Does Retirement Age Impact Mortality?

By

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1

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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. Difference-in-differences estimation based on monthly birth cohorts and treatment group status show that the early retirement programme significantly reduced the retirement age; this holds true also when we account for programme substitution, for example into the disability pension. Instrumental variables estimation results show no effect on mortality of retirement age; neither do estimation results from a hazard rate model.

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. Moreover, 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; 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 programmes, it is important to separate the potential effect of an early retirement programme 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 programme*, 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 2010. 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 programme substitution, since early retirement may serve as a substitute for disability pension and other social insurance programmes. 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. As a final robustness check we estimate a triple difference estimator of the effect of retirement eligibility on mortality in a hazard rate model framework with flexible monthly mortality risk. The results of this exercise are well in line with our main results.

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

4

and the data. 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. Results from the hazard rate model are presented in Section 6. Section 7 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, identify 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 do not allow firm conclusions to be drawn regarding the link between specific categories of retirement and mortality, although they find that "all-type" retirement, which includes health induced retirement, is a risk factor for mortality. They conclude that there is a "critical" need for further research.[1]

Several studies have also recognised 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 suggests 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 are 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 health status. Among women without reduced earnings capacity, earlier retirement reduces mortality. On the other hand, Quaade et al. (2002) use a similar approach and find mortality among early retirees to be "normal" initially but subsequently increasing. 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.

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[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 age 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 approach eliminates biases due to selection into retirement of persons in such bad health that they die before age 70, at the same time as it fails to capture 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[2], 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 that increased 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 is 13 percent lower among men who retired early, but there is 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.

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[2] The most recent study of which we are aware is a preliminary paper by Bingley and Pedersen (2011). In an instrumental variable approach, they exploit the introduction of an early retirement programme in Denmark. Using population based administrative data on blue collar workers they find that those induced to retire early by the programme have subsequently better health and reduced mortality, both by age 70 and 80.

3. Institutional setting and data

The Norwegian old age pension system

During our observation period and before a major pension reform came into effect in 2011, the statutory pension age in Norway was 67. In common with other advanced economies, Norway shows a steady upward trend in life expectancy at retirement. 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).

For most retirees, the National Insurance System (NIS), a mandatory, public pay-as-you-go defined benefit plan, is the main provider of pension income.[3] This pension consisted of a basic pension corresponding to somewhat less than 20% of full time average earnings and supplementary earnings based pension. For those who did not qualify for the earnings based pension, there was an additional minimum pension almost equal to the basic pension. For a hypothetical worker with constant real earnings just below the earnings based pension accrual level, the replacement rate would be approximately 60%, compared to a replacement rate of 33% for a worker with constant earnings at the maximum pension accrual level (approximately twice the average of full-time earnings). The NIS was therefore very progressive and redistributive. An earnings test for persons aged 67-69 was abolished for one cohort each year between 1 January 2008 and 1 January 2010.

In addition to the NIS, there are occupational pensions in both the public and the private sector, for which the most common eligibility age is 67. The public sector pension is fully integrated with the NIS so that the two combined give a pension corresponding to 66% of the final salary at full accrual, which is 30 years. In the private sector, occupational pensions have been mandatory only since 2006. They are firm based and can be of the defined benefit or defined contribution type, with considerable variation in benefit levels: the minimum level is equivalent to a 2% defined contribution pension, while most defined benefit plans are targeted at a replacement rate of 60 – 65 % when the NIS pension is added in. Private sector firms are free to choose whether to provide disability and survivor coverage as part of the pension.

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[3] An overview of the Norwegian National Insurance System and the proposed pension reform can be found in The Ministry of Labour and Social Inclusion (2006).

Disability pensions

The disability pension system constitutes the most important early exit route from the labour market, as about 40% of all 66 year-olds are receiving disability pensions. Eligibility for disability pension benefits is contingent on certification by a physician, and benefits can be granted by the Social security administration after one year of sick leave and subsequent participation in rehabilitation programmes. The benefits are based on projections of future earnings up to age 67 and otherwise calculated the same way as old age pension benefits. Some workers are also eligible for additional benefits from occupational pensions with disability insurance.

Early retirement

From 1 January 1989 the early retirement programme AFP came into operation as a result of the central tariff negotiations between the employers' and employees' confederations and the government. The programme 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. In order to be eligible, an individual must have her main occupation in a participating firm up until the time of retirement, have reached the eligibility age, and meet certain individual labour market history requirements.[4]

From the general retirement age at 67, the programme lowered the eligibility age for those covered to 66 with effect from 1 January 1989[5], 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.[6] 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 benefits are broadly similar to disability benefits, in that they are calculated the same way as old age pensions and based on earnings projected up to age 67.[7] In addition comes a subsidy of about 1000 NOK/month (net of taxes) during the early retirement years. The

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[4] The individual requirements include employment in the firm for the last three years or in AFP affiliated firms the last five years; not to be receiving any pension from the firm; current (annualized) earnings and last year's earnings at least on a level corresponding to 1 Basic Amount (BA); at least 10 years with earnings exceeding 1 BA after age 50; and the average of the 10 highest yearly incomes since 1967 exceeding 2 BA. 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 6 % over our observation period. The average Basic Amount for 2010 was 74,721 NOK, which corresponds to 17% of average full time wages and to about 12,500 (9,800) USD (EUR).

