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Prolongation of working life and its effect on mortality and health in older adults: Propensity score matching

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ABSTRACT

Many countries are raising the age of pension eligibility because of increases in life expectancy. Given the social gradient in life expectancy and health, it is important to understand the potential late-life health effects of prolonging working life and whether any effects differ by socioeconomic position. We examined the effect of prolonging working life beyond age 65 on mortality and a series of indicators of late-life physical health (the ability to climb stairs without difficulty, self-rated health, ADL limitations, and musculoskeletal pain) in a representative sample of the Swedish population. In addition to average effects, we also examined heterogeneous effects, for instance by occupational social class. To do this, we use propensity score matching, a method suitable for addressing causality in observational data. The data came from two linked Swedish longitudinal surveys based on nationally representative samples with repeated follow-ups; The Swedish Level of Living Survey and the Swedish Panel Study of Living conditions of the Oldest Old, and from national income and mortality registries. The analytical sample for the mortality outcome included 1852 people, and for late-life physical health outcomes 1461 people. We found no significant average treatment effect on the treated (ATT) of working to age 66 or above on the outcomes, measured an average of 12 years after retirement: mortality (ATT -0.039), the ability to climb stairs (ATT -0.023), self-rated health (ATT -0.009), ADL limitations (ATT -0.023), or musculoskeletal pain (ATT -0.009) in late life. Analyses of whether the results varied by occupational social class or the propensity to prolong working life were inconclusive but suggest a positive effect of prolonging working life on health outcomes. Accordingly, more detailed knowledge about the precise mechanisms underlying these results are needed. In conclusion, working to age 66 or above did not have effect on mortality or late-life physical health.

1. Background

The ongoing increase of life expectancy in Western societies puts a strain on social and welfare systems (Stattin, 2005) and has led many countries, including Sweden, to develop policies to encourage people to delay retirement and extend their working life. However, the policy discussion about delaying retirement seems to overlook potential inequalities in life expectancy between different socioeconomic groups. For example, despite acknowledging differences in working conditions, health, and life expectancy by socioeconomic position, a recent Swedish governmental investigation of pension reform treated the general increase in life expectancy as a fundamental rationale for raising the official retirement age for everyone (Government Commission for Longer Working Life and Retirement Age, 2013).

Given the social gradient in health, it is of importance to understand whether and how different socioeconomic groups are affected by raising the official retirement age. Retirement is one of the most important life-course transitions, as it is accompanied by changes in income, lifestyle, social network, and time availability (Kim and Moen, 2002). Studies on retirement and subsequent health face important methodological limitations due to endogeneity, that is, potential reverse causality and confounding in the association between retirement and health. In this study, we examined whether working longer at the end of the occupational career affects health outcomes in late life. More specifically, we examined the effects of working beyond age 65 on physical health and mortality and whether prolonging working life could have heterogeneous effects by socioeconomic position or the propensity to prolong working life. To do this, we used propensity score matching (PSM), a quasi-experimental method suitable for addressing causality in observational data.

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1.1. Literature review

Studies on the effect of the retirement transition on health have had inconsistent findings; results depend on the outcome considered and the specific study design. A systematic review of 22 longitudinal studies found strong evidence that retirement had beneficial effects on mental health, but results regarding perceived general health and physical health varied (van der Heide et al., 2013). Studies on retirement and late-life health face a substantial challenge because of the endogenous relationship between retirement and health: people in poor health tend to retire earlier than others. When studying the effect of retirement on health outcomes, researchers must therefore take into account the possibility that adverse health conditions may influence retirement decisions (Jokela et al., 2010; Schirle, 2010) while also taking into account other factors that affect the timing of retirement. Previous studies have shown that these factors include working conditions (Kjellberg et al., 2016), financial security (Madero-Cabib and Kaeser, 2016), socioeconomic position (Blekesaune and Solem, 2005), spousal retirement, and family circumstances (Henkens and van Solinge, 2002; Svensson et al., 2015). In addition to this endogeneity problem, studies on retirement are subject to variations in sample selection, the ways retirement age is identified, and the health outcomes considered.

The *psychosocial-environmental*, *psychosocial-materialist*, and *cultural-institutional* hypotheses provide three theoretical lenses through which the relationship between retirement and health has been interpreted. According to the *psychosocial-environmental* hypothesis, by eliminating work-related physical and psychological stress and increasing the ability to enjoy leisure time and to exercise, retirement should have positive effects on health. Such beneficial effects have been found in many quasi-experimental studies that use causal approaches.

