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

October 2010

Abstract

We estimate the causal effect of early retirement on mortality for blue-collar workers. To overcome the problem of negative health selection, 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. For males, instrumental-variable estimates show that retiring one year earlier causes a significant 2.4 percentage points (about 13%) increase in the probability of dying before age 67. We do not find any adverse effect of early retirement on mortality for females. Our analysis of death causes suggests that 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 causes account for 78 percent of the causal retirement effect (while accounting for only 24 percent of all deaths in the sample). About 32 percent of the causal retirement effect are directly attributable to smoking and excessive alcohol consumption.

JEL classification: I1, J14, J26
Keywords: early retirement, mortality, premature death, health behavior, causes of death, endogeneity, instrumental variable

*We thank Michael Anderson, Joshua Angrist, David Autor, David Dorn, Christian Dustmann, Pieter Gautier, Christian Hepenstrick, Hans-Martin von Gaudecker, Marcus Hagedorn, Bas van der Klaauw, Rafael Lalive, Maarten Lindeboom, Tom van Ourti, Erik Plug, Mario Schnalzenberger, Steven Stillman, Alois Stutzer, Philippe Sulger, Uwe Sunde, Fabrizio Zilibotti, participants at the Engelberg Labor Economics Seminar 2010, the Ifo/CESifo and University of Munich Conference on Empirical Health Economics 2010, the Netspar theme conference on "health and income, work and care across the life cycle" 2010, the MIT Labor Lunch (fall 2010), as well as seminar participants in Amsterdam, Basel, Bern, Linz, Madrid, St.Gallen and Zurich for many helpful comments and suggestions. We also thank Janet Currie, Dayanand Manoli, Kathleen Mullan and Till von Wachter for valuable discussions at an early stage of this project. Financial support from the Austrian Science Fund ("The Austrian Center for Labor Economics and the Analysis of the Welfare State"), the "Forschungskredit" of the University of Zurich and the Swiss National Science Foundation (grant no. PBZHP1-133428) is gratefully acknowledged. Contact: Institute for Empirical Research in Economics, University of Zurich, Mühlebachstrasse 86, 8008 Zurich, Switzerland; kuhn@iew.uzh.ch; wuellrich@iew.uzh.ch; zweim@iew.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. 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 (Rohwedder and Willis, 2010) as well as with changes in daily routines and lifestyles which are potentially associated with unhealthy behavior (e.g. Balia and Jones, 2008; Henkens *et al.*, 2008; Midanik *et al.*, 1995; Scarmeas and Stern, 2003). In sum, the overall consequences of early retirement are not at all clear.

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 which affected some but not all older workers. Defining the date of early retirement as the date of permanent withdrawal from employment, this policy change allowed older workers in eligible regions to retire up to 3.5 years earlier 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 reverse-causality problem. 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. Moreover, the comparison between OLS and IV estimates allows us to assess the extent of health-driven selection into early retirement.

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. According to our estimates, one additional year of early retirement causes an increase in the risk of premature death of 2.4 percentage points (a relative increase of about 13.4 percent). In line with expectations, we find that IV estimates are considerably 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 of early retirement on mortality. Our results indicate no causal effect of early retirement on mortality for females suggesting that the negative association between retirement age and mortality indicated by the OLS estimate is entirely due to negative health selection. There are several reasons why male but not female blue-collar workers suffer from higher mortality. Women may be more capable of coping with major life events such as retirement; they may be more health-conscious and adopt less unhealthy behaviors (such as smoking, drinking and unhealthy diet); they may be more active after permanently exiting the labor market due to their higher involvement in household activities; and they may suffer less from a loss of social status and identity because work is less central in life for additional income earners as compared to the main breadwinner (our empirical analysis is based on older cohorts for whom the traditional role model is still the dominant one).

We consider several channels to understand why male early retirees die earlier. A *first* channel suggests that early exit from the labor market is associated with lower permanent income. We find that earnings losses due to early retirement cannot explain our finding for men, because these losses are quantitatively too small to have a substantial impact on mortality. A *second* channel suggests that changes in health-related behaviors associated with smoking, drinking, unhealthy diet, and little physical exercise may cause premature death following early retirement. Our results strongly support this hypothesis. We find that 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 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). 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 detrimental mortality effect arises from retirement following an involuntary job loss but not from voluntary quits. Even though our data do not distinguish between voluntary and involuntary retirement, we exploit severance payment rules to proxy the voluntariness of the retirement decision. Our empirical results suggest that retirement following an involuntary job loss is likely to cause excess mortality among blue collar males, while retirement after a voluntary quit does not.

Our study goes beyond the existing literature in several respects. *First*, our empirical strategy is based upon a policy change that, arguably, 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*, the 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.

While our empirical design is based on a policy change in a small country, we think our results are of more general interest. *First*, the effect we estimate with our empirical design is unlikely to originate from the particular institutional framework. Treated and control workers are both covered by mandatory universal health insurance and by a generous old-age social security system (for workers with a continuous employment history). This implies that our estimated effect cannot be driven by (lack of) access to health care or by major income losses

[1] The distinction between subjective and objective measures appears to be of special importance (Bound, 1991), as even self-reported measures of physical health may be subject to considerable reporting error (Baker *et al.*, 2004). It is likely that truly subjective measures of health, i.e. individuals' assessment of their well-being, perform even worse because of ex-post justification bias 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.

after retirement. Instead we estimate a more direct effect of early retirement on mortality. In environments where retirees have no access to health care or suffer from major income losses after retirement, our estimate provides a lower bound. A *second* reason why we think that Austria in an interesting case is that early retirement is a very common phenomenon. In the early 1990s, the average age at retirement entry was as low as 58 for the whole Austrian population and it was even lower for blue collar workers. 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.

Among the large number of papers studying the health and mortality effects of retirement, studies adopting convincing empirical strategies to estimate the causal impact of retirement on health and/or mortality are rare. Bound and Waidmann (2007) use institutional rules governing eligibility to public pensions to identify the effect of retirement on both subjective and objective measures of physical health, by relying both on survey data and vital statistics for the UK. They find no effects, or a slightly positive influence, of retirement on health, once the possibility of endogenous entry into retirement is taken into account. Even though institutional rules offer an apparently plausible instrument for the age at retirement, the fact that workers know the exact rules may render these instruments invalid.[2] Coe and Lindeboom (2008) improve on this methodology and exploit sudden and arguably unexpected changes in retirement opportunities (i.e. early retirement opportunities offered by firms to groups of workers) in the US to identify the causal effect of early retirement on men's health. They find no detrimental effects of early retirement on health and, if anything, even slightly temporary improvements.[3] Charles (2002) also uses age discontinuities in the financial incentives to retire, as well as legal changes to these incentives, to identify the causal effect of retirement on subjective well-being. He finds a positive effect of retirement on subjective well-being when accounting for the endogeneity of the retirement decision, while the raw correlation between age at retirement and well-being is negative. Similar results on mental well-being are reported in Neuman (2008) for the US and Johnston and Lee (2009) for the UK, and Coe and Zamarro (2008) in a cross-country study for

[2] As pointed out by Coe and Lindeboom (2008), workers who know the exact legal rules may adjust their behavior before actually retiring. Moreover, workers subject to different retirement rules may also differ with respect to unobserved variables, absent any behavioral responses.

[3] A potential problem with this approach is that even though firms were restricted in targeting specific groups of individuals, they were free to choose whether or not to offer any early retirement window at all. Hence workers who were offered any early retirement opportunity may differ from workers who were not.

Europe, all using survey data and a similar empirical design. Kerkhofs and Lindeboom (1997) 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. One of the few studies finding a detrimental effect of retirement on health is Behncke (2009), who applies matching methods to survey data from the UK. She finds that retirement increases both the risk of a cardiovascular disease and the risk of being diagnosed with cancer. While the estimated positive effect on several health outcomes is in line with much of the medical literature (see also footnote 4), the empirical design may still suffer from endogeneity bias dues to unobserved factors (such as individuals' true health status). Qualitatively similar results are reported in Dave *et al.* (2008), 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. 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.[4]

Our paper is also related to a literature that focuses on the impact of involuntary job loss on mortality, with respect to both research topic and methodology. An interesting recent study by Sullivan and von Wachter (2009) estimates the effect of job displacement on mortality in the US. They find a strong impact of involuntary job loss on mortality, particularly for older (high-seniority) workers and for workers who suffer large earnings losses (i.e. low-wage workers). In a related study in Sweden, Eliason and Storrie (2009) examine the impact of job loss on cause-specific mortality. They find a strong increase in overall mortality among men, but no impact on females. There was, however, an increase in suicides and alcohol-related mortality for both men and women. Adverse effects of involuntary job loss on mortality are also reported in another recent study based on Norwegian data by Rege *et al.* (2009).