[5] Limited to those who turned 66 after 31 March 1988.

[6] See NOU 1998:19, page 22.

[7] Public sector employees aged 65-66 also receive their public sector occupational pensions, similarly based on earnings projected to age 67.

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[8]. Programme costs are shared between the government and the participating employers (by means of funds financed by fees per employee varying according to hours worked (three categories)), and regulations ensure that the programme has the same structure in all participating firms and that all workers in participating firms are covered.

Recipients of early retirement, unemployment and disability insurance benefits suffer immediate income losses of 30-40% on average, net of taxes. For recipients of early retirement and disability insurance benefits, the public pension benefit level from age 67 and onwards is the same as it would be had they continued working until age 67 in the job they had just prior to retirement or disability, based on projected earnings. Recipients of unemployment benefits, however, will receive a lower level of public pension benefits than they would have had they not lost their job, as pension accruals for the unemployed are based on unemployment benefits, as opposed to projected earnings.

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[9], 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 various sources, including wages, pensions, sick-leave, disability, and unemployment benefits. The administrative registers cover the years 1992-2010. In addition, we have access to earnings histories, in terms of annual pension entitlement accruals in the NIS, dating back to 1967.

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[8] 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.

[9] 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.

The *actual retirement age* (ARA) can be defined either in terms of take up of pensions or other benefits or as sharply reduced earnings (indicating substantial labour force withdrawal), or a combination of the two. We have data for receipt of various benefits on a monthly basis, and earnings on an annual basis with dates for start and stop of each employment spell. Using a combination of these different data sources we define ARA as the last month of regular work[10] without receipt of any pensions or benefits; early retirement pensions, ordinary old age pensions, unemployment or disability insurance benefits.[11]

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 alive, resident, and employed at age 62. There has been a substantial decrease in mortality up to this age over our sample period, and the fraction still alive and resident at age 62 has increased from 80.9 percent to 85.9 percent. The employment rate at age 62, however, has been fairly constant at about 42 percent.

Since our estimation dataset only includes those employed at their monthly birth cohort's 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.

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[10] Monthly working status is defined as having earnings corresponding to an annual level of at least 1 Basic Amount (see footnote 3).

[11] Within year start and stop dates of employment spells are considered somewhat less reliable than the other data. Therefore, there may be some measurement error of month of retirement within years for a very limited number of workers who leave employment without any pensions or benefits.

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

Birth year	Percentage alive, resident at age 62	Percentage of residents employed at age 62	Mean early retirement age (ERA)	Sample: Employed at early retirement age (ERA)	Percentage of sample with an employer that offers early retirement
(1)	(2)	(3)	(4)	(5)	(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 the early reti

rement 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. The percentage resident at age 62 is primarily driven by mortality, but also to some extent by migration.

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 programme: treatment group workers are those in participating public and private sector firms while control group workers are those in non-participating private sector firms.[12] Although individual eligibility for early retirement is determined both by firm affiliation and 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. About 95 percent of the workers in our sample do meet the individual criteria, however, and conditioning on these does not alter our results.

We have also checked for strategic job moves into AFP employment just prior to early retirement eligibility, without finding any signs of this. The total job moves frequency among those employed in two consecutive years falls steadily with age (measured at ages 50, 55, 60 and 65), in line with other results on job moves, see e.g. Hernaes et al. (2011). Furthermore, the moves into and out of early retirement coverage are of similar magnitude and constitute only a small fraction of the total moves. In our data, the fraction of workers moving into (out

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[12] 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 firm's unique organizational number.

of) ER coverage are 1 (0.7), 0.9 (0.8) and 0.8 (0.8) percent when measured at age 60, 62 and 64, respectively.

Table 2 presents some descriptive statistics for the treatment and control group workers. Treatment group workers are more educated and more often female. They are also more often working in the services sector, and more likely to be defined as white-collar workers[13]. Not surprisingly, mortality is also slightly higher in the control group than in the treatment group.

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 is observable only for cohorts born before 1935 and 1931.

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 is the same between the treatment and control groups among those born in 1928, leaving with a disability pension is much more common in the control group for those born in 1938. Hence, the increased take-up of early retirement pensions is partly mirrored in

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[13] 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.

the control group, where the fraction leaving with disability pensions increases substantially from the 1928 to the 1938 cohort. In terms of generosity, early retirement pensions, disability pensions and unemployment benefits do not vary much. For most workers, all these programmes replace approximately 2/3 of previous earnings.

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

Turning to the relationship between entitled retirement age (ERA), actual retirement age (ARA) and mortality, the upper panel of Figure 1 depicts the ERA for each of the 132 monthly birth cohorts included, divided into treatment (red, solid line) and control groups (blue, dashed line). 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; as the gap in ERA increases from two to five years, the corresponding gap in ARA increased from less than one year to almost two years. The relationship between ERA and ARA will form the first stage in an instrumental variables framework 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 treatment group left through early retirement.

