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Does Retirement Age Impact Mortality?*

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Abstract

The relationship between retirement and mortality is studied with a unique administrative data set covering the full population of Norway. A series of retirement policy changes in Norway reduced the retirement age for a group of workers but not for others. By employing a difference-in-differences framework based on monthly birth cohort and treatment group status we first establish that the early retirement program significantly reduced the retirement age – this remains true when we account for program substitution, for example into the disability pension. Using instrumental variables estimation we find that retirement age has no effect on mortality.

JEL Classification: H55, I10, J11, J26

Keywords: Retirement age, Mortality, Instrumental variables, Policy evaluation

1. Introduction

Is there a causal link, positive or negative, from retirement age to mortality? Leaving employment may involve reduced stress and greater enjoyment of life, suggesting that early retirement enhances longevity. However, it may also lead to reduced mental and physical activity, loss of social networks, and health-adverse habits, suggesting that later retirement may extend expected lifespan.

Increasing life expectancy, especially at older ages, is imparting a new urgency to this question. Many OECD countries, looking ahead to the burgeoning fiscal burden of social security entitlements, have responded to increasing longevity by raising the statutory pension age; others have announced future increases (OECD 2011). To the extent that pension access age influences actual retirement age, economic assessment of these policy reforms requires evidence about whether, how, and to what extent such changes affect life expectancy.

While many papers address the relationship between retirement and mortality, the existing literature has thus far not succeeded in providing definitive guidance on its nature. This is primarily because health status influences both the timing of retirement and mortality. While early retirement may influence longevity, poor health may both induce a worker to retire and lead to an earlier death. Controlling for the ensuing selection bias is difficult, and until recently, attempts to do so have been unconvincing. As well, data sources vary in their time span and reliability, and data records sometimes do not extend to late ages.

Recently, however, a number of studies have adopted approaches which take seriously the endogeneity of health status and retirement. Typically, policy changes such as differential retirement ages by cohort, region or industry, have been enlisted as instruments. However, since involuntary retirement may also occur in early retirement programs, it is important to separate the potential effect of an early retirement program as such – which should be related to the voluminous literature on the effects of job-loss – from the potential effect of a change in the retirement age. In order to isolate the effect of the retirement age on mortality we require exogenous variation in the (entitled) retirement age *conditional on participation in an early retirement program*, compared with a group facing no such change, to capture time trends.

This paper combines such a research design with a unique administrative data set covering the entire population of Norway from 1992 to 2008. The data include highly reliable information on earnings, pension and labour market status as well as demographic information, such as birth and mortality dates, gender, education, and marital status.

Between 1989 and 1998, Norway progressively introduced an early retirement scheme for some employers, while for others, the official retirement age remained at 67. We use this gradual and differential change in policy to investigate whether the early retirement opportunity generated significant differences in mortality between the groups, using an approach based on instrumental variables (IV) and difference-in-differences. Focusing on the cohorts born between 1928 and 1938 we construct a treatment group for which the *entitled retirement age* (ERA) fell from 65, via 64, 63 and finally to 62 years in 1998, and a control group for which the ERA remained 67 throughout.

From this quasi-natural experiment we first study the impact of the fall in ERA on *actual retirement age* (ARA, defined as the age when a person was last observed working). Importantly, we take into account all forms of program substitution, since early retirement may serve as a substitute for disability pension and other social insurance programs. From this first-stage analysis we find, unsurprisingly, that lowering the entitled retirement age clearly and significantly reduces the actual retirement age. The mapping from entitled to actual retirement age is however well below one to one.

Secondly, we study the impact of exogenous reductions in retirement age on mortality using the ERA as an instrumental variable for the ARA. Our data records mortality up to age 77 for some cohorts, well above most other studies in this field. We also decompose the data to perform separate analyses by gender, marital status, industry and education. Our instrumental variable estimates consistently fail to reject the null hypothesis of no causal effect of retirement age on mortality, despite a strong first stage and relatively precisely estimated coefficients. We also conduct several robustness and sensitivity tests, including different treatment group ERA-margins (65 to 64 and 64 to 62), employer fixed effects, and controls for whether or not the employer downsize (as indications of involuntary job loss), all of which support our main findings.

The paper proceeds as follows: Section 2 surveys some related empirical studies of the relationship between retirement age and mortality. Section 3 describes the institutional setting and the data, and gives an empirical overview of retirement age and mortality in Norway. Section 4 presents the empirical strategy and discusses the identifying assumptions, before the main results are presented in Section 5 together with several tests for robustness. It also presents results from separate estimations on a number of subgroups. Section 6 concludes.

2. Previous literature

The literature relating retirement, health, and mortality is vast and until the last 10 years or so has developed seemingly independently of policy considerations. Shim et al. (2010), undertaking a systematic review of retirement as a risk factor for mortality, identified more than 1100 studies on the topic. But only a small proportion of these survived their filtering processes. They report that the surviving research studies did not allow firm conclusions to be drawn regarding the link between specific categories of retirement and mortality, although they found that "all-type" retirement, which includes health induced retirement, was a risk factor for mortality. They conclude that there is a "critical" need for further research.[1]

Several studies also recognise the simultaneous influence of health status on retirement and mortality, but to date, have been similarly inconclusive in identifying the nature, if any, of a direct retirement-mortality link. Waldron (2001) defines early retirement as taking benefits at various ages prior to 65, and finds that early retirement among men in the US is associated with higher mortality. He speculates that this may be a manifestation of optimising behaviour. Hurd and McGarry (2002) find that individuals' subjective survival probabilities roughly predict actual survival. A positive correlation between age of retirement and life expectancy might be expected if individuals were retiring in light of their longevity expectations. On the other hand, some of these studies find no impact of retirement age on longevity (Tsai et al. (2005), Litwin (2007)).

Controlling for health status to avoid the simultaneity bias, Brockman et al. (2009) report differential effects of early retirement, depending on the health status. Among women without reduced earnings capacity earlier retirement reduces mortality. On the other hand, using a similar approach, Quaade et al. (2002) find among early retirees initially "normal" but increasing mortality, interpreted as a negative effect of early retirement. Bamia et al. (2007) base their analysis on a Cox hazard regression approach with controls for various heath conditions, and also find early retirement to be strongly associated with higher mortality.

[1] Shim et al. (2010) also point out that the term "retirement" is not always used in the same way, leading to further confusion in studies focused on its mortality impact.

These results all hinge on the assumption that retirement is uncorrelated with present or future mortality risk, after controlling in various ways for pre-retirement health status. Selection processes beyond this are discussed, but not modelled.

A recent study based on Norwegian data (Skirbekk et al. (2010)) attempts to circumvent the selection problem by including only those who work at age 60, live beyond age 70 and do not receive disability pensions. This will reduce selection into retirement of persons in such bad health that they die before age 70, but will also leave out of the analysis any effect of retirement on mortality prior to this age. They find that early retirement is associated with higher mortality.