[4]Unsurprisingly, similar ambivalence regarding the health effects of retirement is found among medical and epidemiological studies. Bamia *et al.* (2008) find that the risk of all-cause mortality is significantly higher for retirees than for older workers still engaged in economic activity. This finding is consistent with the results of Gallo *et al.* (2006), who argue that job loss increases individuals' risk of cardiovascular disease and therefore has detrimental effects on the health of older workers. Morris *et al.* (1994) also find increases risk of cardiovascular disease for the UK. Somewhat contrasting evidence is presented in Tsai *et al.* (2005) who study the effects of early retirement on mortality in a very specific sample of workers in the petrochemical industry. Similarly, Litwin (2007) finds no association between early retirement and all-cause mortality and Brockmann *et al.* (2009) find no effect of retirement on health, at least when focusing on previously healthy workers only.

The paper is structured as follows. In section 2 we discuss the institutional background and we describe how changes in the unemployment insurance system lead to early permanent withdrawal from work for some groups of workers. Section 3 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 4. Our results are presented in sections 5 and 6. In section 7, we focus on potential channels explaining excess mortality among male retirees. Section 8 concludes.

2 Pathways to Retirement in Austria

In this section we describe the various pathways into early retirement in Austria. We define as "early retirement" the date at which an individual withdraws permanently from the labor market. This 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 an individual gets after having permanently withdrawn from work.

2.1 The Retirement System

Almost all workers in Austria are covered by the old age social security system, and the benefits paid by this system are the most important source of income for retirees (OECD, 2007). The level of old age social security benefits depends 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).[5] The maximum gross replacement rate for a worker retiring at the statutory retirement age in the year 1993 was 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 age 65 for men and age 60 for women. For workers with long-insurance duration the statutory retirement age is age 60 for men and age 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

[5]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 Hofer and Koman (2006) for details.

security 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.

There are several pathways into regular retirement. A first pathway is the direct transition from employment to retirement. A second pathway is the indirect transition from employment to retirement via the unemployment system. Individuals with a continuous work history become eligible for regular old-age social security benefits at age 60 after having drawn regular unemployment benefits and/or means-tested unemployment assistance for at least 12 out of the previous 15 months ("vorzeitige Alterspension wegen Arbeitslosigkeit"). An unemployed person aged 50 or older could draw regular unemployment benefits for a maximum period of 52 weeks (30 weeks before August 1989) with a replacement rate of 40–60%. Unemployment assistance payments may, in principle, last for an indefinite time period. Alternatively, unemployed individuals who had paid social security contributions for at least 15 out of the last 25 years are also eligible to regular early retirement benefits at age 60 after a period of 12 months in special income support ("Sonderunterstützung"). which is equivalent to a regular unemployment spell but grants a transfer that is 25% higher than regular unemployment benefits. Individuals eligible to special income support could "move" from unemployment benefits to special income support. This pathway essentially allowed workers to withdraw permanently from work 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. This latter pathway becomes more easy to access after age 55 when eligibility rules to disability benefits become significantly relaxed (Hofer and Koman, 2006).[6]

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 allowed eligible workers to withdraw permanently from employment as much as 3.5 years earlier than non-eligible workers. The

[6] 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.

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.[7] 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.[8] In contrast, 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).

Figure 1 about here

Figure 1 summarizes the institutional design of our study. The figure makes clear 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 a 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 more detail in Zweimüller *et al.* (2009). 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

[7] Previous econometric evaluations of the REBP have found large effects of the program on realized unemployment duration (Lalive, 2008; Lalive and Zweimüller, 2004a,b; Winter-Ebmer, 1998).

[8] 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 workplace had to be in a REBP region (see section 3.2 for details).

(year and month of birth, age, sex, general occupation) and additional information on the firms where the workers were employed. 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 because we want to exploit the exogenous variation in the retirement age that the program induces, i.e. we will use eligibility status as an instrument for the retirement age. We use information on individuals' month of birth and employment history to determine whether a worker meets the age and employment criteria set by the REBP. However, we do not observe the place of residence. To proxy community of residence we use the community of work. While this introduces some measurement error due to the false classification of REBP eligible workers as non-eligible and vice versa, we find that this is not a major drawback, as most individuals work in the same labor market district where they live.[9]

3.2 Sample Selection

Workers

First, we restrict the analysis to blue collar workers.[10] 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, in principle, also available to white collar workers, effective take-up by white collars was weak.[11]

We further restrict the sample to workers who meet the age criteria at some time during the period the REBP was in effect and who had a continuous work history before reaching the

[9] 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.

[10] 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.

[11] 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 (results for white collar workers are available upon request).

age of 50. The *age criterion* implies that we consider only men born between July 1929 and December 1941 and women born between July 1934 and December 1941, respectively.[12] This ensures that these individuals eventually attain age 50 during the REBP and men (women) are aged 59 (54) or less when the program was introduced. Put differently, while these cohorts were able to retire earlier (recall that men/women can claim special income support as soon as they turn 59/54) not all of them could take full advantage of the program. 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 already were 56 years old when the REBP started) and cohorts born after 1939 were too young (i.e. they were younger than 55 when the REBP was abolished).

The *experience criterion* selects workers who meet the REBP work experience requirement, 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 unemployment 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' place of residence (proxied by place of work; see footnote 9). This means that by using REBP eligibility as instrument for the retirement age, we basically compare individuals who work in eligible districts with those who work in non-eligible districts (section 4 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 second important program to alleviate problems associated with mass redundancies in the steel sector, the "steel foundation". This program was available both in treated and in control regions. 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 (see Winter-Ebmer, 2001, for an

[12]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).

evaluation of the steel foundation). 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).[13] 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.[14]

Figure 2 about here

Figure 2 highlights the communities within those eight NUTS-3 units that we actually consider in the empirical analysis. The areas in black denote eligible communities and the areas in dark gray denote non-eligible communities within these NUTS-3 units, respectively. The remaining communities, i.e. those shaded in light gray, denote eligible and non-eligible communities which are not considered in the analysis.

3.3 Key Measures

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

[13]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) ("Bezirk(e)").

[14]However, the map also shows that treated regions were not selected randomly. 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 4 below). See also the discussion in Winter-Ebmer (1998) and Lalive and Zweimüller (2004a,b) on how the regions were selected for eligibility in the first place. Importantly, Lalive and Zweimüller (2004a) 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 (Lalive, 2008). Indeed, our results on the first-stage effect of the program are perfectly in line with this finding (see section **??** below).

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.[15]

Since workers in our sample have to be alive at age 50 and meet the REBP age and experience criteria. Hence 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 15). 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.[16] 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 male and female retirement and mortality patterns are very different.

3.4 Descriptive Statistics

Table 1 shows descriptive statistics for our two different subsamples and by eligibility status. Our sample consists of 17,590 blue-collar males and 3,283 blue-collar females of whom 18.0 percent

[15] 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).

[16] 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.

and 7.2 percent die before age of 67, respectively. Male workers in eligible districts retire 0.75 years (9 months) earlier than their colleagues in non-eligible regions. This is strong prima-facie evidence that male workers use the REBP as an indirect channel into early retirement. The situation is even more pronounced for females, who retire 1.15 years (14 months) earlier in treated than in control regions.

Table 1 about here

Table 1 also shows that the treated and control samples are well balanced (though not identical) with respect to observable characteristics. Columns (1) to (4) shows almost no difference in average (and variance of) age, indicating the absence of any major differences in age composition of the blue collar workers between the two types of regions. The various variables describing the previous work experience indicate slightly higher work experience before age 50 in non-eligible regions; the difference is rather small, however. Interestingly, blue collar workers in eligible regions were slightly less often on sick leave before age 50 than workers in control regions. Moreover, male blue collar workers in treated regions earned higher wages before age 50 (average earnings at ages 43 to 49) than those in control regions. We also see that the industry mix between regions is similar though not identical. There is a somewhat higher fraction of manufacturing workers in treated regions, and a somewhat larger fraction of construction and agriculture workers in control regions. Since treated and control groups are similar but not identical controlling for remaining differences in worker characteristics and in industry structure is potentially important in the empirical analysis below.