Finally, panel (c) 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.
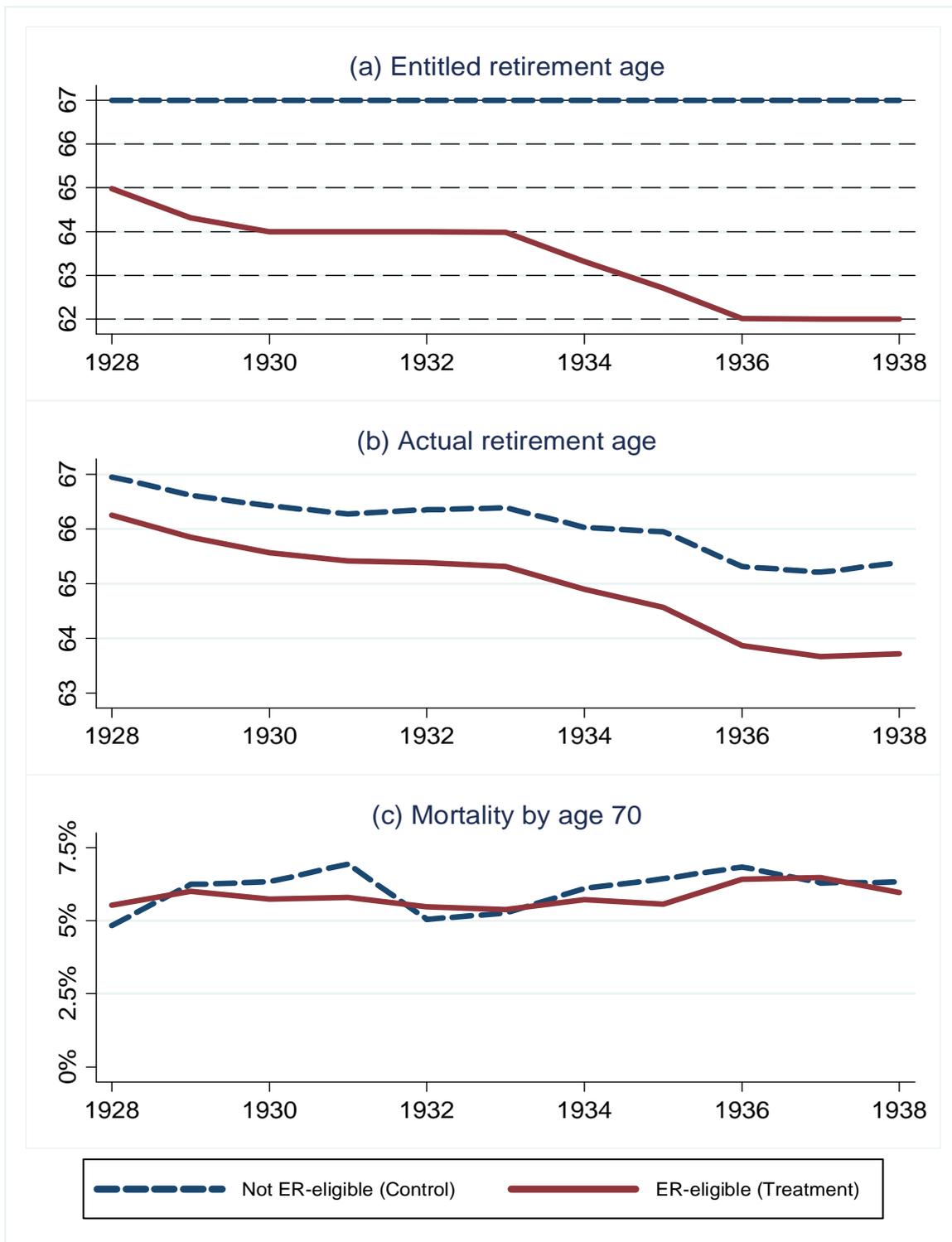

(a) Entitled retirement age

(b) Actual retirement age

(c) Mortality by age 70

Not ER-eligible (Control)   ER-eligible (Treatment)

Figure 1: Retirement age and mortality in the sample by cohort, for treatment and control group workers.

4. Econometric model and identification

There are good reasons to suspect that results from a regression of mortality on retirement age (ARA) would be biased. First, since survival is a prerequisite for employment, there might be biases due to reverse causality. Second, there may be unobserved factors such as health status influencing both retirement age and mortality, thereby creating an omitted variables bias. Adequately accounting for all such factors is difficult, if not impossible, even with access to self-reported or other health information. We therefore make use of exogenous variation in retirement opportunities, provided by the stepwise reductions in the ERA of treatment group workers, in an instrumental variable framework. Since the formal retirement age for control group workers remained constant at age 67 throughout the period, we build our identification on a *difference-in-differences* strategy.