Among the studies based on instrumental variables approaches, Coe and Zamarro (2011) use country specific early and normal retirement ages as an instrument for retirement behaviour in a regression discontinuity design. They find a positive association between early retirement and health status, but do not link this directly to mortality. Coe and Lindeboom (2008) use unexpected early retirement window offers to instrument for retirement behaviour and find no effect of early retirement on men's health or mortality, six years after retirement.

Kuhn et al. (2010) rely on an institutional change in Austria, increasing access to early retirement in the form of extended duration of unemployment benefits in certain regions. In an IV analysis following blue collar workers up to age 67, they find significantly higher mortality among early retirees: The proportion surviving until 67 was 13 percent lower among men who retired early, but there was no difference among women. However, the authors point to evidence suggesting that their findings may to a large extent be related to involuntary job loss.

The most recent study of which we are aware is a preliminary paper by Bingley and Pedersen (2011). They exploit the introduction of an early retirement program in Denmark to instrument both retirement and income. Using population based administrative data on blue collar workers they find that those induced to retire early by the program have subsequently better health and reduced mortality before age 80.

3. Institutional setting and data

Trends in retirement and mortality

The combined trends of statutory retirement age and life expectancy at retirement for selected OECD countries are shown in Figure 1.[2] The statutory retirement age was at a minimum, on average, about the end of last century, after which there is a slow upward trajectory. Life expectancy at the official retirement age, however, continues to increase, in spite of the retirement age increases enacted and foreshadowed through the first half of the 21st century.

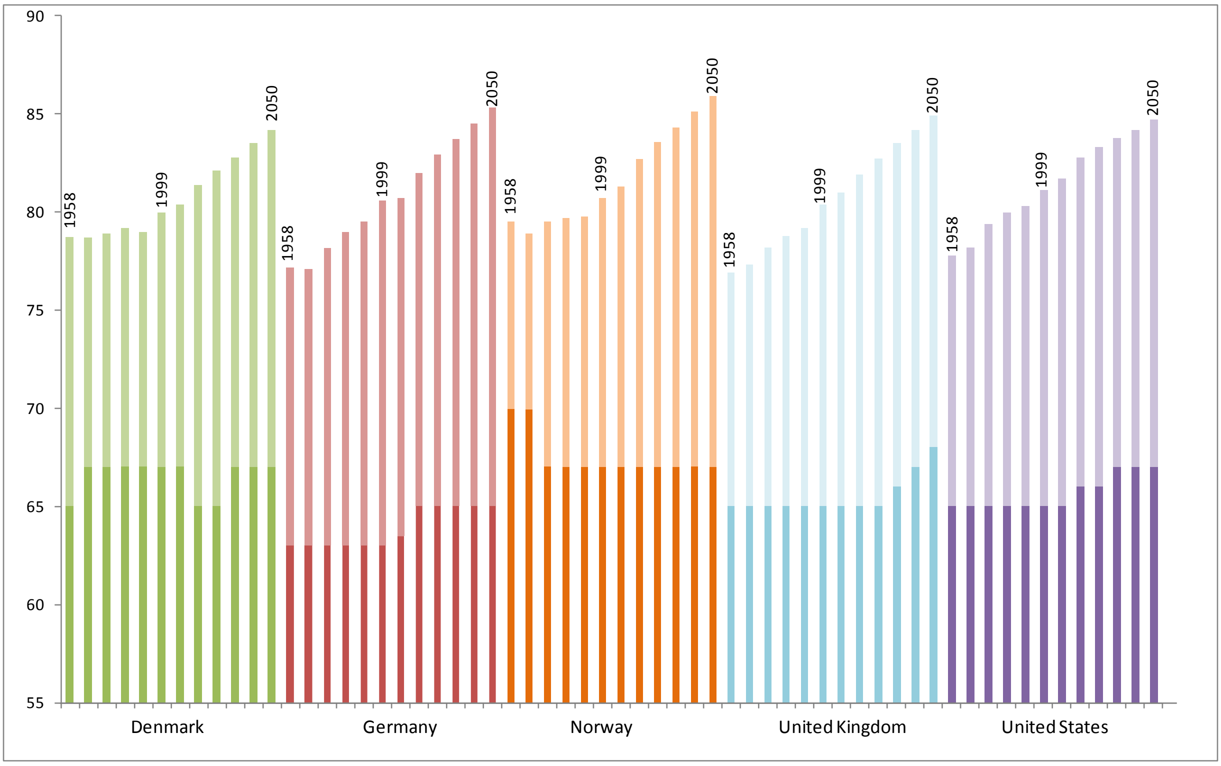

Figure 1: Men's pensionable age and life expectancy at official retirement age in selected OECD countries (1958 – 2050)

A recent paper projects an increase in the expected number of years in retirement from 18.5 years in 2010 to 20.3 years in 2050 on average for OECD countries (Chomik and Whitehouse (2010))

Norway and Denmark have the highest statutory retirement ages, although both have introduced large scale earlier retirement possibilities for at least some of their older workers. In common with all major economies, Norway shows a steady upward trend in life

[2] Data source, OECD. Note that the statutory retirement age may vary between groups of workers. E.g. for Norway, a substantial group of workers have access to early retirement (ER) and may retire before age 67, which is exactly what we use for estimation below.

expectancy at retirement. The Norwegian experience is in this critical dimension very similar to that of major OECD countries.

Institutional setting

From 1 January 1989 an early retirement program came into operation as a result of the central tariff negotiations between the employers' and employees' organisations. The program covers the entire public sector and private sector firms taking part in the central tariff agreements. In the early nineties participating firms employed about half the workers in the private sector. Program costs are shared between the government and the participating employers (by means of funds financed by contributions proportional to total wages paid), and regulations ensure that the program has the same structure in all participating firms and that all workers in participating firms are covered.

In order to be eligible, an individual has to be employed by a participating firm up until the time of retirement (institutional criteria), have reached the eligibility age, and meet certain individual labour market history requirements.

From the general retirement age at 67, the program lowered the eligibility age for those covered to 66 with effect from 1 January 1989, to 65 from 1 January 1990, to 64 from 1 October 1993, to 63 from 1 October 1997, and to 62 from 1 March 1998. Hence, month by month, parts of new cohorts sequentially qualified. Given that all eligibility criteria are met, individuals may apply for early retirement from the month after they reach the eligible age. The replacement rate decreases with earnings; average replacement rates, net of taxes, for early retirement benefits are around 70 percent, which makes early retirement a rather attractive option relative to other informal exit routes[3].

Data sources and definitions

We base our analysis on individual data from administrative registers owned and maintained by Statistics Norway. These files are linked by a unique personal identification number[4], and cover the entire population of Norway. Demographic files contain both birth and death dates, gender, education and other demographic variables. Tax return files record income from

[3] Røed and Haugen (2003) find that average replacement rates, net of taxes, for early retirement benefits, disability pension benefits and unemployment benefits are 72, 64 and 62 percent, respectively. Sickness leave could be seen as another informal exit route which gives a benefit replacement rate of 100 percent, but for a maximum duration of 12 months.