Columns (5) to (8) show analogous descriptive statistics for female blue collar workers. It turns out that the differences across regions among females are very similar to those among males. There is only a negligible difference in age and experience indicators. Blue collar females in treated regions have a lower incidence of sick days, earn somewhat higher wages, and are more concentrated in manufacturing than blue-collar females in control regions.

4 Econometric Framework

Estimating the causal effect of early retirement on health and mortality is difficult because poor health is a key determinant in individuals' retirement decisions (e.g. Disney *et al.*, 2006; Dwyer and Mitchell, 1999). This negative health selection implies that simple 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. We now detail how we deal with this issue.

To fix ideas, let Death_i^{67} denote a dummy variable indicating death before age 67 (such that Y_i takes on the value 1 in the event of death before age 67, and 0 otherwise) and let ER_i denote the number of years spent in early retirement. That is, ER_i measures the difference between the statutory and actual retirement age such that positive values correspond to exit from the labor force before the statutory retirement age. Our regression model of interest can then simply be written as

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

where X_i denotes additional control variables and ϵ_i is the error term. We are interested in estimating parameter β_1, the causal effect of early retirement years (i.e. the number of years between the last day in regular employment and the statutory late retirement age) 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

Our empirical design tackles reverse causality by exploiting the exogenous variation in the date of permanent exit from employment generated by the REBP. As we explained, the REBP allowed eligible workers in treated regions to advance permanent withdrawal from employment by up to 3.5 years. To assess the causal relationship between early retirement and mortality, we use an instrumental variable (IV) approach. Using this empirical strategy, we estimate the causal effect for those individuals whose date of permanent exit from employment is affected by their eligibility to the REBP, i.e. we use workers' REBP eligibility as an instrument for their actual retirement age (e.g. Angrist *et al.*, 1996; Imbens and Angrist, 1994). 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 a function of age, previous work experience, and location of residence. Hence REBP eligibility should be considered to be conditionally randomized, where the conditioning is done on the eligibility criteria mentioned above.[17] 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 4.2 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 his or her location of residence. From this perspective, we have to argue that each of these indicator functions is exogenous from an individual's standpoint. Otherwise, it would be possible for an individual to manipulate one (or more) of the variables determining eligibility and thus indirectly manipulate his or her eligibility status. Age and previous work experience are unlikely to be endogenous in the present context.[18] 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. While this is a potential problem, it 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.[19] 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

[17]Introducing covariates into the heterogeneous effects model technically calls for the semi-parametric procedure proposed by Abadie (2003). 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, Angrist (2001) argues that 2SLS is likely to give a good approximation to the causal relationship of interest in many cases (i.e. the Abadie procedure is identical to 2SLS when the first stage is linear).

[18]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 1 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.

[19]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).

instrument. To circumvent this potential problem, we included only those NUTS-3 regions in our sample that comprise both districts eligible to the REBP and those 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.[20]

The specification of the first-stage regression remains. Based on the previous discussion, we assume that the following equation determines the duration of early retirement

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

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 (in which case $Z_i = 1$) 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.[21] We also include additional control variables denoted by X_i in some specifications.[22] 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 only 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

[20] 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 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 5.

[21] 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.

[22] 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.

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

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

which implies that we now have 25 instruments for our male cohorts (1929h2–1941h2) and 15 instruments for our female cohorts (1934h2–1941h2), respectively.

4.2 Assessing Instrument Validity

As we have explained, our key identifying assumption is that location of residence in either a treated or a control region is exogenous with respect to individuals' health status. We now provide two pieces of evidence supporting the validity of our instrument.

Table 2 about here

First, Table 2 shows the estimates of a regression of standardized mortality rates at the district level for the years 1978–1984, well before the REBP was implemented. We explore differences in standardized mortality rates 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 eligible districts. 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 3 about here

The second piece of evidence makes use of individual-level information on workers' days on sick leave provided by the ASSD. This is a good proxy for workers' ex-ante health condition. We measure the number of sick leave days *before* the individual turns age 50, i.e. immediately before he or she meets the age criterion on the REBP. 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 3 shows reduced-form results for four different counts of sick leave days, for male and female workers separately. Irrespective of the length of retrospective information used for the sickness indicator, 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 2 and 3 provides strong support for our claim that the selection of eligible labor-market districts was unrelated to mortality in these districts.

5 Program Eligibility and Early Retirement

A first look at descriptive statistics in section 3.4 above shows that both males and females withdraw substantially earlier from the work force in eligible regions. We proceed by presenting first-stage estimates of equations (2) and (3), respectively. Results are given in Table 4 for men and Table 5 for women, respectively. We will first discuss the results for males.

Tables 4 and 5 about here

We show estimates for four different regression specifications. Columns (1) and (2) estimate one common effect of the instrument on the endogenous variable, while columns (3) and (4) allow for a varying effect across birth cohorts. Columns (1) and (3) control for cohort fixed-effects, past work experience, and NUTS-3 fixed-effects; columns (2) and (4) additionally include past sick leave days, the average and standard deviation of yearly earnings (during ages 43 to 49), and industry fixed-effects.

We start with the just-identified case (i.e. estimates of equation (2)), shown in the first two columns of each table. 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 column (2), the effect of the instrument is somewhat reduced to 0.59 years (roughly 7 months). Table 5 reports corresponding results for female workers. The 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 (see column (2) of Table 5).

Figure 3 about here

Next, we turn to the over-identified case, given by equation (3) above. The overall pattern becomes more apparent in a graph. Figure 3 displays the relevant parameter estimates, $\hat{\alpha}_{Zj}$, per year-semester cohort (these estimates correspond to those displayed in column (3) of Table 4). 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 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)).

Column (3) of Table 4 reports the estimates from Panel (a) of Figure 3. 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 the 1%-level (except for birth cohort 1933h2, which is only marginally significant at the 10%-level). 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 12, i.e. it is larger than the threshold value of 10 above which 2SLS is not supposed to be subject to a weak instruments critique as proposed by Staiger and Stock (1997). Adding further controls again reduces the magnitude of the first-stage effect somewhat, but the F statistic for the excluded instruments is still slightly larger than 10.[23]

[23]Table A.1 in the appendix 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.

Column (3) of Table 5 shows the corresponding point estimates for women, displayed graphically in Panel (b) of Figure 3. 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 the 1%-level. Adding further controls in column (4) hardly changes anything. The F statistic for the excluded instruments exceeds the value of 10 in both column (3) and column (4). This again suggests that we do not run into any weak-instruments issues.

Treatment Intensity

Figure 4 takes a closer look at the distribution of the effective age at retirement by eligibility status, for men and women separately. More precisely, the figure shows the difference in the survivor function of still being in employment at a given age between individuals from eligible versus non-eligible regions. The difference measured on the vertical axis of the figure is negative throughout, indicating that the fraction of workers still at work at any particular age is lower in eligible regions than in non-eligible regions.

Figure 4 about here

We showed above that the individuals retiring between age 55 and 59 are those who drive these effects. This exactly is what we expect from the institutional rules: workers eligible to extended unemployment benefits due to the REBP can already retire at age 55, draw regular unemployment benefits until the age of 59, and then draw benefits from special income support before they become eligible to regular early retirement benefits at the age of 60. Workers in non-eligible regions have no access to extended unemployment benefits and can first claim special income support at age 59. Male blue collar workers eligible to the REBP are 9-14% less likely to be in employment within the age bracket 55-59. As a consequence, our IV estimates capture the causal effect of changes in the retirement age within this age bracket, but tell us little, if anything, about the effects of retiring between the statutory retirement age with long insurance duration (60/55) and the statutory retirement age (65/60).[24]

[24]Early retirement also involves a substitution among different labor market activities. Figure A.1 in the appendix shows how eligible and non-eligible workers differ with respect to labor market activities. The left-hand panel shows that workers eligible to the REBP spend less time in employment at ages 50-65 than non-eligible workers. If eligibility to extended unemployment benefits drives earlier effective retirement of blue collar workers in eligible regions, we should see more workers on unemployment benefits after permanent exit from

6 The Effect of Early Retirement on Mortality

Tables 6 and 7 report our main results for blue collar males and females, respectively. Column (1) of Table 6 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 0.0011). Taken literally, this would imply that the probability of dying before age 67 increases by 3.22 percentage points 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 6 about here

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 0.0078), 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 0.0086, 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

employment. This is exactly what we find: eligible workers spend more than 2 percentage points more of their time on unemployment benefits than non-eligible workers. Apparently, the instrument induces individuals to retire earlier by means of the extended unemployment as a channel from work to retirement by first claiming extended unemployment benefits before accessing regular retirement benefits. The right-hand panel shows that eligible workers substitute regular old-age pension with unemployment benefits after they permanently drop out of employment. The figure also shows that time spent out of the labor force does not substantially differ across the two groups (at least for men). In sum, this strongly suggests a pattern of labor market behavior that is consistent with the incentives generated by the REBP.