The first stage in our instrumental variables model is given by equation (1), 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 entitled retirement age which is 67 for the control group and between 65 and 62, depending on the birth cohort, for the treatment group:

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

The second stage equation is given by equation (2), where y^a is an indicator for whether or not the person lived through age a, and the observed ARA is replaced by predicted values from (1):

$$(2) \quad y^a = x\beta^2 + C^2 + \lambda^2 T + \theta^{IV} A\hat{R}A + e$$

Technically, identification is obtained if $\gamma \neq 0$ and if ERA affects mortality only through ARA (the exclusion restriction). Substantial labour supply effects of the early retirement programme are well documented in the existing literature[14] and are readily confirmed by the

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[14] See e.g. Hernæs, Sollie and Strøm (2000), Røed and Haugen (2003), Bratberg, Holmås and Thøgersen (2004) and Vestad (2012).

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 2 we present 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 treatment group workers. The left-hand column of Figure 2 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.

Comparing treatment and control groups

Group means by cohort - Difference in means relative to first cohort

Fraction females

Years of schooling
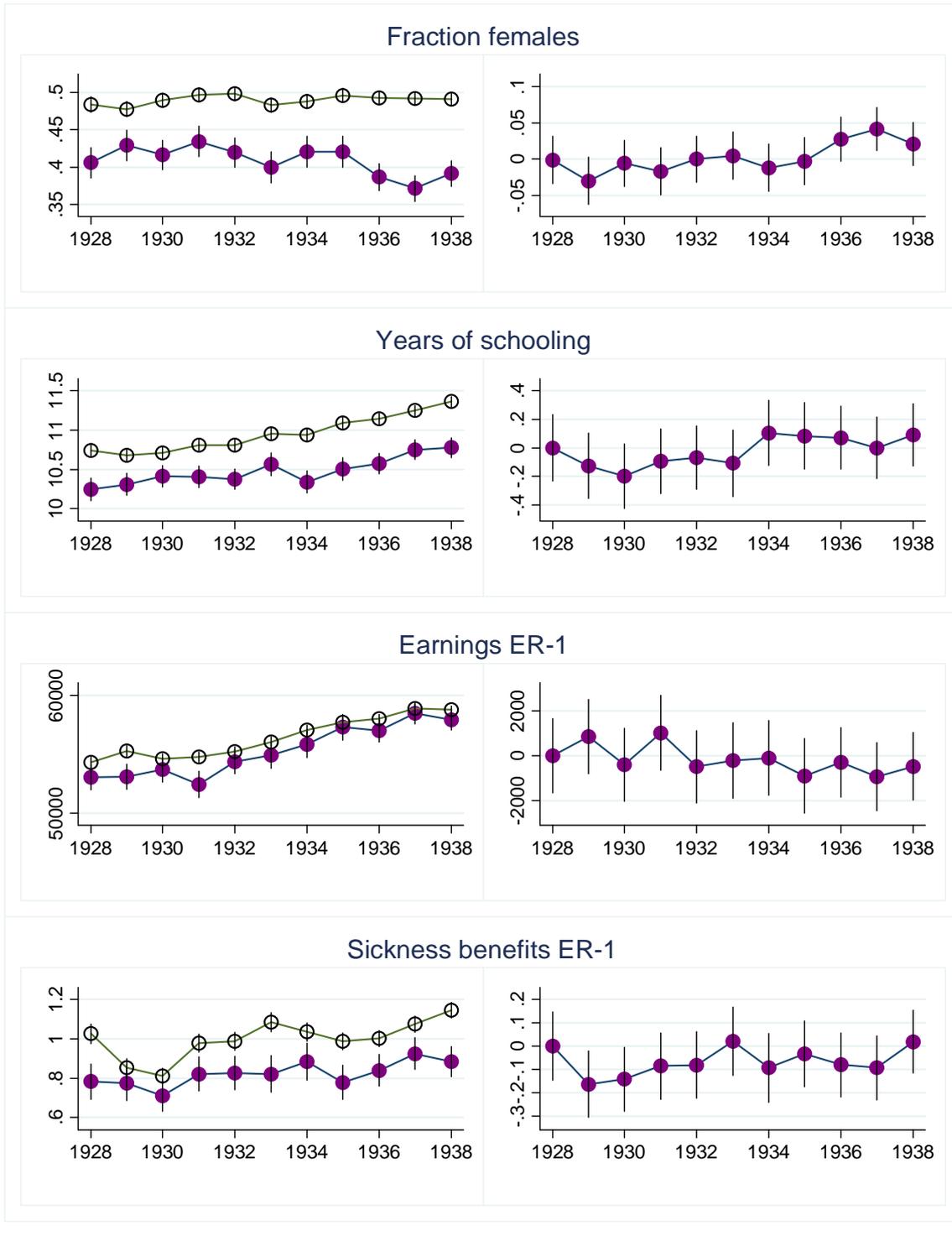

Earnings ER-1

Sickness benefits ER-1

Figure 2: Comparing the treatment and control groups across cohorts. In the leftmost panels, the treatment group is represented by hollow circles and the control group by purple circles.

17

5. Results

Estimation results for the effects of entitled retirement age (ERA) on actual retirement age (ARA), i.e. equation (1), 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.[15] 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 based on 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 (equation 2): 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 variables bias. A complete presentation of all coefficients of the first and second stage is provided in the Appendix.