[4] This number is an encrypted version of the official personal identification number, and is only used for the internal linking of files at the Frisch Centre. Permissions for use have been given by the data owners as well as by the Norwegian Data Inspectorate.

various sources, including wage, pensions, sick-leave, disability, and unemployment benefits. In this study, we primarily use data from 1999 to 2008. We also have access to individual earnings data (in terms of pension points) dating back to 1967, which we use to identify firms participating in the early retirement program.[5]

The *actual retirement age* (ARA) can be defined either in terms of the take up of pension or other benefits or as sharply reduced earnings (indicating substantial labour force withdrawal), or a combination of the two. Our basic earnings data consist of annual employer-specific earnings for each employee, with dates for start and stop of each employment spell. The annual amounts are the basis for the income tax and are considered reliable, even if within year dates are considered less reliable. Yet we have used these dates to allocate earnings over months each year, adding up multiple employment spells for each person. Hence, for a limited number of workers leaving employment without receipt of any pensions or benefits, such as early retirement pension, ordinary age pension, temporary or permanent disability or unemployment, the actual timing (month) of retirement within a year is measured with some error[6]. We also have data on a monthly basis for receipt of unemployment benefits, disability benefits and pensions. ARA is defined as the last month of regular work, without receipt of any of these benefits.

Estimation sample

Included in our dataset are all workers employed in the month in which they reach the early retirement age of their monthly birth cohort, regardless of whether they have access to early retirement. For the first cohort, born in January 1928, the early retirement age is 65. We thus sample all workers born in January 1928 and employed in January 1993. Workers in this cohort may, if eligible, leave employment with early retirement pensions from February 1993 at the earliest.

Columns 2 and 3 of Table 1 display the fraction of the yearly birth cohorts still resident and employed at age 62. There has been a substantial decrease in mortality up to this age over our

[5] We make use of the fact that all workers of a participating firm are automatically covered, and identify participating firms by tracking previous employment of individuals observed to be receiving early retirement pensions, using the unique organizational firm number which is part of the employment information of individuals.

[6] Monthly working status is defined as having earnings corresponding to an annual level of at least 1 Basic Amount. The Basic Amount is frequently referred to as G and is a central feature of the public pension system in Norway. It is adjusted every year, with a nominal rate of growth varying between 2 and 14 % since its introduction in 1967. The average Basic Amount for 2010 was 74,721 NOK, which corresponds to about 12,500 (9,800) USD (EUR).

sample period and the fraction still resident at age 62 has increased from 80.9 percent to 85.9 percent. The employment rate at age 62, however, has remained at about 42 percent.

Since our dataset used for estimation only includes those employed at their monthly birth cohorts' early retirement age the earliest cohorts are sampled at higher ages than the latest cohorts. Consequently, the number of workers included in our sample, displayed in Column 5 of Table 1, increases from 11,082 born in 1928 to 18,022 born in 1938. The fraction eligible for early retirement remains roughly constant, as displayed in Column 6.

Table 1: Mortality trends, sample size, entitled retirement age and employment

Birth year (1)	Percentage resident at age 62 (2)	Percentage of residents employed at age 62 (3)	Mean early retirement age (ERA) (4)	Sample: Employed at early retirement age (ERA) (5)	Percentage of sample with an employer that offers early retirement (6)
1928	80.9		65.0	11,082	80.3
1929	81.9		64.3	11,568	82.2
1930	83.0		64.0	12,906	81.4
1931	83.5	41.5	64.0	12,661	82.3
1932	83.9	42.3	64.0	12,860	80.9
1933	84.6	44.9	64.0	12,053	82.2
1934	84.0	42.0	63.3	12,255	82.3
1935	84.7	42.2	62.7	12,721	83.0
1936	86.1	42.1	62.0	15,549	81.9
1937	85.7	42.3	62.0	16,966	82.1
1938	85.9	41.8	62.0	18,022	82.5
All			63.3	148,643	81.8

Note: For the cohorts 1928-1930 we are able to track employment from age 64 and 65, when they reached early retirement age and entered the sample, but we lack details on employment at age 62 as the registers cover only the years from 1992 and onwards. Percentages resident have been extracted from "Statistikkbanken" at Statistics Norway.

Treatment and control groups

We allocate all workers in the sample to either a treatment group or a control group according to each worker's affiliation with the early retirement program: Treatment group workers are those in participating public and private sector firms while control group workers are those in non-participating private sector firms.[7] Table 2 presents some descriptive statistics for the treatment and control group workers. Treatment group workers are more educated, they are more often working in the services sector and more likely to be defined as white-collar

[7] Although individual eligibility for early retirement is determined by both firm affiliation and criteria related to individual work histories we have chosen to define treatment and control groups based on firm affiliation only. The reason is that fulfilment of the individual criteria might be somewhat more susceptible to individual adjustments than is firm affiliation, as worker mobility is extremely low at the relevant ages, especially across participating and non-participating firms (see Røed and Haugen (2003)). About 95 percent of the workers in our sample do meet the individual criteria, and conditioning on these does not alter our results.

workers[8]. One should bear these compositional differences in mind, but also note that what matters for our identification is that these have not changed dramatically over time, a point to which we return below. Mortality is rather similar across treatment and control groups at all ages (67, 70, 74, and 78).

Table 2: Descriptive statistics

	All	Treatment group	Control group
Females %	47.5	49.0	40.7
Earnings the year before early retirement age (USD, 2011)	56,423	56,635	55,468
Education			
Compulsory only	31.0	31.2	29.8
Upper secondary / high school	46.3	44.3	54.9
College / University	22.8	24.4	15.2
Sector of employment			
Manufacturing / transport	43.8	38.7	67.1
Services / public sector	55.5	60.7	32.3
Blue-collar workers	39.4	34.5	61.1
White-collar workers	18.6	20.5	9.6
Average retirement age	65.0	64.8	66.0
Mortality			
By age 67	2.8	2.8	2.9
By age 70	5.9	5.9	6.1
*By age 74**	11.5	11.5	11.9
*By age 78**	20.4	20.2	21.1
Number of observations	148,643	121,598	27,045

*Mortality at age 74 and 78 are observable only for cohorts born before 1935 and 1931.

The upper panel of Figure 2 depicts the ERA for each of the 132 monthly birth cohorts included, divided into treatment (red, solid) and control (blue, dashed) groups. The gap in ERA was initially two years and increased non-linearly to five years for the most recent cohorts. Panel (b) shows the ARA for the same groups, measured as the average number of years of employment after reaching the ERA of the treatment group. As the gap in ERA increases from two to five years, the corresponding gap in ARA increased from less than one

[8] We do not observe the nature of different jobs directly, but use a combination of educational attainment and sector codes to distinguish between blue and white collar jobs: Blue collar workers are workers with low education working in the manufacturing or transport sectors, while white collar workers are those with high education (university or college) working in the services sector.

year to almost two years. The relationship between ERA and ARA will form the first stage in an instrumental variables model in the analysis below.