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 6 as our preferred estimate and refer to it as such in the following.

Furthermore, as proposed by Angrist and Pischke (2009), we compare the 2SLS estimates with those produced by the limited information maximum likelihood (LIML) estimator in the over-identified case.[25] 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 0.0086). 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 0.0096). In both cases, the LIML estimates are very similar to the 2SLS estimates (though, 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).

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 7 about here

Table 7 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.

[25] 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.

[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 6 (results are available upon request).

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) and (5)). The 2SLS estimates tell us that female workers' earlier exit from the work force has no impact on mortality. Again, the corresponding LIML estimates 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.

Figure 5 about here

Our IV strategy in the over-identified case lends itself to a simple graphical representation, which is given by Figure 5. 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 Angrist, 1991). Briefly, the left-hand panel of Figure 5 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 5 shows average residuals from regressions that include additional control variables (corresponding to regression specification shown in column (6) in Table 6). 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 5 shows that no such relation exists for female workers.

Years of Life Lost

While our dependent variable, death before age 67, is precisely defined, it does not tell us whether and to which extent early retirement affects life expectancy. Calculating the impact on life expectancy is not straightforward because the underlying mortality hazard is nonlinear in age and because we observe actual mortality only until age 67 for all individuals in our sample. To convert our estimated effect of into years of life lost, we need to impose further assumptions. To get a benchmark for the impact of early retirement on life expectancy, we assume that differences in survival rates between treated and controls occur between age 60 and age 67 only and that there are no retirement-effects on mortality rates (i.e. non-survivor rates) outside this age bracket. 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.[27] 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). 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 6 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 6 is the main estimate from column (6) of Table 6).

Figure 6 about here

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 6 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. Recall that this estimate is valid only if all differences in survivor rates occur between ages 60 and 67 – and that the (cumulative) difference in survivor rates between treated and control groups outside the age bracket 60 to 67 is zero. This estimate is biased upward if the cumulative difference outside this age bracket is higher among treatment groups and vice versa.

[27] Denoting by T the duration of life after age 50, expected remaining life expectancy at age 50 is given by $E(T) = \sum_{t=51}^{\infty} S(t)$ (Lancaster, 1992, p.13). 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.

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 Sullivan and von Wachter (2009), 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

[28]See Table A.2 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 6. 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 6.

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. Ekerdt *et al.*, 1989; Lang *et al.*, 2007; Midanik *et al.*, 1995), retirement and (excessive) alcohol use (e.g. Neve *et al.*, 2000; Perreira and Sloan, 2002), as well as between retirement and changes in diet and physical activity (e.g. Chung *et al.*, 2009a,b; Evenson *et al.*, 2002; Mein *et al.*, 2005; Slingerland *et al.*, 2007).

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 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)

[31] This is similar to Bedard and Deschênes (2006) who use cause-specific mortality rates to investigate excess mortality among World War II and Korean War Veterans in the U.S. They find that military-induced smoking drives most of the observed excess mortality.

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 A.3 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 8 about here

The results for the cause-specific mortality are displayed in Table 8. 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 6 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

[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 U.S. 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 U.S. Department of Health and Human Services (2001). 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 (Canto and Iskandrian, 2003; Greenland *et al.*, 2003; Yusuf *et al.*, 2004)

[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 U.S. males aged 55-64 (65 and older) are related to alcohol.

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).

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 A.3 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 as "alcohol-related causes" are directly attributable to excessive alcohol consumption;[35] 34% of ischemic heart diseases are caused by smoking;[36] 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

[34] We also investigate several subsets of the remaining causes in Table A.4. 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] 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 A.3).

[36] 34% corresponds to the weighted average of the age dependent smoking attributable fractions regarding ischemic heart diseases (see footnote 32; the weights are the share of individuals dying of ischemic heart diseases before and after age 65).

important underlying reasons for ischemic heart diseases (see U.S. Department of Health and Human Services (2001)). 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. We conclude that detrimental changes in health-related behaviors are a major reason for excess mortality among blue collar early retirees.

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 (Henkens *et al.*, 2008; van Solinge and Henkens, 2007).[37]

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 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.

[37] 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 A.5 sheds light on this issue. The mortality effect is strong and highly significant ex-ante among workers who are unhealthier. This suggests that effective 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.

Table 9 about here

Table 9 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 6 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 9).

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). 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. 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 and identity.

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.

References

Abadie, A. (2003). Semiparametric instrumental variable estimation of treatment response models. *Journal of Econometrics*, **113**(2), 231 – 263.

Angrist, J. (1991). Grouped-data estimation and testing in simple labor-supply models. *Journal of Econometrics*, **47**(2/3), 243–266.

Angrist, J. (2001). Estimation of Limited Dependent Variable Models with Dummy Endogenous Regressors: Simple Strategies for Empirical Practice. *Journal of Business & Economic Statistics*, **19**(1), 2–16.

Angrist, J. and Pischke, J. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton University Press.

Angrist, J., Imbens, G., and Rubin, D. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association*, **91**(434).

Baker, M., Stabile, M., and Deri, C. (2004). What do self-reported, objective, measures of health measure? *Journal of Human Resources*, **39**(4), 1067.

Balia, S. and Jones, A. (2008). Mortality, lifestyle and socio-economic status. *Journal of Health Economics*, **27**(1), 1–26.

Bamia, C., Trichopoulou, A., and Trichopoulos, D. (2008). Age at retirement and mortality in a general population sample: the Greek EPIC study. *American Journal of Epidemiology*, **167**(5), 561–569.

Bedard, K. and Deschênes, O. (2006). The long-term impact of military service on health: evidence from World War II and Korean War veterans. *American Economic Review*, **96**(1), 176–194.

Behncke, S. (2009). How Does Retirement Affect Health? IZA Discussion Paper No. 4253.

Bound, J. (1991). Self-reported versus objective measures of health in retirement models. *Journal of Human Resources*, **26**(1), 106–138.

Bound, J. and Waidmann, T. (2007). Estimating the Health Effects of Retirement. Working Paper 2007-168, University of Michigan.

Brockmann, H., Müller, R., and Helmert, U. (2009). Time to retire - Time to die? A prospective cohort study of the effects of early retirement on long-term survival. *Social Science & Medicine*, **69**(2), 160–164.

Canto, J. and Iskandrian, A. (2003). Major risk factors for cardiovascular disease: debunking the "only 50%" myth. *Jama*, **290**(7), 947.

Charles, K. (2002). Is Retirement Depressing? Labor Force Inactivity and Psychological Well-Being in Later Life. NBER Working Paper No. 9033.

Chung, S., Domino, M., Stearns, S., and Popkin, B. (2009a). Retirement and Physical Activity:: Analyses by Occupation and Wealth. *American journal of preventive medicine*, **36**(5), 422–428.

Chung, S., Domino, M., and Stearns, S. (2009b). The effect of retirement on weight. *The Journals of Gerontology: Series B*, **64B**(5), 656–665.

Coe, N. and Zamarro, G. (2008). Retirement Effects on Health in Europe. RAND Working Paper No. 588.

Coe, N. B. and Lindeboom, M. (2008). Does Retirement Kill You? Evidence from Early Retirement Windows. IZA Discussion Paper No. 3817.

Dave, D., Rashad, I., and Spasojevic, J. (2008). The effects of retirement on physical and mental health outcomes. *Southern Economic Journal*, **75**(2), 497–523.

