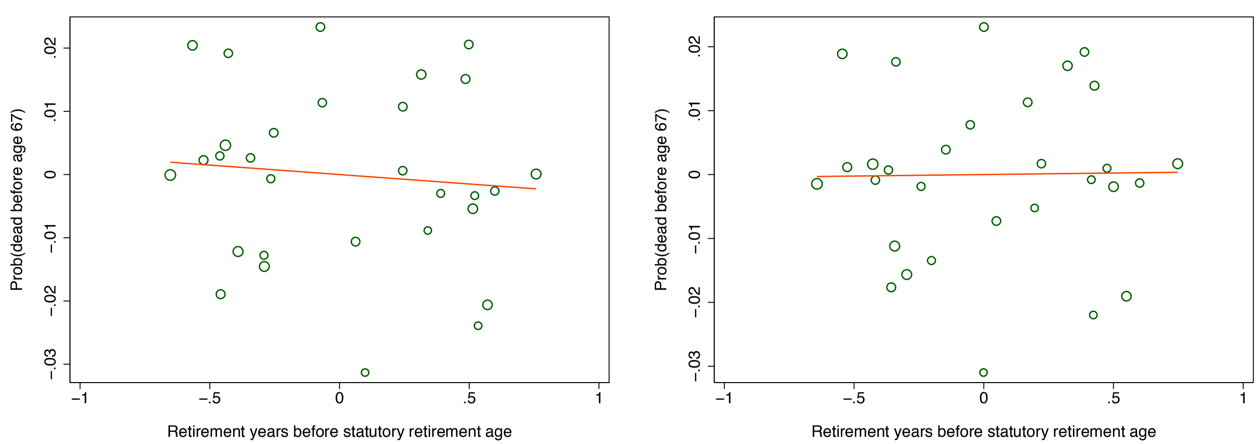
(b) Women

Notes: The figures plot the difference in the retirement age between eligible and non-eligible districts by year-semester birth-cohort in the sample of male and female workers, respectively. Dashed lines show 95% confidence bands.

Figure 2: Visual representation of IV-estimates



(a) Men



(b) Women

Notes: The figures plot the probability of premature death (before age 67) against the number of retirement years. More specifically, the figures plot average residuals from a regression of the probability of death before age 67 (vertical axis) and the number of retirement years before the statutory retirement age (horizontal axis) on cohort fixed effects, NUTS-3 fixed effects and controls for past work experience (left-hand figures). The figures on the right-hand side plot residuals from regressions that additionally include industry fixed effects as well as controls for sick-leave days and earnings.

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A Additional Tables and Figures

Table A.1: First stage results for cohorts ineligible to the REBP

	Men	Women
Mean	8.2685	5.3875
Standard deviation	3.4948	2.1897
Eligible district	−0.0071	0.0791
	(0.1185)	(0.0759)
Cohort fixed-effects	Yes	Yes
Experience	Yes	Yes
NUTS fixed-effects	Yes	Yes
Additional controls	Yes	Yes
Number of Observations	3,444	3,005
R^2	0.2397	0.1876

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. Considered birth cohorts are 08.1943–04.1947 for men and 08.1943–04.1952 for women. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table A.2: The association between earnings from age 50 onwards and early retirement

	Earnings from age 50 onwards	
Mean	9.7237	9.7237
Standard deviation	0.3540	0.3540
Retirement years before age 65	-0.0222^{***}	-0.0250^{***}
	(0.0012)	(0.0010)
Cohort fixed-effects	Yes	Yes
Experience	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes
Additional controls	No	Yes
Number of observations	17,590	17,590
R^2	0.3141	0.6223

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. Mean earnings derived from work income, unemployment benefits (assuming a replacement rate of 40%), and disability and old-age retirement (assuming a replacement rate of 80%) are estimated up to individuals' death date (right-censored death dates (July 1, 2009) are replaced by the expected death date based on workers' expected life-expectancy (taken from mortality tables by Statistics Austria). There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table A.3: Classification of alcohol- and smoking-related causes