There are at least three reasons why an increasing gap in ERA does not increase the gap in ARA on a one-to-one basis. First, many workers choose not to retire as soon as they become eligible for social security. Second, for some workers early retirement will replace other informal exit routes, such as disability pensions. Third, workers in the control group may also have been affected by reductions in the ERA in the sense that they may have felt more entitled to leaving employment with, for example, disability pensions as the control group left through early retirement. Table 3 provides additional information regarding the different exit routes for three selected birth year cohorts; 1928, 1932, and 1938. Whereas the fraction leaving employment with a disability pension was the same between the treatment and control groups among those born in 1928, leaving with disability pension was much more common in the control group for those born in 1938.

Table 3: Labour market exit routes for the cohorts of 1928, 1932 and 1938

	1928 Cohort Sampled at age 65		1932 Cohort Sampled at age 64		1938 Cohort Sampled at age 62	
	Treatment	Control	Treatment	Control	Treatment	Control
Formal retirement age	65	67	64	67	62	67
Actual retirement age (ARA)	66.3	66.9	65.4	66.3	63.7	65.4
Employed up to age 67	33.5	54.8	19.5	43.4	13.1	35.2
Leaving before age 67 with disability pension	18.4	18.5	19.8	26.8	22.1	34.5
Leaving before age 67 with unemployment benefits	2.2	6.9	1.0	6.0	0.9	7.6
Leaving before age 67 with early retirement pension	30.0	0.0	51.0	0.0	56.0	0.0
Leaving before age 67, other (including death)	15.8	19.7	8.7	23.8	8.1	22.7

Finally, panel (c) of Figure 2 shows mortality, measured as the proportion of the cohort deceased by age 70. Since the earlier cohorts are sampled at a higher age than the later cohorts, the former are positively selected. Consequently, the figure suppresses the substantial decrease in mortality over this period. What it does show, however, is that the increasing gap in the ARA between the treatment and control groups does not feed into changes in mortality between the groups, which from this seems unrelated to the changes in retirement age.

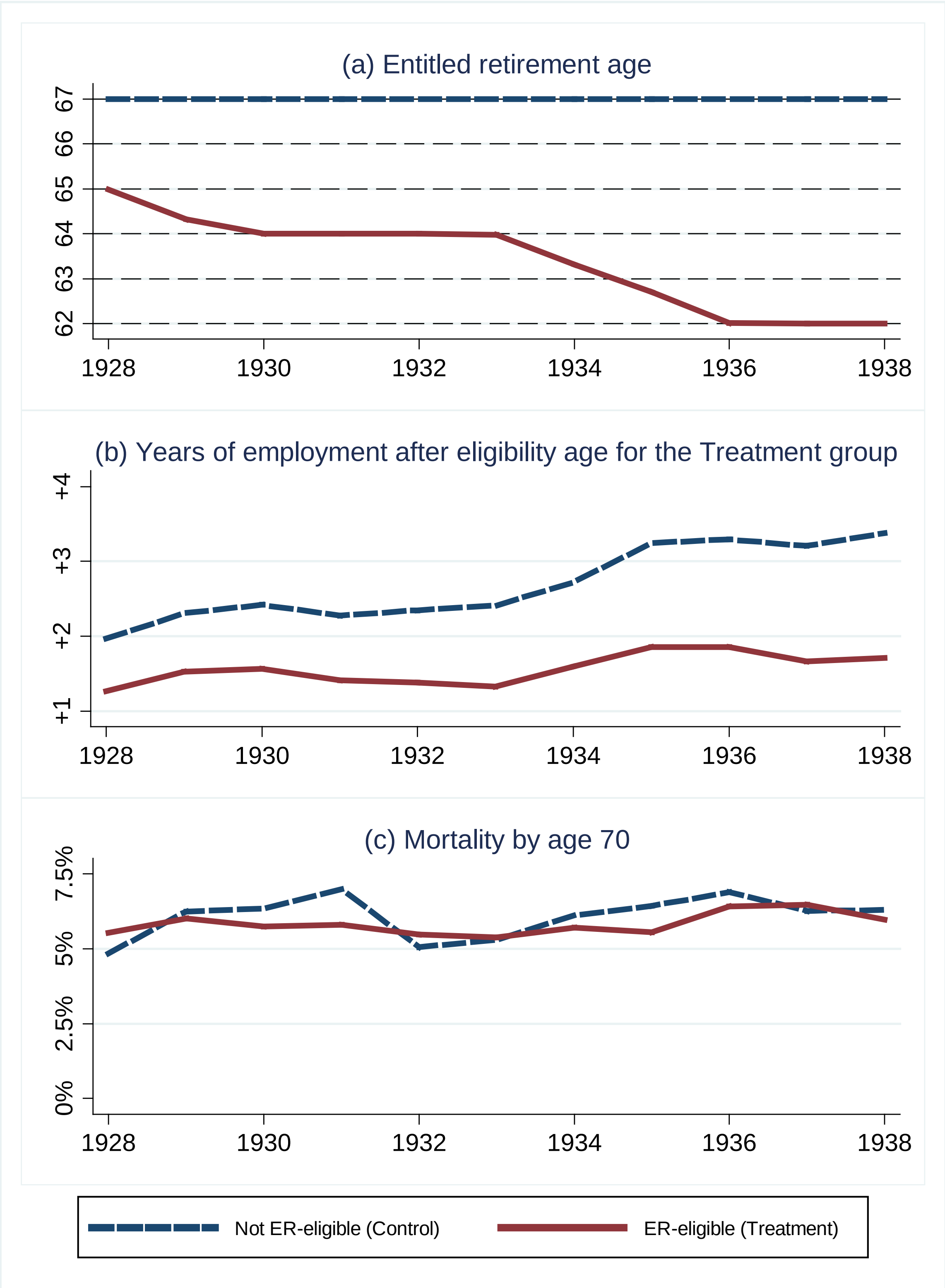

Figure 2: Retirement age and mortality in the sample by cohort, for treatment and control group workers.

4. Econometric model and identification

A natural starting point for studying the relationship between retirement age and mortality would be to estimate equation (1) by means of linear regression, where y^a is an indicator for whether or not the person lived through age a, x is a set of control variables and *ARA* is the actual (observed) retirement age:

$$(1) \qquad y^a = x\beta^0 + \theta ARA + \varepsilon$$

There are, however, good reasons to suppose that this strategy would provide biased results. First, since survival is a prerequisite for employment, (1) will suffer from reverse causality. Second, if the vector of observable control variables is incomplete there may be unobserved variables affecting both retirement age and mortality. This will lead to omitted variables bias. Health could be one such variable, and even with access to self-reported or other health information it is hard to believe that the *x*-vector would be anywhere close to complete.

To obtain trustworthy estimates for the relationship between retirement age and mortality, we make use of an identification strategy that exploits variation in retirement age not caused by unobserved confounders. The stepwise reductions in the ERA of our treatment group provide exogenous variation in retirement opportunities, which we use in an instrumental variable framework. Since early retirement is available only for workers in the treatment group, these stepwise reductions were only affecting a fraction of the workers, whereas the control group still faced a formal retirement age of 67. Technically, the ERA is a non-linear function of treatment group status and birth-month such that it is possible to jointly estimate common birth-month effects for the treatment and control groups, a time-constant treatment group dummy and the effect of the ERA. The identification is thus built on a *difference-in-differences strategy* (DD).