Disney, R., Emmerson, C., and Wakefield, M. (2006). Ill health and retirement in Britain: A panel data-based analysis. *Journal of Health Economics*, **25**(4), 621–649.

Dwyer, D. and Mitchell, O. (1999). Health problems as determinants of retirement: Are self-rated measures endogenous? *Journal of Health Economics*, **18**(2), 173–193.

Ekerdt, D., De Labry, L., Glynn, R., and Davis, R. (1989). Change in drinking behaviors with retirement: findings from the normative aging study. *Journal of studies on alcohol*, **50**(4), 347.

Eliason, M. and Storrie, D. (2009). Does Job Loss Shorten Life? *Journal of Human Resources*, **44**(2), 277.

Evenson, K., Rosamond, W., Cai, J., Diez-Roux, A., and Brancati, F. (2002). Influence of retirement on leisure-time physical activity: the atherosclerosis risk in communities study. *American journal of epidemiology*, **155**(8), 692.

Gallo, W., Teng, H., Falba, T., Kasl, S., Krumholz, H., and Bradley, E. (2006). The impact of late career job loss on myocardial infarction and stroke: a 10 year follow up using the health and retirement survey. *British Medical Journal*, **63**(10), 683.

Greenland, P., Knoll, M., Stamler, J., Neaton, J., Dyer, A., Garside, D., and Wilson, P. (2003). Major risk factors as antecedents of fatal and nonfatal coronary heart disease events. *Jama*, **290**(7), 891.

Henkens, K., Van Solinge, H., and Gallo, W. (2008). Effects of retirement voluntariness on changes in smoking, drinking and physical activity among Dutch older workers. *European Journal of Public Health*, **18**(6), 644.

Hofer, H. and Koman, R. (2006). Social security and retirement incentives in Austria. *Empirica*, **33**(5), 285–313.

Imbens, G. and Angrist, J. (1994). Identification and estimation of local average treatment effects. *Econometrica*, **62**(2), 467–475.

Johnston, D. and Lee, W. (2009). Retiring to the good life? The short-term effects of retirement on health. *Economics Letters*, **103**(1), 8–11.

Kerkhofs, M. and Lindeboom, M. (1997). Age related health dynamics and changes in labour market status. *Health Economics*, **6**(4), 407–423.

Lalive, R. (2008). How do extended benefits affect unemployment duration? A regression discontinuity approach. *Journal of Econometrics*, **142**(2), 785–806.

Lalive, R. and Zweimüller, J. (2004a). Benefit Entitlement and the Labor Market: Evidence from a Large-Scale Policy Change. In J. Agell, M. Keen, and A. J. Weichenrieder, editors, *Labor Market Institutions and Public Regulation*, pages 63–100. MIT Press.

Lalive, R. and Zweimüller, J. (2004b). Benefit entitlement and unemployment duration. The role of policy endogeneity. *Journal of Public Economics*, **88**(12), 2587–2616.

Lancaster, T. (1992). *The econometric analysis of transition data*. Cambridge University Press.

Lang, I., Rice, N., Wallace, R., Guralnik, J., and Melzer, D. (2007). Smoking cessation and transition into retirement: analyses from the English Longitudinal Study of Ageing. *Age and ageing*, **36**(6), 638–643.

Litwin, H. (2007). Does early retirement lead to longer life? *Ageing and Society*, **27**(05), 739–754.

Mein, G., Shipley, M., Hillsdon, M., Ellison, G., and Marmot, M. (2005). Work, retirement and physical activity: cross-sectional analyses from the Whitehall II study. *The European Journal of Public Health*, **15**(3), 317.

Midanik, L., Soghikian, K., Ransom, L., and Tekawa, I. (1995). The effect of retirement on mental health and health behaviors: the Kaiser Permanente Retirement Study. *Journals of Gerontology Series B: Psychological Sciences and Social Sciences*, **50**, 59–59.

Morris, J., Cook, D., and Shaper, A. (1994). Loss of employment and mortality. *British medical journal*, **308**(6937), 1135–1139.

Neuman, K. (2008). Quit Your Job and Get Healthier? The Effect of Retirement on Health. *Journal of Labor Research*, **29**(2), 177–201.

Neve, R., Lemmens, P., and Drop, M. (2000). Changes in alcohol use and drinking problems in relation to role transitions in different stages of the life course. *Substance Abuse*, **21**(3), 163–178.

OECD (2007). Pensions at a Glance. Technical report, Organisation for Economic Co-operation and Development.

Perreira, K. and Sloan, F. (2002). Excess alcohol consumption and health outcomes: a 6-year follow-up of men over age 50 from the health and retirement study. *Addiction*, **97**(3), 301–310.

Rege, M., Telle, K., and Votruba, M. (2009). The effect of plant downsizing on disability pension utilization. *Journal of the European Economic Association*, **7**(4), 754–785.

Rohwedder, S. and Willis, R. J. (2010). Mental retirement. *Journal of Economic Perspectives*, **24**(1), 119–138.

Scarmeas, N. and Stern, Y. (2003). Cognitive reserve and lifestyle. *Journal of Clinical and Experimental Neuropsychology*, **25**(5), 625–633.

Slingerland, A., Van Lenthe, F., Jukema, J., Kamphuis, C., Looman, C., Giskes, K., Huisman, M., Narayan, K., Mackenbach, J., and Brug, J. (2007). Aging, retirement, and changes in physical activity: prospective cohort findings from the GLOBE study. *American journal of epidemiology*, **165**(12), 1356–1363.

Staiger, D. and Stock, J. (1997). Instrumental variables regression with weak instruments. *Econometrica*, **65**(3), 557–586.

Sullivan, D. and von Wachter, T. (2009). Job Displacement and Mortality: An Analysis Using Administrative Data. *Quarterly Journal of Economics*, **124**(3), 1265–1306.

Tsai, S., Wendt, J., Donnelly, R., de Jong, G., and Ahmed, F. (2005). Age at retirement and long term survival of an industrial population: prospective cohort study. *British Medical Journal*, **331**(7523), 995.

U.S. Department of Health and Human Services (2001). The surgeon general's call to action to prevent and decrease overweight and obesity. Technical report, U.S. Department of Health and Human Services, Public Health Service, Office of the Surgeon General.

van Solinge, H. and Henkens, K. (2007). Involuntary retirement: The role of restrictive circumstances, timing, and social embeddedness. *Journals of Gerontology Series B: Psychological Sciences and Social Sciences*, **62**(5), S295.

Winter-Ebmer, R. (1998). Potential unemployment benefit duration and spell length: lessons from a quasi-experiment in Austria. *Oxford Bulletin of Economics and Statistics*, **60**(1), 33–45.

Winter-Ebmer, R. (2001). Evaluating an Innovative Redundancy-Retraining Project: The Austrian Steel Foundation. IZA Discussion Paper No. 277.

Yusuf, S., Hawken, S., Ôunpuu, S., Dans, T., Avezum, A., Lanas, F., McQueen, M., Budaj, A., Pais, P., Varigos, J., *et al.* (2004). Effect of potentially modifiable risk factors associated with myocardial infarction in 52 countries (the INTERHEART study): case-control study. *The Lancet*, **364**(9438), 937–952.

Zweimüller, J., Winter-Ebmer, R., Lalive, R., Kuhn, A., Ruf, O., Wuellrich, J.-P., and Büchi, S. (2009). The Austrian Social Security Database (ASSD). IEW Working Paper No 410.