Category	Included diseases[a]	ICD-9[b]	ICD-10[b]	Attributable fraction (in %)[c]
Alcohol-related causes	Chronic conditions:			
	Alcoholic psychosis	291	F10.3-F10.9	100
	Alcohol abuse	305.0, 303.0	F10.0, F10.1	100
	Alcohol dependence syndrome	303.9	F10.2	100
	Alcohol polyneuropathy	357.5	G62.1	100
	Degeneration of nervous system due to alcohol	n/a	G31.2	100
	Alcoholic myopathy	n/a	G72.1	100
	Alcohol cardiomyopathy	425.5	I42.6	100
	Alcoholic gastritis	535.3	K29.2	100
	Alcoholic liver disease	571.0-571.3	K70-K70.4, K70.9	100
	Alcohol-induced chronic pancreatitis	n/a	K86.0	100
	Liver cirrhosis, unspecified	571.5-571.9	K74.3-K74.6, K76.0, K76.9	40
	Esophageal cancer	150	C15	40
	Chronic pancreatitis	577.1	K86.1	84
	Portal hypertension	572.3	K76.6	40
	Gastroesophageal hemorrhage	530.7	K22.6	47
	Acute conditions:			
	Alcohol poisoning	980.0-980.1, E860.0-E860.1, E860.2, E860.9	X45,Y15, T51.0-T51.1, T51.9	100
	Suicide by and exposure to alcohol	n/a	X65	100
	Excessive blood level of alcohol	790.3	R78.0	100
Smoking-related causes	Malignant Neoplasms:			
	Lip, Oral Cavity, Pharynx	140-149	C00-C14	71
	Esophagus	150	C15	72
	Larynx	161	C32	82
	Trachea, Lung, Bronchus	162	C33-C34	87
	Urinary Bladder	188	C67	46
	Cardiovascular Diseases:			
	Aortic Aneurysm	441	I71	64
	Respiratory Diseases:			
	Bronchitis, Emphysema	490-492	J40-J42, J43	91
	Chronic Airway Obstruction	496	J44	81

Notes: [a] The choice of included diseases for alcohol-related causes is based on the Alcohol-Related Disease Impact (ARDI) software provided by the Centers for Disease Control and Prevention (CDC), one of the major operating components of the U.S. Department of Health and Human Services (HHS). We restrict alcohol-related diseases to those with alcohol-attributable mortality fractions of at least 40% (fractions of at least 40% are considered "high causation" diseases by the HHS). The alcohol-attributable mortality fractions refer to 5-year average annual estimates of health impacts based on the years 2001-2005 for US males. The choice of included diseases for smoking-related causes is based on the Smoking-Attributable Mortality, Morbidity, and Economic Costs (SAMMEC) application also provided by the CDC. Again, we restrict smoking-related diseases to those with smoking-attributable mortality fractions of at least 40%. The smoking-attributable mortality fractions refer to US males aged 65 and above in the year 2001. [b] ICD (International Classification of Diseases) is the international standard diagnostic classification for all general epidemiological, many health management purposes and clinical use. [c] Alcohol- or smoking-attributable fractions are defined as the proportion of deaths from the listed causes that are due to alcohol or smoking, repsectively (these fractions are derived from meta-studies conducted by the HHS).

Table A.4: Causes of death, disaggregation of *other causes* (see column (6) of table **??**), men only

	Other causes	Alcohol-unrelated digestive system diseases	Non-ischemic heart diseases	Smoking-unrelated respiratory diseases	Smoking-unrelated cancer	Self-inflicted injuries	Other injuries	Cerebro-vascular diseases	All remaining causes
Mean	0.1101	0.0020	0.0125	0.0019	0.0308	0.0049	0.0036	0.0067	0.0476
Standard deviation	0.3130	0.0446	0.1111	0.0439	0.1728	0.0702	0.0602	0.0813	0.2130
Retirement years before age 65	0.0079	−0.0011	0.0039	0.0000	0.0028	0.0020	−0.0003	0.0014	−0.0009
	(0.0071)	(0.0009)	(0.0024)	(0.0009)	(0.0035)	(0.0017)	(0.0016)	(0.0019)	(0.0048)
Cohort fixed-effects (biannually)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590
R^2	0.0292	.	0.0075	0.0062	0.0059	0.0058	0.0038	0.0066	0.0102