The first stage in our instrumental variables model is given by equation (2), where *C* is a set of dummy variables for birth month, *T* is a dummy for the treatment group, *x* is a vector containing a number of individual characteristics and *ERA* is the eligible retirement age which is 67 for the control group and between 65 and 62, depending on the birth cohort, for the treatment group:

$$(2) \qquad ARA = x\beta^1 + C^1 + \lambda^1 T + \gamma ERA + u$$

The second stage equation is given by equation (3) where ARA is replaced by predicted values from (2):

$$(3) \qquad y^{a} = x\beta^{2} + C^{2} + \lambda^{2}T + \theta^{IV}\widehat{ARA} + e$$

ERA is a valid instrument for ARA if it (i) has a first stage (i.e. if $\gamma \neq 0$) and (ii) affects mortality only through ARA (the exclusion restriction). Substantial labour supply effects of the early retirement program are well documented in the existing literature[9] and are readily confirmed by the reported first stage estimates in Section 5. As for the exclusion restriction, differences in mortality across treatment groups are captured by treatment group fixed effects and differences in mortality over time by cohort dummies. Hence, the validity of our instrument will only be called into question if there is a direct link between ERA and mortality arising from the *interaction* of time and treatment status.

In Figure 3 we inspect the treatment and control groups over time by plotting the fraction of females, average years of schooling, average earnings and average number of months with sickness benefits, all measured in the year prior to the ERA of the treatment groups. The left-hand column of Figure 3 shows the mean levels for each of the 11 birth cohorts, separately for treatment and comparison group workers. The right-hand column reports the difference in means (treatment minus comparison groups) for each cohort, relative to the difference in means for the first cohort, along with the corresponding 95% confidence intervals (vertical spikes). Despite differences between treatment and comparison group workers within cohorts, particularly in terms of the fraction of females and years of schooling, there are few signs of diverging trends. The changes in differences, relative to that for the 1928 cohort, are significantly different from zero only for the fraction of females in the 1937 cohort and the number of months with sickness leave-benefits for the 1929 cohort.

Clearly, all *observed* differences between the two groups will be captured by the extensive vector of observable characteristics, *x*, containing gender (1 dummy variable), marital status (6 dummy variables), education (16 dummy variables), industry (9 dummy variables), previous paid sick leave (12 dummy variables), previous earnings (2 continuous variables), pension points history (3 continuous variables) and public sector (1 dummy variable).

[9] For example, Hernæs, Sollie and Strøm (2000), Røed and Haugen (2003) and Bratberg, Holmås and Thøgersen (2004).

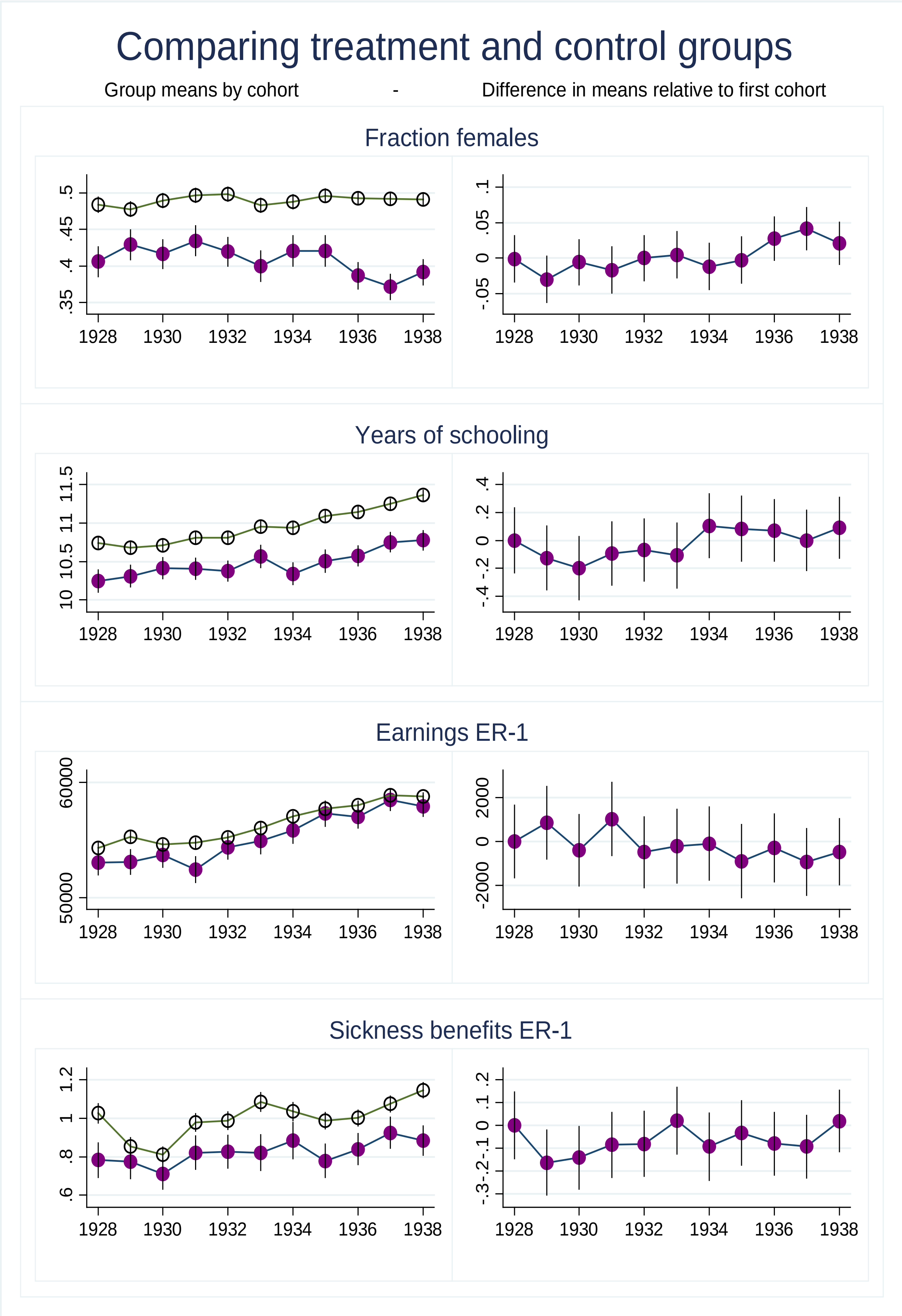

Figure 3: Comparing the treatment and control groups across cohorts.