One might want to evaluate the magnitude of the 2SLS point estimates, their statistical insignificance notwithstanding. Acknowledging the fact that the point estimates should be interpreted as local average treatment effects, we calculate that the effect of a 0.288 years reduction in the ARA (which is the estimated average treatment effect on ARA of a one year

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[15] 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.

increase in the ERA) results in a 2.1% increase in mortality by age 67[16], while mortality by age 70 is increased by 1%. By the same logic we calculate that a 0.238 years increase in ARA results in a 5% increase in mortality by age 74, and finally that a 0.151 years increase in mortality results in a 4.9% increase in mortality by age 77.

Table 4: Main estimation results

	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 retirement age	-0.006*	0.002	-0.008*	0.002	-0.011*	0.025	-0.013*	0.066
(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, * denotes coefficients significant at the 1% level. 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. All regressions are estimated with controls for gender, marital status, education, industry, previous paid sick leave, previous earnings, pension points histories and a dummy for public sector workers. The 2SLS regressions also include treatment group fixed effects and cohort dummies.

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). A common way of justifying the common trends assumption is to compare trends for the treatment and comparison groups prior to treatment. Unfortunately, such an exercise is not feasible in our setting, as the employment records are only available from 1992, three years after the ER programme was introduced.[17] Another test is to explicitly include a linear time trend for the treatment (or control) group. This will clearly result in a weaker first stage as we solely make use of the deviations from a linear trend in the reductions in ERA (see Figure 1). The results in the leftmost column of Table 5 confirm this intuition. The first stage point estimate is reduced by more than 50 per cent, the F-value is barely above 10, and the standard error is considerably larger. Nevertheless, the 2SLS point estimate is still positive, and still not statistically different from zero. Hence, we do not find any indications of

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[16] I.e. (0.2 * 0.288)/2.8 = 2.1%, where 0.2 is the 2SLS point estimate in percentage points, 0.288 is the estimated first stage effect of ERA on ARA, and 2.8 is the average mortality rate by age 67 (in per cent) for treatment group workers (cf. Table 2).

[17] As an indirect test, we have made use of age standardized mortality trends over 10-year periods from Strand et al. (2010), stratified on gender and educational groups, to calculate the mortality changes that would follow given the composition of the treatment and control groups. The resulting crude estimates indicate that mortality trends should be expected to be very similar for the treatment and control groups in our time window.

different time-trends masking a causal relationship between retirement age and mortality. The common trends assumption might also be violated if the composition of the control and treatment groups changes over time, in a way that alters the relative differences in mortality, and if this compositional change is not well captured by the observable characteristics included in the model. 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 are affecting our estimates, we estimate the model using firm fixed effects. We also estimate the model on a subset of the sample consisting only of workers in firms that are present in all years. The results from these exercises are displayed in columns two and three of Table 5; we note that the results are unaltered in both specifications.

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 65-64 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.

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 well-established literature describing mortality and health effects resulting from (involuntary) job-loss[18]. 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 the treated. Involuntary job-losses could be disguised as (voluntary) early retirement if

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[18] See *inter alia* Gallo et al. (2004), Eliason and Storrie (2009), Rege et al. (2009), Salm (2009), Sullivan and von Wachter (2009), Browning and Heinesen (2012) and Black et al. (2012).

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.

Our data does 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 into 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. If anything, the estimates indicate that as focus is shifted towards workers more likely to have experienced involuntary job-loss, the point estimate is smaller, while as we move towards workers more prone to voluntary labour market exits the point estimate is larger.

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

	Separate time-trend for treatment group	Composition of firms		Separate reductions in ERA		Employers sending workers into unemployment at the year of labour market exit		
		Firm F.E.	Firms present all years	65 to 64 Born 1928-31	64 to 62 Born 1932-38	None	> 0	> 25%
First stage								
Entitled retirement age (ERA)	0.137*	0.237*	0.207*	0.121*	0.190*	0.235*	0.219*	0.255*
	(0.039)	(0.020)	(0.078)	(0.042)	(0.019)	(0.000)	(0.020)	(0.051)
F-statistic	12.46		7.04	8.2	104.04	215.0	115.4	24.7
Second stage								
Actual retirement age (ARA)	0.024	0.004	0.003	0.113	0.014	0.011	0.007	0.003
	(0.038)	(0.012)	(0.053)	(0.072)	(0.013)	(0.009)	(0.014)	(0.031)
Number of obs.	148,037	148,021	52,589	44,708	90,138	62,222	76,624	3,534

Note: Standard errors in parentheses, * denotes coefficients significant at the 1% level. 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 histories and a dummy for public sector workers.

Previous studies have found different effects for different types of 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 of subsamples and estimate the model separately for each of them. The results are reported in Table 6.[19]

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 take-up of disability pensions. The second stage estimates for men and for women are both

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[19] In addition to the results shown we have also estimated the model for private sector workers only. The results mirror the others and provide a non-significant but positive coefficient for retirement age on mortality using 2SLS. Since all public sector employees are included in the treatment group, separate estimation for employees in the public sector is not feasible.