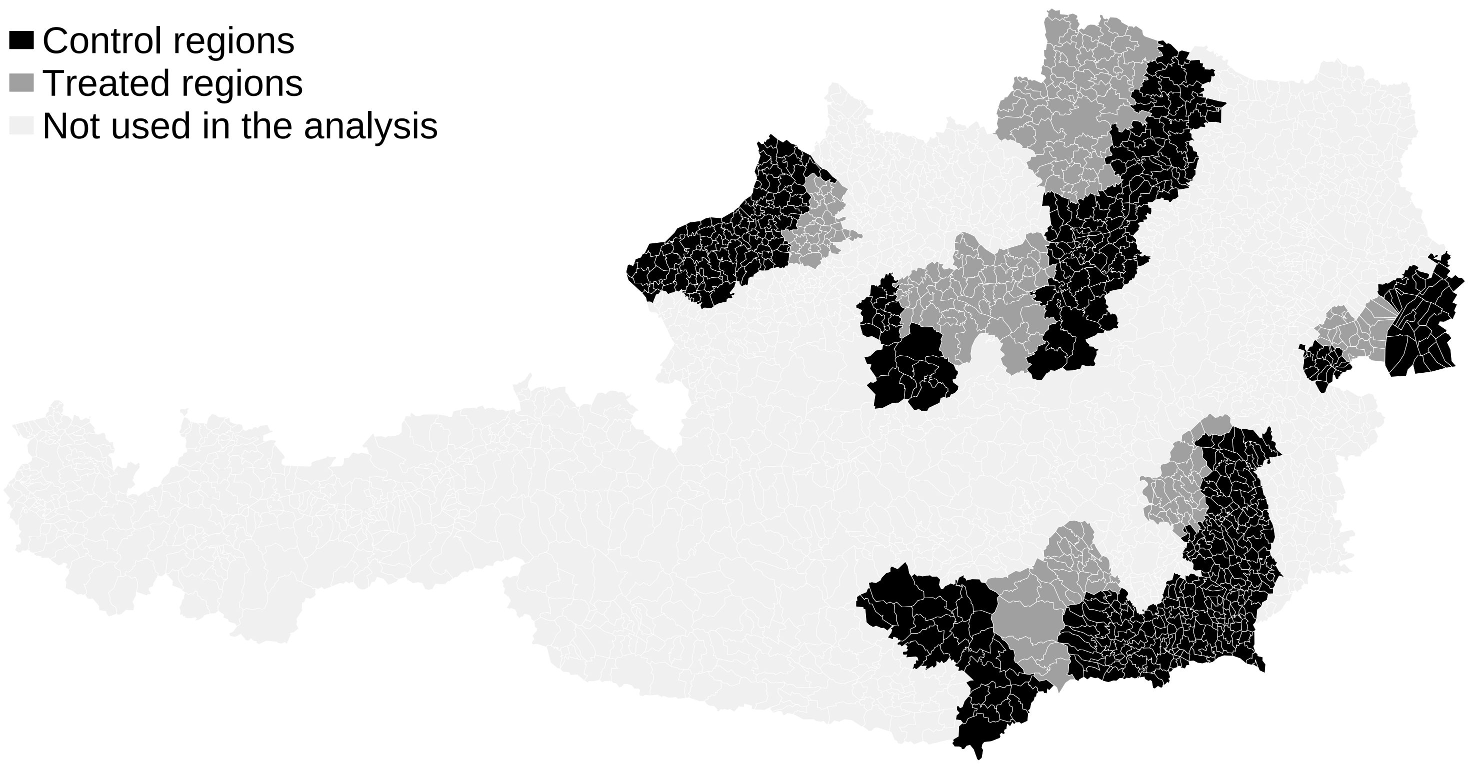
Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. A missing R^2 denotes a negative R-squared. The causes of death are classified by means of ICD-9 and ICD-10. "Other causes" are defined as described in the notes of Table **??**. Columns (2)–(9) disaggregate the diseases included in the category "other causes" in column (6) of Table **??** into more specific subcategories. The ICD codes for the subcategories are as follows: *Alcohol-unrelated digestive system diseases*: 520-579 (ICD-9); K00-K93 (ICD-10). *Non-ischemic heart diseases*: 390-429, 440-459 (ICD-9); I01-I52, I70-I99 (ICD-10). *Smoking-unrelated respiratory diseases*: 460-519 (ICD-9); J00-J99 (ICD-10). *Smoking-unrelated cancer*: 140-239 (ICD-9); C00-D48 (ICD-10). *Self-inflicted injuries*: 950-959 (ICD-9); X60-X84 (ICD-10). *Other injuries*: 850-869, 880-949 (ICD-9); W00-W99, X01-X59 (ICD-10). *Cerebrovascular diseases*: 430-438 (ICD-9); I60-I69 (ICD-10). *All remaining causes* include the remaining causes as well as those observations for which the cause of death is unknown due to failure of the match between data on causes of death and the ASSD. Note, however, that these subcategories necessarily exclude those diseases that are contained in the "alcohol- and smoking-related causes" as defined in Table **??**. Hence, these subcategories are, taken together with the categories in columns (2)–(5) of Table **??**, exhaustive and mutually exclusive.