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
	(0.3335)	(0.4241)	(0.6492)	(1.0382)	(0.9182)	(1.1431)	(1.7162)	(2.2929)
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. Robust standard errors in parentheses. 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, men

	Retirement years before the statutory retirement age			
Mean	8.6678	8.6678	8.6678	8.6678
Standard deviation	2.9346	2.9346	2.9346	2.9346
Eligible district	0.7100***	0.5895***		
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***
Eligible district · 1935h1			0.9849***	0.7802***
Eligible district · 1935h2			0.7494***	0.5207**
Eligible district · 1936h1			1.2162***	1.1637***
Eligible district · 1936h2			0.6622***	0.6336***
Eligible district · 1937h1			1.0500***	1.0469***
Eligible district · 1937h2			1.3570***	1.2406***
Eligible district · 1938h1			0.9968***	0.9708***
Eligible district · 1938h2			0.5397**	0.3333
Eligible district · 1939h1			1.1068***	0.9968***
Eligible district · 1939h2			0.7041***	0.5939***
Eligible district · 1940h1			0.8803***	0.9863***
Eligible district · 1940h2			0.9150***	0.8897***
Eligible district · 1941h1			0.9651***	0.9921***
Eligible district · 1941h2			0.6944***	0.5212**
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	17,590	17,590	17,590	17,590
R^2	0.1326	0.1980	0.1357	0.2021
First Stage F-statistic (Instruments)	243.0828	174.5787	11.9630	10.2984

Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. 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: First stage effect, women

	Retirement years before the statutory retirement age			
Mean	6.4526	6.4526	6.4526	6.4526
Standard deviation	2.2675	2.2675	2.2675	2.2675
Eligible district	1.0104***	0.9399***		
Eligible district · 1934h2			0.6717**	0.5006*
Eligible district · 1935h1			0.3345	0.3267
Eligible district · 1935h2			0.4140	0.2519
Eligible district · 1936h1			0.7349**	0.4873*
Eligible district · 1936h2			0.7092**	0.6789**
Eligible district · 1937h1			1.1734***	0.9597***
Eligible district · 1937h2			1.2091***	1.0138***
Eligible district · 1938h1			1.1244***	0.9275***
Eligible district · 1938h2			0.9883***	1.1578***
Eligible district · 1939h1			0.8349***	0.8560***
Eligible district · 1939h2			1.6288***	1.6173***
Eligible district · 1940h1			1.1276***	1.1186***
Eligible district · 1940h2			1.3271***	1.2925***
Eligible district · 1941h1			1.2916***	1.3071***
Eligible district · 1941h2			1.1064***	1.0060***
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	3,283	3,283	3,283	3,283
R^2	0.1721	0.2489	0.1779	0.2558
First Stage F-statistic (Instruments)	153.1078	137.4533	11.9306	11.2351

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. 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 6: 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**
	(0.0011)	(0.0011)	(0.0088)	(0.0106)	(0.0078)	(0.0086)	(0.0086)	(0.0096)
Log(average yearly earnings (age 43-49))		−0.1001***		−0.1003***		−0.1001***		−0.1001***
		(0.0138)		(0.0140)		(0.0138)		(0.0138)
Log(std. dev. of yearly earnings (age 43-49))		0.0172***		0.0174***		0.0173***		0.0173***
		(0.0047)		(0.0047)		(0.0047)		(0.0047)
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. Robust standard errors in parentheses. 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: 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
	(0.0021)	(0.0023)	(0.0096)	(0.0104)	(0.0089)	(0.0095)	(0.0094)	(0.0099)
Log(average yearly earnings (age 43-49))		0.0152		0.0212		0.0202		0.0205
		(0.0146)		(0.0162)		(0.0159)		(0.0160)
Log(std. dev. of yearly earnings (age 43-49))		−0.0016		−0.0026		−0.0024		−0.0025
		(0.0062)		(0.0062)		(0.0062)		(0.0062)
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. Robust standard errors in parentheses. 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 8: 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
	(0.0086)	(0.0027)	(0.0037)	(0.0037)	(0.0011)	(0.0072)
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. Robust standard errors in parentheses. 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 A.3 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 9: 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
	(0.0181)	(0.0189)	(0.0174)	(0.0182)	(0.0145)	(0.0165)	(0.0127)	(0.0145)
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. Robust standard errors in parentheses. 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: Earliest possible withdrawal from the labor force

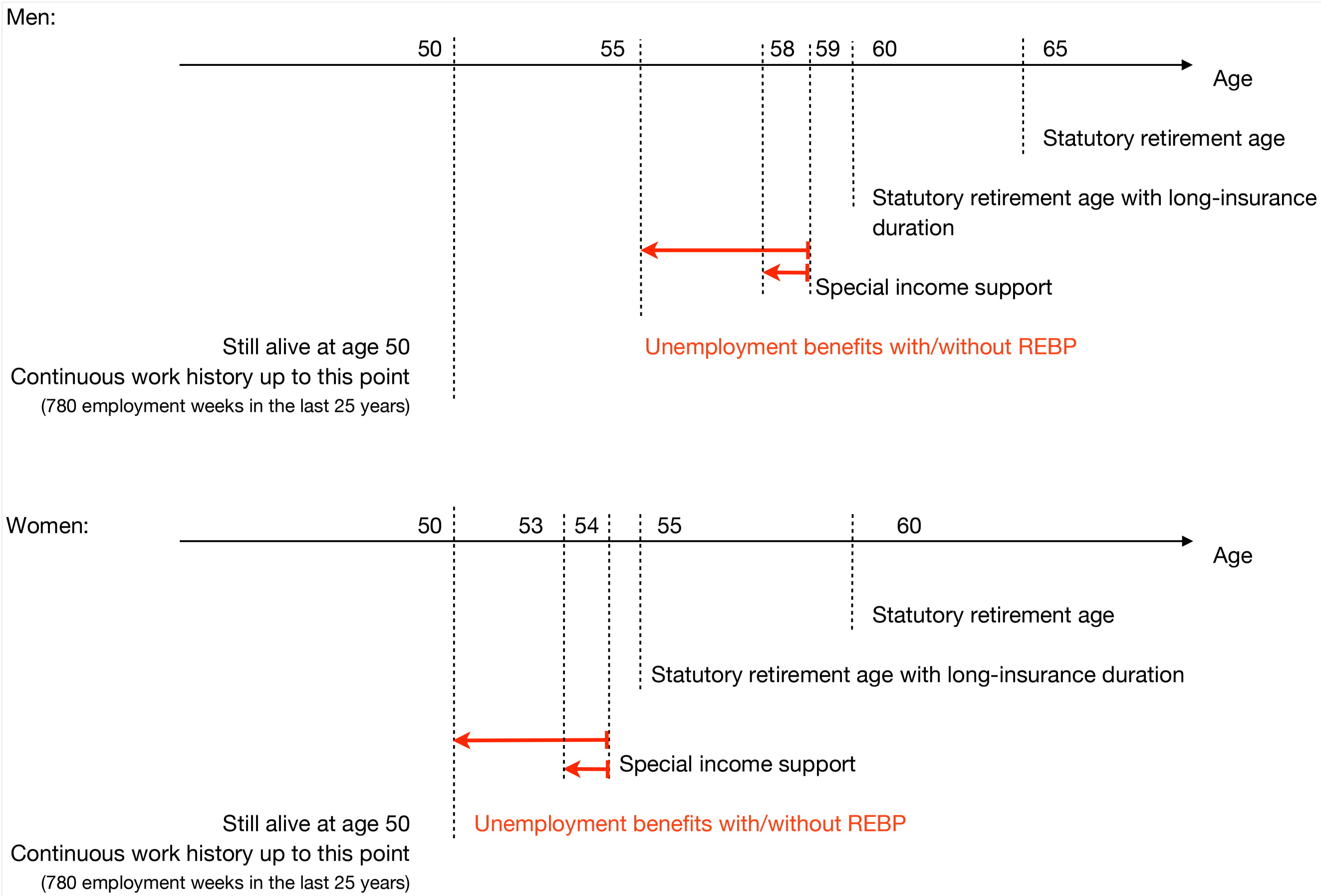
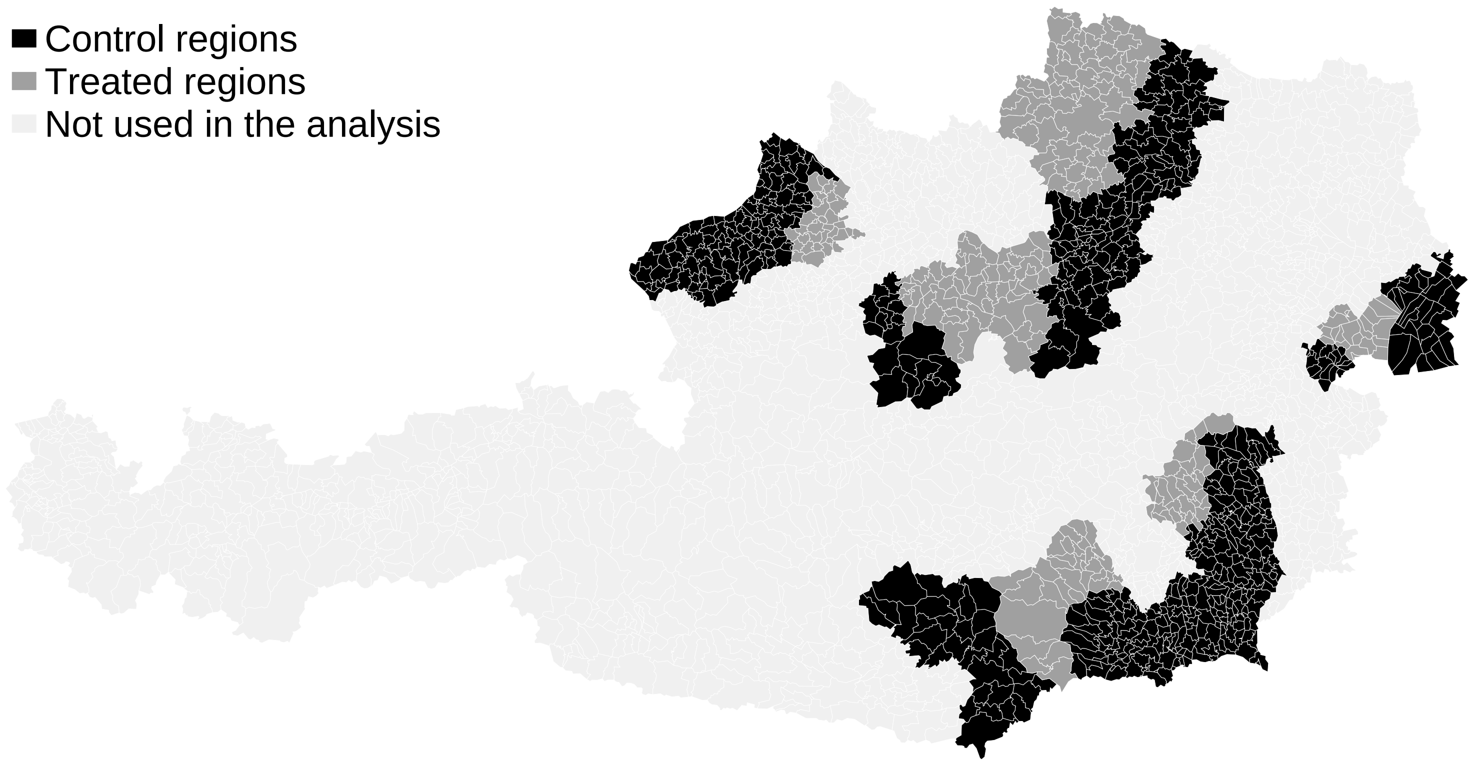
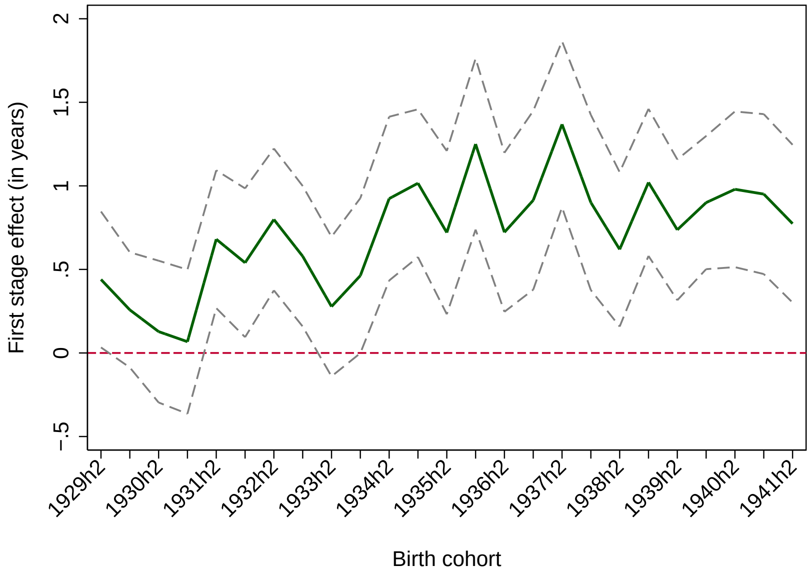