5. Results

Estimation results for the effects of entitled retirement age (ERA) on actual retirement age (ARA), i.e. equation (2), the first stage, and for the effects of ARA on mortality by age 67, 70, 74 and 77 are given in Table 4. Starting with the first stage estimations, we note that ERA has significant effects on ARA in all four models, although the instrument is somewhat weaker when mortality is measured at later ages. This comes as no big surprise, as many observations and some of the variation in ERA is lost when we move from mortality at age 67 and 70 to mortality at age 74 and then further to 77.[10] The point estimates from the full sample is a precisely estimated 0.288, which implies that a one year increase in ERA increases ARA by about 3.5 months. This is a weighted average of the effects of gradual reductions in ERA from 65 to 62 for treatment group workers, relative to the counterfactual trend in retirement age approximated by the retirement ages of control group workers. The point estimate for the "Mortality by age 74 (77)" sample equals 0.238 (0.151), which implies that a one year increase in ERA increases ARA by about 2.9 (1.8) months.

Turning to the effects of ARA on mortality we first note that all four OLS estimates are significantly negative. They reveal that retiring one year later is associated with a 0.6, 0.8, 1.1 and 1.3 percentage point decrease in mortality by age 67, 70, 74 and 77, respectively, which in relative terms correspond to a 21%, 14%, 10% and 6% reduction in mortality at the respective ages. The importance of controlling for the negative health selection into early retirement becomes clear, however, when we compare OLS estimates with the 2SLS estimates: The two sets of estimates have opposite signs, but the 2SLS estimates are not significantly different from zero. Hence, while the OLS estimates consistently show that those who work longer also tend to live longer, the 2SLS estimates are equally consistent in showing that this relationship is not a causal one, but rather due to reverse causality or omitted variable bias. A complete presentation of all coefficients of the first and second stage is provided in the Appendix.

[10] For mortality at age 74 (77) only those born in 1934 (1931) and earlier are included, and the variation from the reduction of ERA from 63 to 62 (64 to 62) is lost.

Table 4: Estimation results for equations (1)-(3)

	Mortality by age 67		Mortality by age 70		Mortality by age 74		Mortality by age 77	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
First stage								
Entitled retirement		0.288		0.288		0.238		0.151
age (ERA)		(0.012)		(0.012)		(0.029)		(0.041)
F-statistic		568.6		568.6		66.7		13.7
Second stage								
Actual retire-ment	-0.006	0.002	-0.008	0.002	-0.011	0.025	-0.013	0.066
age (ARA)	(0.000)	(0.004)	(0.000)	(0.005)	(0.001)	(0.026)	(0.001)	(0.073)
Number of obs.	148,037	148,037	148,037	148,037	85,355	85,355	48,214	48,214

Note: Standard errors in parentheses. The dependent variable takes the value 1 for individuals who did not survive through age *a*, age *a* being 67, 70, 74 and 77 for columns 1-2, 3-4, 5-6 and 7-8, respectively. In addition to treatment group fixed effects and cohort dummies, all regressions are estimated with controls for gender, marital status, education, industry, previous paid sick leave, previous earnings, pension points history and public sector.

As discussed above, our identification strategy would be called into question if there is a direct link between ERA and mortality arising from the *interaction* of time and treatment status, as this would violate the *common trends assumption* (Angrist and Pischke 2009, p. 230). One possible way the common trends assumption could be violated is if the composition of the control and treatment groups changes over time, in a way that also alters the relative differences in mortality. The control group consists of workers in much smaller and perhaps less stable firms, and in order to investigate whether changes in the composition of employers affects our estimates we estimate the model using firm fixed effects. We also estimate the model on a subset of the data consisting only of employers that are present in all years. The results from these exercises are displayed in Table 5. We can see that the results are unaltered in both specifications and hence they provide additional support for our main results.

Another violation of the common trends assumption arises if workers in the treatment group, eligible for early retirement, were exposed to other business cycle conditions than those in the control group. There is a literature describing mortality and health effects resulting from (involuntary) job-loss[11], and if early retirement also made involuntary job-losses relatively more common in the treatment group, this could violate our identification strategy. Note, however, that our identification strategy is not violated by involuntary job-loss being more (or less) common in the treatment group than in the control group. It is only violated if the reductions in ERA contribute to making involuntary job-loss more (or less) common among

[11] For example, Gallo et al (2004), Salm (2009).

the treated. Involuntary job-losses could be disguised as (voluntary) early retirement if the employer gives the employee a "choice" between unemployment and early retirement. If so, our estimates could consist of two off-setting effects: a *positive* causal effect of retirement age on mortality, such that retiring early decreases mortality, and a *positive* effect of involuntary job-loss on mortality combined with such job-losses being more common when the early retirement age is reduced.

Unfortunately our data do not contain information on the specific reasons *why* people stop working. We have, however, constructed a measure to at least partly capture whether or not retirement can be considered voluntary: whether or not the employers send workers (of any age) into unemployment. The administrative data we have at hand contains all workers in Norway and we have for each employer-year observation calculated how many of those employed at the beginning of the year who later that same year experienced unemployment. Based on this measure we divide our dataset into those working in a firm not sending any workers out in unemployment and those sending at least one worker into unemployment. We also construct a subset consisting of workers in firms who sent at least 25 percent of their employees into unemployment. The results from estimating our IV-model on these three data subsets are displayed in Table 5. Note first of all that the first stage estimates are strong and not much different from those presented in Table 4. Neither are the second stage estimates much altered and none of them are significantly different from zero. However, if anything, the estimates indicate that as focus is shifted towards workers more likely of having experienced involuntary job-loss the point estimate is smaller, while as we move towards workers more probable of voluntarily labour market exits the point estimate is higher.

Our baseline models are somewhat restrictive in the sense that they assume the effect of changes in ERA on ARA to be the same across all margins. As the first stage estimates in Table 4 do indicate that this assumption might be called into question, we have estimated the models for sub-samples of workers that are selected in such a way that only the 65-64 and the 64-62 margins, respectively, are used for identification. The two subsets of data are constructed such that they do not overlap, containing the 1928-31 cohorts and 1932-38 cohorts, respectively. As can be seen from Table 5, the first stage is rather weak when we focus on the 65-54 margin solely, with an F-statistic of 8.2. One should thus be careful when interpreting the second stage estimates, which are much larger than in the other specifications but still not statistically significant on any conventional level. The estimate for the larger subset focusing on the ERA reduction from 64 to 62 is much in line with the other estimates presented.

Table 5: Exit-inducing firms, firm attrition and different margins of ERA.

	Employers sending workers into unemployment at the year of labor market exit			Composition of firms		Separate reductions in ERA	
	None	> 0	> 25%	Firm F.E.	Firms present all years	65 to 64 Born 1928-31	64 to 62 Born 1932-38
First stage							
Entitled retire-ment age (ERA)	0.235 (0.000)	0.219 (0.020)	0.255 (0.051)	0.237 (0.020)	0.207 (0.078)	0.121 (0.042)	0.190 (0.019)
F-statistic	215.0	115.4	24.7		7.04	8.2	104.04
Second stage							
Actual retire-ment age (ARA)	0.011 (0.009)	0.007 (0.014)	0.003 (0.031)	0.004 (0.012)	0.003 (0.053)	0.113 (0.072)	0.014 (0.013)
Number of obs.	62,222	76,624	3,534	148,021	52,589	44,708	90,138

Note: Standard errors in parentheses. The dependent variable takes the value 1 for individuals who did not survive through age 70. In addition to treatment group fixed effects and cohort dummies, all regressions are estimated with controls for gender, marital status, education, industry, previous paid sick leave, previous earnings, pension points history and public.