Table A.5: Health predisposition

	Death before age 67			
Sick leave days (past 10 years)	Below median		Above median	
Mean	0.1481	0.1481	0.2117	0.2117
Standard deviation	0.3553	0.3553	0.4085	0.4085
Retirement years before age 65	0.0072	0.0122	0.0254**	0.0340***
	(0.0103)	(0.0112)	(0.0106)	(0.0115)
Cohort fixed-effects	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of observations	8,681	8,681	8,909	8,909
R^2	0.0267	0.0399	0.0722	0.0845

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Standard errors are adjusted for clustering on the cohort-region level. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Figure A.1: The Regional Extended Benefit Program (REBP)



Notes: The figure shows which communities were, or were not, eligible to the REBP among those eight NUTS-3 regions that cover both eligible and non-eligible communities). Communities shaded in black (dark gray) were (were not) eligible to the REBP. Regions shaded in light gray denote communities that we do not use in the empirical analysis because they represent the remaining NUTS-3 regions that contain either only control or only treated communities. Thus in these regions there is no variation in eligibility status across communities within any NUTS-3 region.

Figure A.2: Treatment intensity

(a) Men

(b) Women

Note: Figures show the difference in the survivor function (i.e. the probability of still being employed at a given age) between individuals from eligible and non-eligible regions.

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B Years of Life Lost

Our dependent variable, death before age 67, while precisely defined, does not tell us how early retirement affects life expectancy. To calculate the impact on life expectancy we need to impose further assumptions. To get a benchmark estimate, assume that differences in survival rates between treated and controls occur only between age 60 and age 67 and assume outside this age bracket surivivor rates are identical betwen treated and control regions. Under this assumption, the cumulative difference in survivor rates between treated and non-treated workers in the age bracket 60 to 67 yields an estimate for the impact on life expectancy.[39] If early retirement affects mortality also outside this age range, our estimated impact of early retirement on life expectancy will be biased (where the bias may go in both directions, depending on the cumulative difference in survivor rates after age 67 between treated and controls). As almost all individuals in our sample retire before age 60 (only 1.4% retire after age 60), we can provide meaningful estimates for each premature death indicator defined as the occurrence of death before age 60,...,67 in the same way as we did in our main analysis for death before age 67.

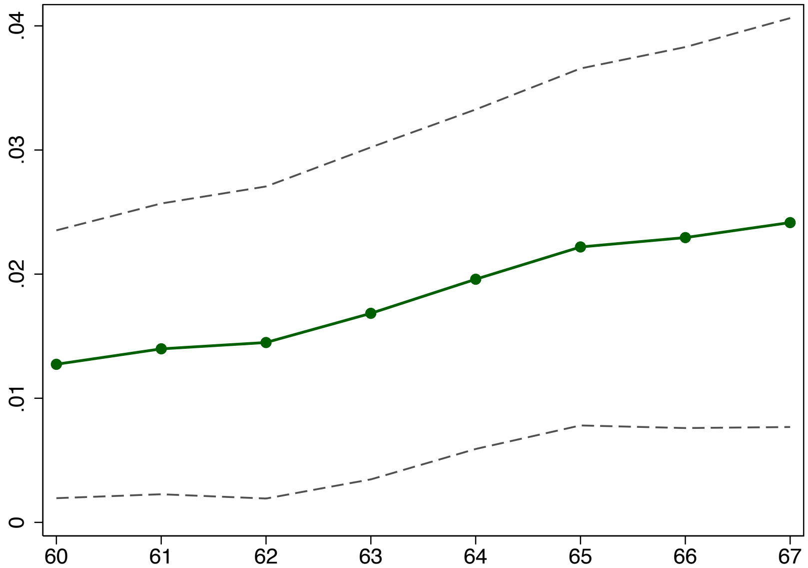
Figure **??** shows estimates for premature death before age 60,...,67. It turns out that the probability of death before age 60 is significantly higher among eligible than non-eligible workers. The estimated effect increases with age and about doubles in absolute size by age 67 (where the rightmost point estimate in Figure **??** is the main estimate from column (6) of Table **??**).

Figure **??**

To calculate the difference in life expectancy that arises due to differences in survivor rates in the age bracket 60 to 67, we simply add up the eight estimated differences in survivor rates shown in figure **??** which yields 0.15 years. More precisely, our estimates indicate that one additional year of early retirement reduces life expectancy of male blue-collar workers by 0.15 years or about 1.8 months.

[39] Denoting by T the duration of life after age 50, expected remaining life expectancy at age 50 is given by $E(T) = \sum_{t=51}^{\infty} S(t)$. Assuming that differences in mortality arise only within ages 60 and 67, the change in remaining life expectancy is given by $\Delta E(T) = \sum_{t=61}^{67} \Delta S(t) = -\sum_{t=61}^{67} \Delta F(t)$ where $F(t)$ is equal to $1 - S(t)$. Note that $F(t)$ is the dependent variable in all our regressions.

Figure B.1: 2SLS estimates of early retirement on premature death



Notes: The figure shows 2SLS estimates (and corresponding 95% confidence intervals) of early retirement on premature death before age 60,...,67 (using the same model specification as in column (6) of Table **??**).