Figure 2: The Regional Extended Benefit Program (REBP)

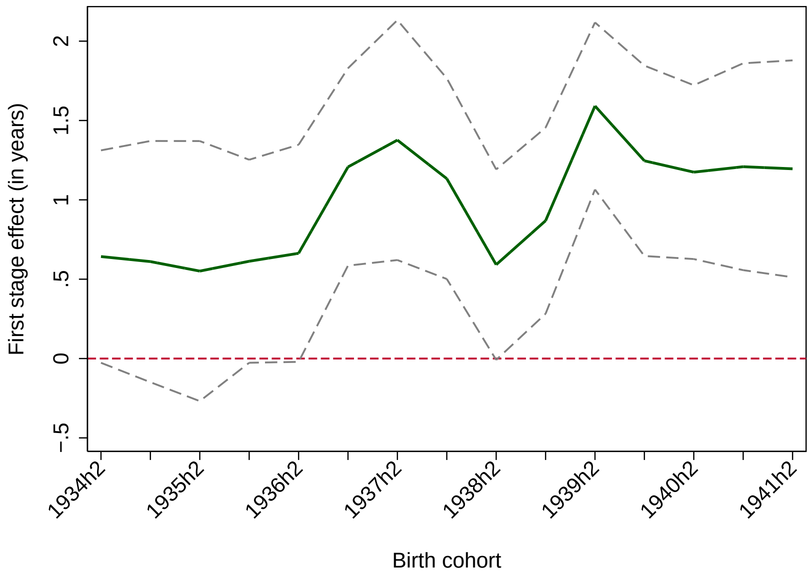


Notes: The figure shows which communities were, or were not, eligible to the REBP among those eight NUTS-3 regions that cover both eligible and non-eligible communities). Communities shaded in black (dark gray) were (were not) eligible to the REBP. Regions shaded in light gray denote communities that 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 3: Fist-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.