Previous studies have found different effects for different workers. Kuhn et al. (2010) found early retirement to strongly increase mortality among men in blue collar jobs, but found no effects for women. In order to investigate whether different groups are affected differently by the reductions in ERA we divide the dataset into a number subsamples and estimate the model separately for each of them. The results are reported in Table 6.

We first estimate the model separately for men and women. Interestingly, the reductions in ERA affected men much more than women. One possible reason is that women more often receive disability pensions so that they were either unaffected by the reductions in ERA, if they already were disability pensioners, or that early retirement substituted not yet realized uptake of such disability pension. The second stage estimates for men and for women are both close to zero in magnitude and also statistically insignificant. The model is also estimated separately for workers in the manufacturing and transport sectors, in services and in office jobs, for workers with high and low education, for workers in blue and white collar jobs, for men and women and separately in white and blue collar jobs, and finally separately for married and unmarried workers. In all subgroups but one, white collar women, the first stage is convincingly strong, with F-values well above the conventional threshold of 10.

However, in none of these specifications do we find statistically significant causal effects of the ARA on mortality.

Table 6: Gender, education, industry and occupation - mortality by age 70

	Men		Women		Low education		High education	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
First stage								
Entitled retire-		0.340		0.219		0.275		0.325
ment age (ERA)		(0.016)		(0.019)		(0.013)		(0.032)
F-statistic		466.1		131.8		453.9		99.9
Second stage								
Actual retire-	-0.010	0.007	-0.006	-0.011	-0.008	-0.001	-0.007	0.014
ment age (ARA)	(0.000)	(0.007)	(0.000)	(0.009)	(0.000)	(0.006)	(0.001)	(0.011)
Number of obs.	77,701	77,701	70,336	70,336	113,789	113,789	34,248	34,248

	Manufacturing and transport		Services and office jobs		Blue Collar workers		White Collar workers	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
First stage								
Entitled retire-		0.322		0.303		0.309		0.255
ment age (ERA)		(0.015)		(0.021)		(0.015)		(0.042)
F-statistic		479.2		201.6		409.7		37.0
Second stage								
Actual retire-	-0.009	0.007	-0.007	-0.011	-0.009	0.006	-0.007	0.024
ment age (ARA)	(0.000)	(0.007)	(0.000)	(0.008)	(0.001)	(0.007)	(0.001)	(0.019)
Number of obs.	68,212	68,212	79,471	79,471	61,320	61,320	27,301	27,301

	Blue Collar workers – men		Blue Collar workers – women		White collar workers – men		White collar workers - women	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
First stage								
Entitled retire-		0.339		0.262		0.273		0.199
ment age (ERA)		(0.019)		(0.026)		(0.052)		(0.079)
F-statistic		323.1		98.0		27.0		6.34
Second stage								
Actual retire-	-0.010	0.013	-0.006	-0.007	-0.008	0.038	-0.004	-0.018
ment age (ARA)	(0.001)	(0.009)	(0.001)	(0.011)	(0.001)	(0.024)	(0.001)	(0.041)
Number of obs.	40,891	40,891	20,429	20,429	14,389	14,389	12,912	12,912

	Married		Unmarried		Married women		Unmarried men	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
First stage								
Entitled retire-		0.303		0.235		0.217		0.266
ment age (ERA)		(0.013)		(0.027)		(0.022)		(0.040)
F-statistic		500.7		77.8		93.9		45.2
Second stage								
Actual retire-	-0.007	0.001	-0.009	0.011	-0.005	-0.017	-0.013	0.011
ment age (ARA)	(0.000)	(0.006)	(0.001)	(0.016)	(0.000)	(0.011)	(0.001)	(0.026)
Number of obs.	112,997	112,997	35,040	35,040	48,424	48,424	13,128	13,128

Note: Standard errors in parentheses. The dependent variable takes the value 1 for individuals who did not survive through age 70. In addition to treatment group fixed effects and cohort dummies, all regressions are estimated with controls for gender, marital status, education, industry, previous paid sick leave, previous earnings, pension points history and public sector.

6. Conclusion

In this paper we have investigated the potential impact of early retirement on mortality in a setting based the gradual phase-in of an early retirement program in Norway. The program did not cover the entire labour force, and hence provided a control group with pension eligibility age constant at 67. Among those covered the eligibility age was reduced in a step-wise manner, and these reductions are used as an instrument for actual retirement age to eliminate biases resulting from underlying variables like health status that may influence both retirement age and mortality.

Detailed and reliable administrative register data allow observations of mortality up to age 70 for cohorts in which treatment group workers were exposed to eligibility ages ranging from 65 to 62. We have also studied mortality up to age 77 for a subset of cohorts for which the eligibility age of the treatment group varied between 65 and 64, but the sample size for this part of the analysis is considerably smaller.

The data reveal a very clear association between retirement age and mortality, up to ages 67, 70, 74 and 77. However, instrumental variable estimation shows that this link is not a causal one: precise 2SLS estimates show that retirement age in itself has no significant effect on subsequent mortality. Hence, in the absence of the early retirement program, we conclude that longevity would have been roughly the same.

Our results serve as an illustration of the importance of controlling for selection into early retirement, known in parts of the literature as the ”healthy worker effect” (see e.g. Shim et al. (2010)). The studies of mortality that have attempted to control for such selection by using information on health status have thus far ended up with varying results. This lack of consistent results in the existing literature is probably largely due to the difficulty in measuring all relevant health related factors that influence both retirement and mortality. In studies like ours, the approach is to control for selection either by comparison of groups similar except for different early retirement options or by constructing an instrument variable for acual retirement age. Coe and Lindeboom (2008) find results very similar to ours, based on a sample with a similar range of retirement ages. The negative association between health and early retirement disappears when an early retirement ”window” is used as an instrument for actual retirement.

Our results stem from a setting in which the eligibility age was reduced, first to 65 from 67, then progressively from 65 to 62. While we find no impact on subsequent mortality from this variation, it might be that other age ranges would have an impact. At even higher ages, there could be beneficial effects of early retirement if work then becomes increasingly demanding, whereas a lower age range in combination with a perceived pressure to leave employment might have the opposite effect. This could be an explanation of the results of Kuhn et al. (2010), who find higher mortality among male early pensioners. The early retirement age in their sample was quite low with an average age in the mid fifties, and the circumstances around the early retirement might indicate that many workers felt they were being pushed out of the labour market.

In principle we cannot rule out that retirement age does impact mortality, even if we find no such effect. Yet, if we are to interpret the statistically insignificant coefficients we do obtain, it seems that if retirement age does affect mortality, then early retirement is more likely to lighten mortality than the reverse, at least for men. However, we reiterate that none of these effects are statistically significant and that such an effect can hardly be of any great importance.