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 retirement		0.340*		0.219*		0.275*		0.325*
age (ERA)		(0.016)		(0.019)		(0.013)		(0.032)
F-statistic		466.1		131.8		453.9		99.9
Second stage								
Actual retirement	-0.010*	0.007	-0.006*	-0.011	-0.008*	-0.001	-0.007*	0.014
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 retirement		0.322*		0.303*		0.309*		0.255*
age (ERA)		(0.015)		(0.021)		(0.015)		(0.042)
F-statistic		479.2		201.6		409.7		37.0
Second stage								
Actual retirement	-0.009*	0.007	-0.007*	-0.011	-0.009*	0.006	-0.007*	0.024
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 retirement		0.339*		0.262*		0.273*		0.199*
age (ERA)		(0.019)		(0.026)		(0.052)		(0.079)
F-statistic		323.1		98.0		27.0		6.34
Second stage								
Actual retirement	-0.010*	0.013	-0.006*	-0.007	-0.008*	0.038	-0.004*	-0.018
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 retirement		0.303*		0.235*		0.217*		0.266*
age (ERA)		(0.013)		(0.027)		(0.022)		(0.040)
F-statistic		500.7		77.8		93.9		45.2
Second stage								
Actual retirement	-0.007*	0.001	-0.009*	0.011	-0.005*	-0.017	-0.013*	0.011
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, * denotes coefficients significant at the 1% level. The dependent variable takes the value 1 for individuals who did not survive through age 70. All regressions are estimated with controls for gender, marital status, education, industry, previous paid sick leave, previous earnings, pension points histories and a dummy for public sector workers. The 2SLS regressions also include treatment group fixed effects and cohort dummies.

6. The effects of retirement eligibility on mortality in a hazard rate model

In this section we set up and estimate a hazard rate model of the relationship between early retirement eligibility and mortality. The main reason for doing so is to make sure that our findings do not merely result from the linear probability model framework not making use of all available information about age patterns of mortality. For the hazard rate model to be well specified, we base the analysis in the current section on a modified version of the main sample, namely one in which all workers enter the sample at the age of 61. While in the main sample we include only individuals registered with an active employment record the month before they reach the ERA of their monthly birth cohort, we now require employment in the month that they reach the age of 61. We thereby sample all workers born between January 1932 and January 1939. For treatment group workers in this modified sample, the ERA varies between 62 and 64, according to their monthly birth cohort. Some descriptive statistics for the modified sample are provided in Table 7.

Table 7: Descriptive statistics for the hazard rate model sample

	All	**Treatment**	**Control**
# of persons/spells	117 628	94 563	23 065
Mean duration until death or censoring (months)	160.6	160.6	160.8
Fraction of spells ending with death	0.1597	0.1597	0.1599
Fraction female	0.45	0.47	0.39

Figure 3 shows the actual retirement age and mortality by age 70 for the treatment and control groups. As can be seen from the figure, workers in both groups tend to retire earlier as the ERA decreases. The gap in retirement age between the groups increases significantly as the ERA is reduced for the treatment group but not for the control group. Mortality, however, seems to be fairly similar in the treatment and control groups, both for earlier and later cohorts.
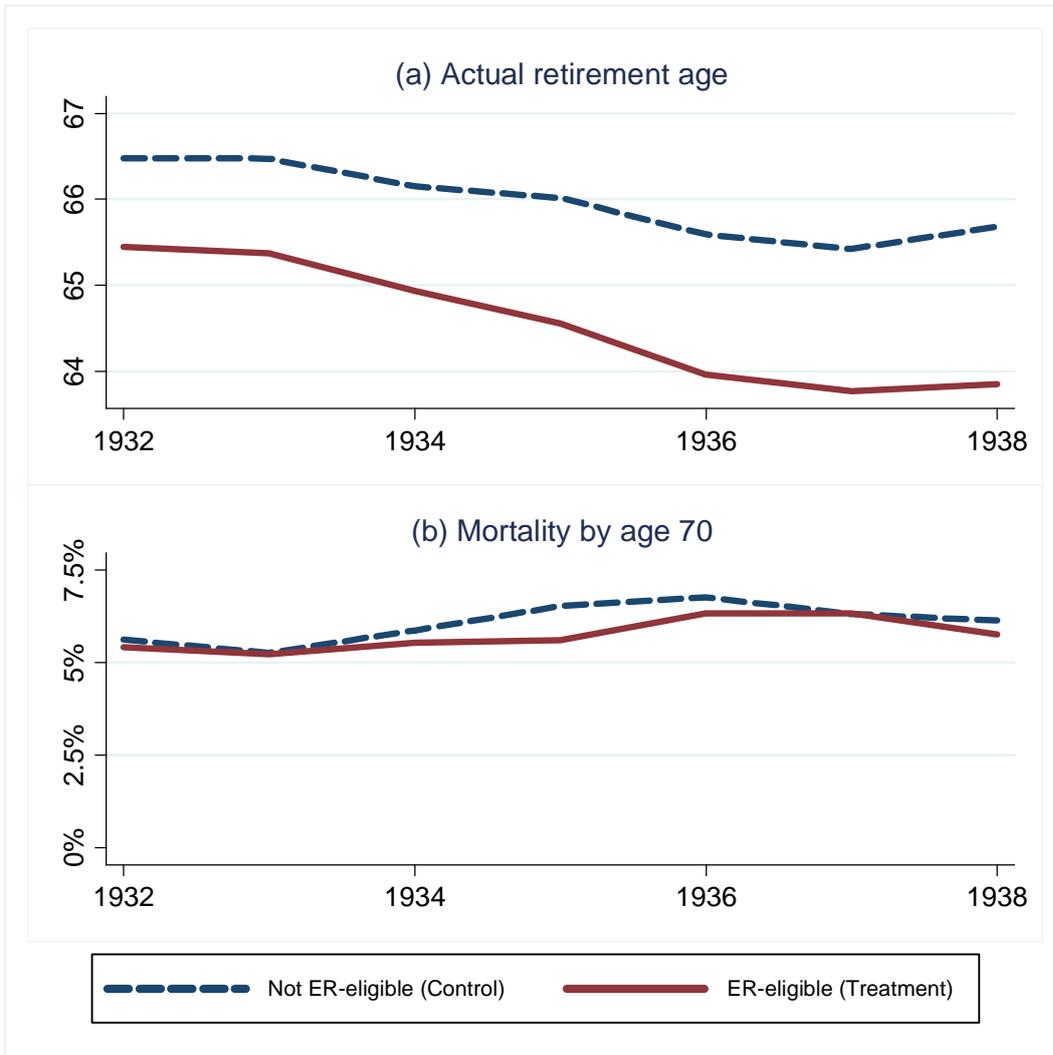
Figure 3: Actual retirement age and mortality in the hazard rate sample by cohort, for treatment and control group workers.