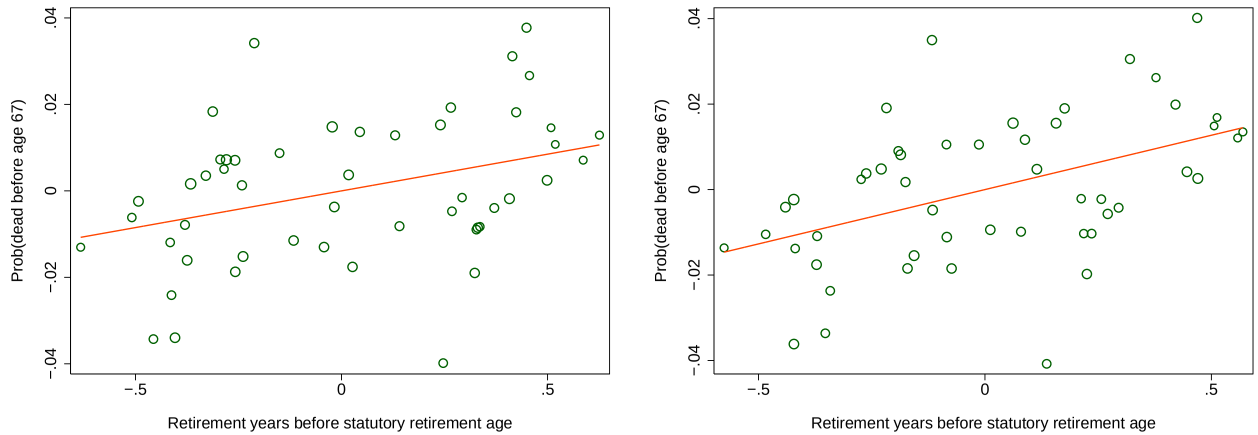
Figure 4: Treatment intensity

(a) Men

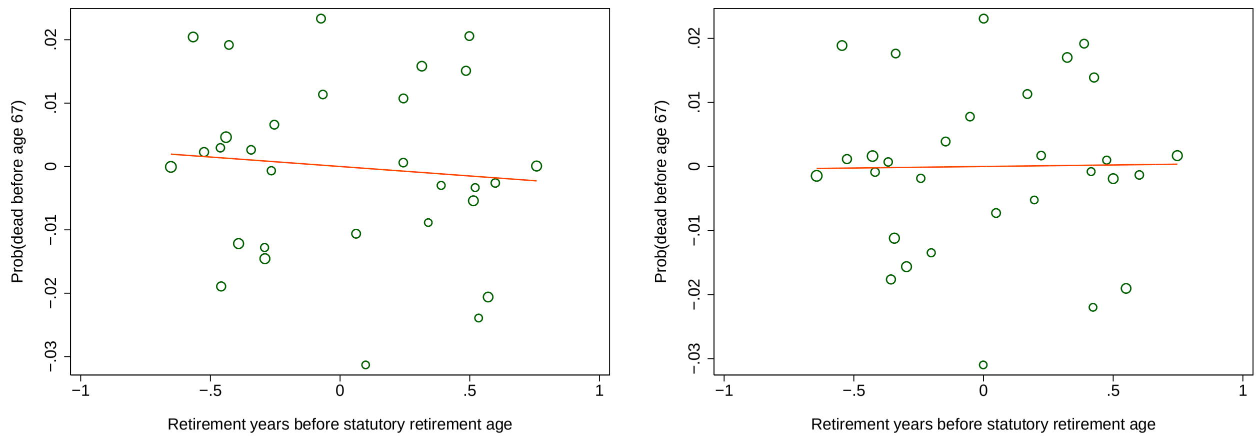
(b) Women

Notes: The 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.

Figure 5: Visual representation of IV-estimates



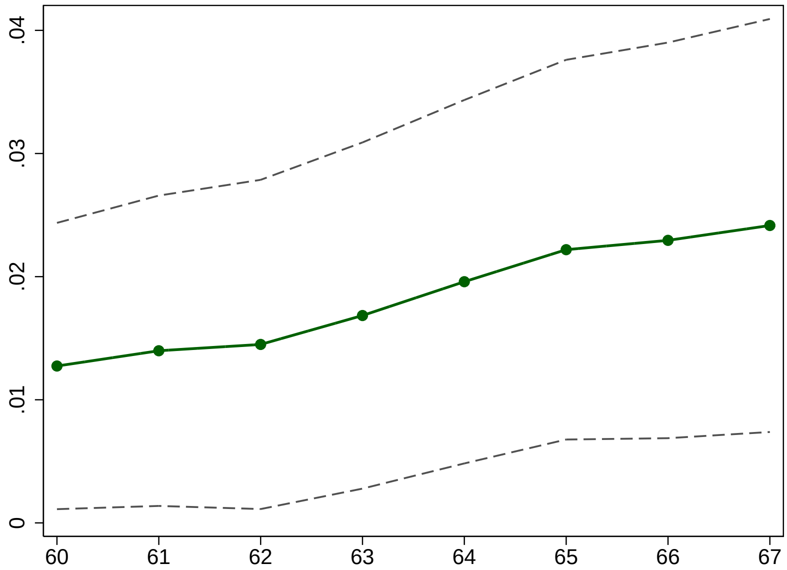
(a) Men



(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.

Figure 6: 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 6).

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
	(0.1115)	(0.0800)
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. Robust standard errors in parentheses. 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^{***}
	(0.0011)	(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. Robust standard errors in parentheses. 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 U.S. 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 U.S. 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 8), 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
	(0.0072)	(0.0009)	(0.0026)	(0.0010)	(0.0039)	(0.0018)	(0.0015)	(0.0019)	(0.0050)
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

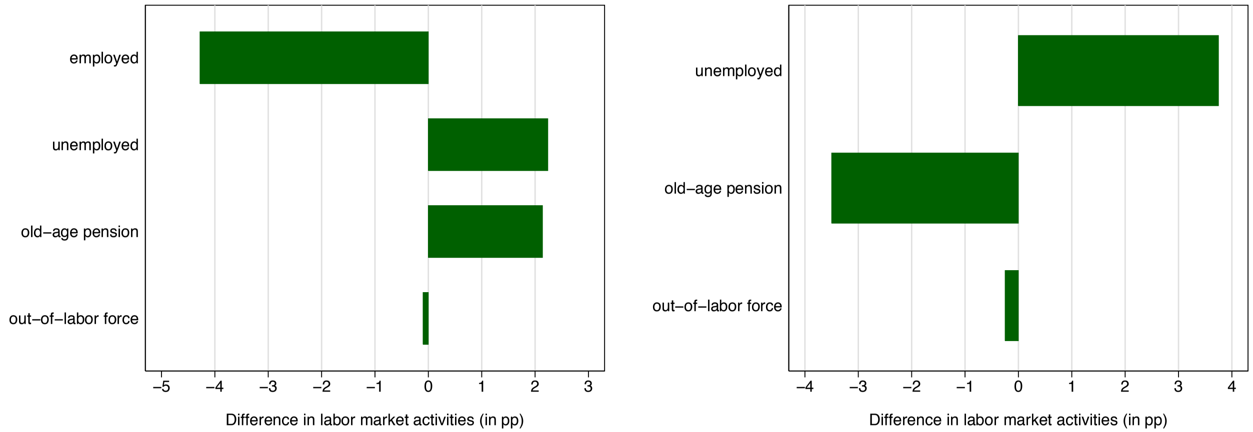
Notes: ***, **, * denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. A missing R^2 denotes a negative R-squared. The causes of death are classified by means of ICD-9 and ICD-10. "Other causes" are defined as described in the notes of Table 8. Columns (2)–(9) disaggregate the diseases included in the category "other causes" in column (6) of Table 8 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 8. Hence, these subcategories are, taken together with the categories in columns (2)–(5) of Table 8, exhaustive and mutually exclusive.

Table A.5: Health predisposition

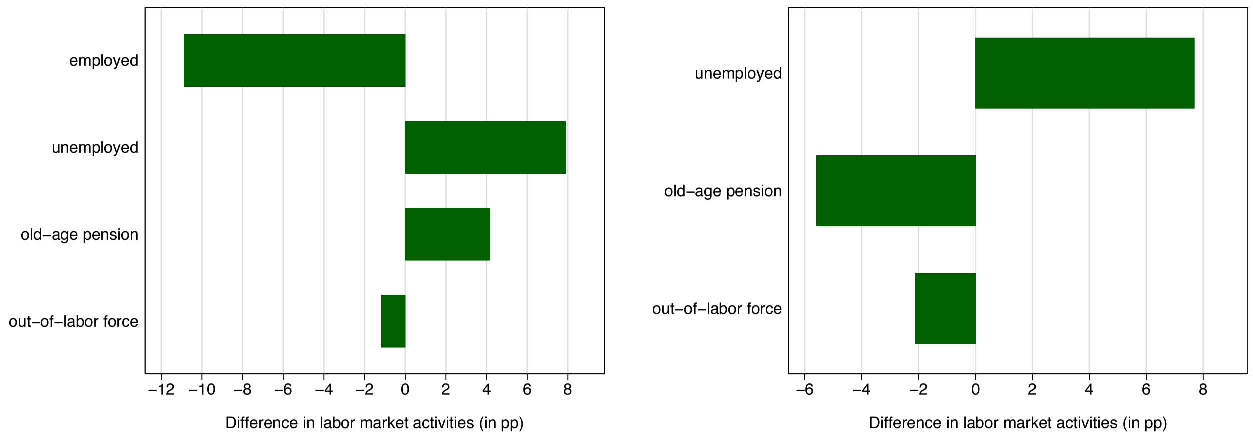
	Death before age 67			
Sick leave days (past 10 years)	Below median		Above median	
Mean	0.1481	0.1481	0.2117	0.2117
Standard deviation	0.3553	0.3553	0.4085	0.4085
Retirement years before age 65	0.0072	0.0122	0.0254**	0.0340***
	(0.0110)	(0.0118)	(0.0109)	(0.0120)
Cohort fixed-effects	Yes	Yes	Yes	Yes
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. Robust standard errors in parentheses. 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: Labor-market activity after age 50 and since entry into retirement, respectively



(a) Men



(b) Women

Notes: The figures show differences in specific labor-market activities between eligible and non-eligible districts (in percentage points), for men and women separately. The figures on the left show differences in activity from age 50 until min(age 65, death); the figures on the right show differences in activity from the effective retirement age until min(age 65, death).