Overall, however, our results provide a clear message for policy. For an age range from the early 60s and upwards, arguably the most relevant age interval for policymakers, our results indicate that mortality considerations should not have a prominent place in policy considerations of the retirement age.

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Appendix

Presentation of all estimated coefficients of equations (2) and (3) using the full dataset and mortality measured at age 70. The coefficients for monthly birth-cohorts and number of months on sickness leave the previous year are presented in Figure A1 below.

Table A1: Complete set of estimated coefficients for main model

	First stage equation: Predicting actual retirement age (ARA)		Second stage equation: The impact on mortality before age 70	
	Coefficient	Standard error	Coefficient	Standard error
Entitled retirement age (ERA)	0.221	0.012	-	-
Actual retirement age (ARA) – predicted	-	-	0.009	0.008
Treatment group	-0.352	0.047	0.009	0.009
Marital status (reference: married)				
Single	-0.034	0.020	0.033	0.003
Widow/widower	0.323	0.017	0.011	0.003
Separated	0.100	0.017	0.025	0.003
Divorced	0.173	0.043	0.027	0.006
Same sex partner	-0.096	0.382	0.153	0.055
Widow/widower from same sex partner	1.030	0.960	-0.058	0.138
Industry (reference: Community, social and services)				
Primary sector	-0.209	0.052	0.000	0.008
Mining and petroleum	-0.412	0.032	0.006	0.006
Manufacturing, primary	-0.529	0.028	0.010	0.006
Manufacturing, secondary	-0.327	0.022	0.002	0.004
Construction, electricity, gas and water supply	-0.370	0.027	0.003	0.005
Wholesale, retail trade and tourism	-0.216	0.023	0.003	0.004
Transport	-0.266	0.021	0.001	0.004
Financial services and administration	-0.192	0.019	0.008	0.003
Education and health	0.005	0.018	0.004	0.003
Number of pension points (earnings history)	-0.003	0.000	-0.000	0.000
Years with pension points accrual	-0.035	0.002	0.001	0.000
Earnings in t-1	0.348	0.009	-0.008	0.003
Earnings in t-2	-0.017	0.009	0.003	0.001
Education level (reference: "nus1=3")				
No education	-0.125	0.062	0.030	0.009
Compulsory only	-0.074	0.015	0.006	0.002
Upper secondary school	0.068	0.017	-0.001	0.002
Extended vocational education	0.029	0.034	0.012	0.005
College / university, lower level	0.033	0.019	-0.003	0.003
College / university, higher level	0.454	0.027	-0.013	0.005
Ph.D	1.934	0.062	-0.025	0.018
Education subject (reference: general/unspecified)				
Languages and arts	0.003	0.030	-0.004	0.004
Teaching	-0.479	0.027	-0.007	0.005
Social science and law	0.147	0.052	-0.003	0.008
Economics and administration	0.009	0.019	-0.000	0.003
Natural science	-0.120	0.019	-0.002	0.003
Health and medicine	-0.072	0.021	-0.005	0.003
Primary (farming, fisheries, forestry)	-0.052	0.034	-0.017	0.005
Transport and services	-0.121	0.028	0.008	0.004
Unknown	0.153	0.109	-0.011	0.016
Public sector employee	0.040	0.015	-0.000	0.002
Female	0.079	0.015	-0.050	0.002
Constant	50.27	0.804	-0.467	0.510
Number of observations	134,846		134,846	

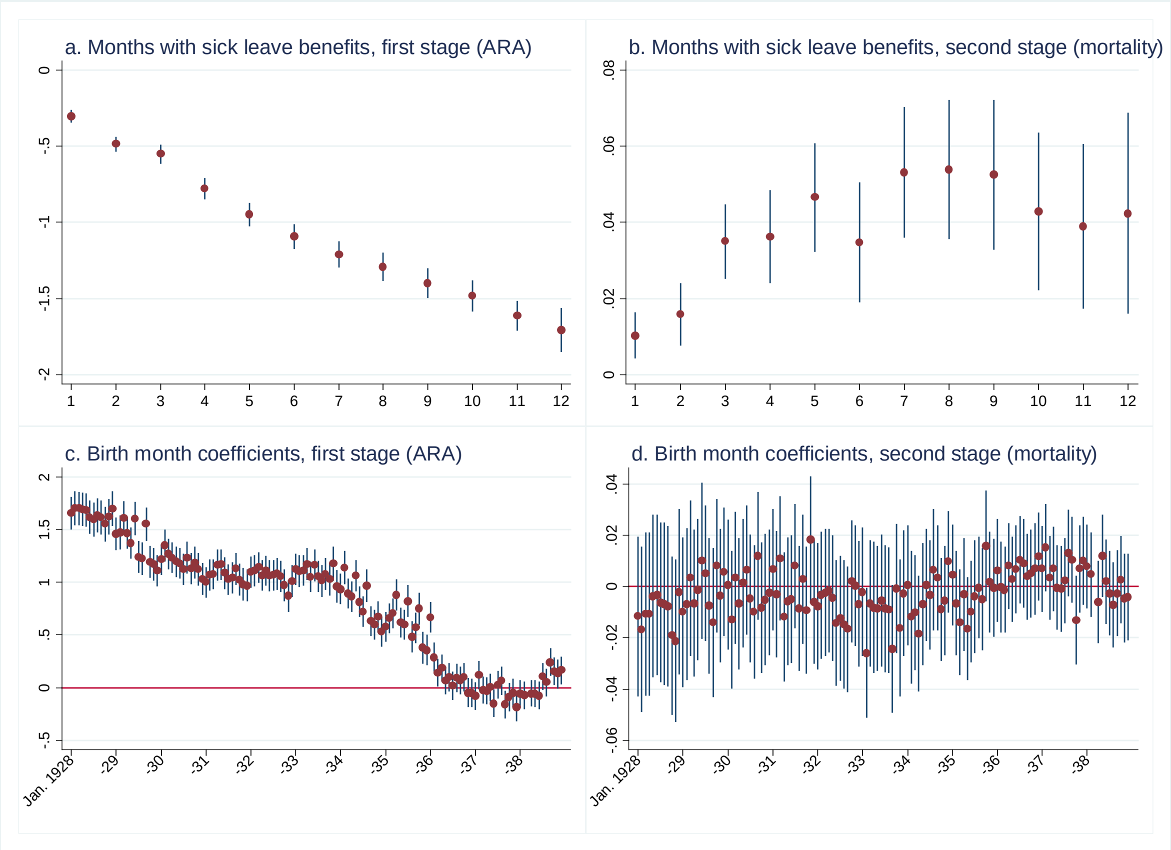

Figure A1: Estimated coefficients for dummies correspond to the number of months with receipt of sickness leave benefits over the last year prior to the early retirement age for each cohort (upper panels) and for birth month dummies (lower panels). The vertical spikes correspond to 95% confidence intervals, and the reference categories are 0 and March 1938 for months with sickness leave benefits and birth month, respectively. Effects on ARA are given in the left-hand panels (the first stage regression) and effects on mortality before age 70 in the right-hand panels (the second stage regression).