In much the same way as for the main specification, identification of causal effects in the hazard rate model is based upon exogenous variation in retirement opportunities provided by the stepwise reductions in the ERA of treatment group workers. Since the hazard rate model makes use of repeated observations of the same individuals we are now able to identify a triple differences estimator, defined as the difference between the difference-in-differences estimators for treatment and comparison group workers.

We write the hazard rate of death as

$$(3) \quad \theta_{ict} = \exp(x_i\beta_1 + \upsilon_i + \beta_2\tau_{it} + \beta_3 c_i + \beta_4 T_i + \beta_5 T_i \times \tau_{it} + \beta_6 T_i \times c_i + \beta_7 P_{ict} + \beta_8 Eligible_{ict}),$$

where i indexes individuals, c indexes monthly birth cohorts, and t indexes age. X is a vector of observable characteristics[20] and ν captures unobserved characteristics, the τ_{it}'s are age fixed effects, the c_i's are cohort fixed effects, and T_i takes the value 1 if individual i belongs to the treatment group (i.e. works in an early retirement affiliated firm), and zero otherwise. P_{ict} is an indicator variable that takes the value 1 when an individual reaches her cohort's ERA and is zero otherwise, and is thereby an interaction of age and monthly birth cohort. $Eligible_{ict}$ is an interaction of age, cohort and treatment status; it takes the value 1 when an individual becomes eligible for (early) retirement.

Equation (3) is estimated on a monthly basis, using an approach similar to the one described in detail by Fevang et al. (2013). Unobserved heterogeneity is modeled non-parametrically by allowing for a finite number of mass points (Lindsay, 1983, Theorem 3.1 and Heckman and Singer, 1984, Theorem 3.5) and the preferred model is selected on the basis of the Akaike Information Criterion (AIC). Gaure, Røed and Zhang (2007) present Monte Carlo evidence indicating that parameter estimates obtained this way are consistent and approximately normally distributed. Their results also indicate that the standard errors conditional on the optimal number of support points are valid for the unconditional model as well, and hence can be used for standard inference purposes.

Table 8 gives the main estimation results for equation (3): the difference-in-differences and triple differences estimates of changes in the ERA on mortality. The difference-in-differences estimator for treatment group workers is $\beta_7 + \beta_8$, and the corresponding point estimate is -0.057. That is, eligibility for early retirement reduces the monthly hazard by (a non-significant) 5.7 percent. The possibility of biases due to changes in age specific mortality for different cohorts is addressed by the difference-in-differences estimator for comparison group workers (β_7), which captures the effect of reaching the monthly birth cohort's ERA, without accounting for actual eligibility for early retirement. The non-significant point estimate of -0.086 suggests that such biases are of no concern for our analysis. Finally, the triple difference estimate is also far from being significant, and indicates a 2.9 percent increase in

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[20] The vector of observable characteristics is the same as in Section 5; it includes controls for previous earnings and pension point histories, and dummies for gender, marital status, education, industry, previous paid sick leave, and public sector workers.

the monthly hazard for treatment group workers who are eligible for early retirement. The results from this section therefore reinforce our main conclusion: there is no *causal* effect of retirement age on subsequent mortality.

Table 8: Estimation results for equation (3); per cent change in hazard rate

Pension benefits eligibility (β_8)	0.029
	(0.137)
$P_{ict}(\beta_7)$	-0.086
	(0.122)
Number of mass points in heterogeneity distribution	2

Note: Standard errors in parentheses. P_{ict} takes the value 1 when an individual reaches her cohort's ERA and is zero otherwise. Additional controls are age, cohort and treatment group fixed effects; treatment group specific age and cohort fixed effects; controls for previous earnings and pension points histories; and dummies for gender, marital status, education, industry, previous paid sick leave, and public sector workers.

7. Conclusion

In this paper we have investigated the potential impact of early retirement on mortality in a setting based on the gradual phase-in of an early retirement programme in Norway. The programme does not cover the entire labour force, and thereby provides a control group with pension eligibility age constant at 67. Among those covered the eligibility age was reduced in a step-wise manner; 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. Estimates from a hazard rate model provide additional support for this result.

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 that are similar except for different early retirement options or by constructing an instrumental variable for actual 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 from 67 to 65, then progressively from 65 to 62. While we find no impact on subsequent mortality from this variation, it might be that other retirement eligibility age ranges would have an impact. At even higher ages, there could be beneficial effects of 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 is quite low (in the mid-fifties), and the circumstances around 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.

Our results are derived using data from a relatively generous welfare state, offering disability insurance (DI) to workers in bad health. We cannot rule out that the results would have been different in the absence of such an insurance system. We do believe, however, that *if* the presence of a DI system does affect our results, it so does by attenuating a reduction in mortality from early retirement.

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 discussions regarding retirement age.

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Appendix

We present all estimated coefficients of equations (1) and (2) 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

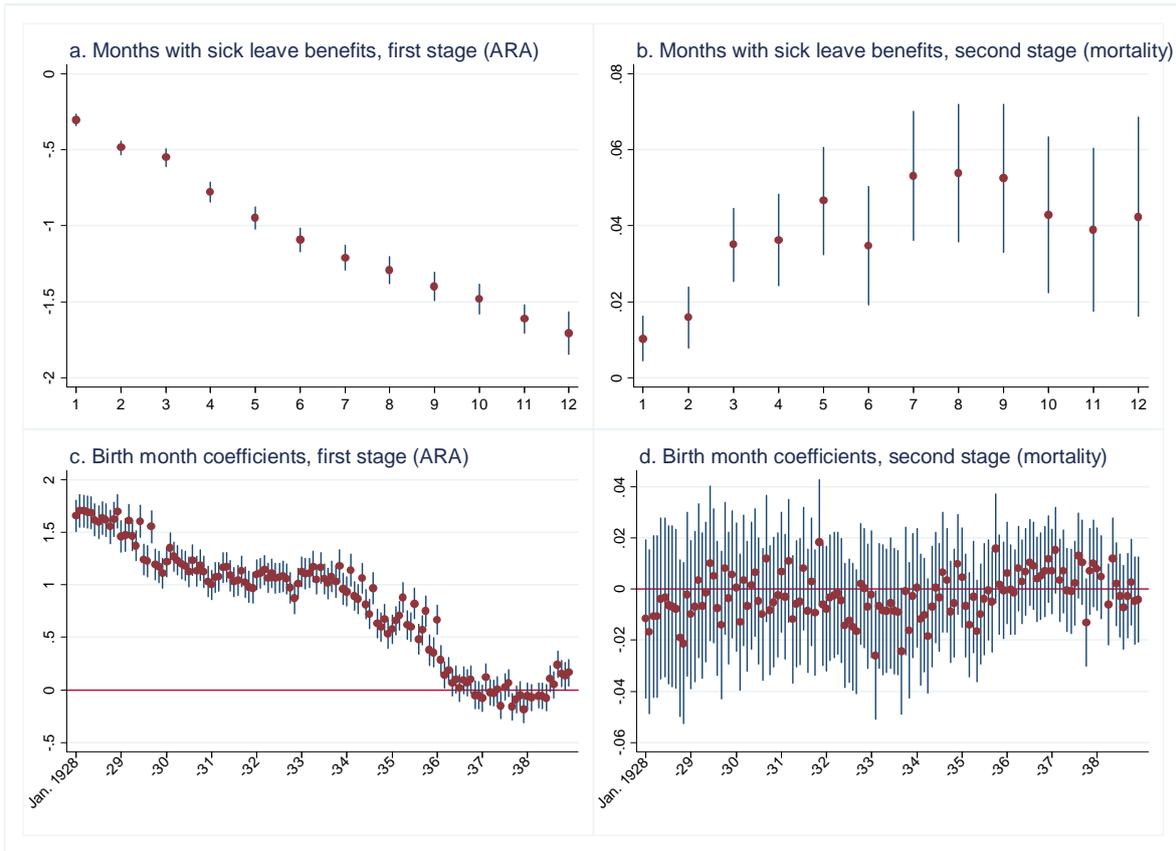|	First stage equation: Predicting actual retirement age (ARA)		Second stage equation: The impact on mortality before age 70	
	Coefficient	Standard error	Coefficient	Standard error
Entitled retirement age (ERA)	0.221	0.012	-	-
Actual retirement age (ARA) – predicted	-	-	0.009	0.008
Treatment group	-0.352	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 year t-1	0.348	0.009	-0.008	0.003
Earnings in year t-2	-0.017	0.009	0.003	0.001
Education level (reference: upper secondary 1 year)				
No education	-0.125	0.062	0.030	0.009
Compulsory only	-0.074	0.015	0.006	0.002
Upper secondary school, 3-4 years	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).