Studies using instrumental variable (IV) approach looking at subjective and objective measures of physical and mental health have found positive effects of early retirement in various settings; Australia (Atalay and Barrett, 2015; Zhu, 2016), the UK (Bound and Waidmann, 2007), the US (Charles, 2004; Coe and Lindeboom, 2008; Gorry et al., 2015; Insler, 2014; Neuman, 2008) and various European countries (Coe and Zamarro, 2011; De Grip et al., 2012; Eibich, 2015; Hessel, 2016). Further, studies in Norway (Hernaes et al., 2013), Sweden (Hallberg et al., 2015), Switzerland (Lalive and Staubli, 2015) and the Netherlands (Bloemen et al., 2017) applying IV approach have shown decreased probability of mortality following retirement. The results of those studies suggest that postponing retirement could have negative effects on health.

Whereas the psychosocial-environmental hypothesis stresses the negative aspects of work and the positive effects of retirement on health, the *psychosocial-materialist* hypothesis suggests that working longer benefits health. This approach states that work provides financial, social, and psychological resources and is a key component of the identity of older adults. Retirement could thus lead to lack of purpose, loss of social interaction, less cognitive and physical stimuli, and financial insecurity, which negatively affect health. A vast body of quasi-experimental studies applying causal methods has found that retirement has negative effects, especially on cognitive functioning (Bonsang et al., 2012; Mazzonna and Peracchi, 2017, 2012; Rohwedder and Willis, 2010), but also mortality for men but not women (Kuhn et al., 2010) and physical and mental health (Behncke, 2012; Calvo et al., 2013; Dave et al., 2008; Heller-Sahlgren, 2017, 2012). However, using Swedish register data and an IV approach, Hagen (2018) found that a reform that raised retirement age by two years (from 63 to 65) had no effects on health care use or mortality up to age 69 for low- and middle-income women who worked in the public sector. He concluded that increasing the statutory retirement age might not affect health in the short run.

It is also possible that the effects of retirement on health might be curvilinear; that is, they might differ on the basis of whether retirement is considered early, on time, or late based on the person's age at the time of the retirement transition (Börsch-Supan and Jürges, 2009). The *cultural-institutional* hypothesis emphasizes the importance of the normative and institutional timing of events, and studies have found that physical and emotional health outcomes are better when experiences are in accordance with such timing (van Solinge and Henkens, 2007). Retirement transitions that occur outside the normative time frame might lead to more negative health outcomes than transitions at the socially accepted age. Using an IV approach and data from the US, Calvo et al. (2013) found that retiring early (before 62 years) had negative effects on emotional and self-reported physical health, whereas retiring at or after the normative retirement age had no effects.

1.2. Socioeconomic differences

The potential health effects of retirement might differ by socioeconomic position. Moreover, socioeconomic position might affect the ability or opportunity to prolong working life. People who occupy lower socioeconomic positions often enter the labor force at a younger age, spend the majority of their working lives in poorer working conditions, have fewer financial resources, experience worse health, and have a shorter life expectancy than people who occupy higher socioeconomic positions (Ravesteijn et al., 2013). Previous studies have found that socioeconomic position is the most important predictor of receiving disability benefits (Höög and Stattin, 2001; Knuth and Kalina, 2002). This phenomenon can partially be explained by the selection of initially less healthy people into more arduous occupations (Case and Deaton, 2005), but there is also evidence that exposure to strenuous working conditions, which is more common in lower socioeconomic groups, contributes to a decline in health (Ravesteijn et al., 2013). People who occupy lower socioeconomic positions might not be able to extend their working lives to the same degree as their more privileged peers because of the physical demands of their occupations or because of poor health, and would—according to the *psychosocial-environmental* hypothesis—therefore benefit from early retirement. Mazzonna and Peracchi (2017) found that people in physically demanding jobs experienced an improvement in their health and cognition if they retired early, whereas people in less physically demanding jobs experienced a reduction in their health if they retired early. Yet, even if they have health problems, people who occupy lower socioeconomic positions might need to extend their working lives for financial reasons, which could lead to poorer health outcomes in old age.

According to the *psychosocial-material* hypothesis, prolonging working life should lead to positive health outcomes. However, working longer might benefit people of higher socioeconomic position more than people of lower position because the latter might have more accumulated years on the labor market and experience adverse physical working conditions that counteract the benefits of retirement.

2. Aim

The overall aim of this study was to explore whether 1) prolonging working life affects late-life mortality and physical health, and 2) effects vary by a) occupational social class or b) the propensity to prolong working life beyond age 65 years.

3. Methods

3.1. Data

We used data from two nationally representative longitudinal studies that are linked at the individual level: The Swedish Level of Living Survey (LNU) and the Swedish Panel Study of Living conditions of the Oldest Old (SWEOLD). We also linked these data with information from the annual income register, Longitudinal Integration Database for Health Insurance and Labour Market Studies (LISA), and the Swedish Cause of Death Register, both of which cover the whole Swedish population. The LNU is a longitudinal, nationally representative study of

Table 1
Description of the data structure and cohorts.

Wave	Age range	Data source	Cohort 1	Cohort 2
			Birth year 1920–1934	Birth year 1929–1944
			Survey year	
T_0	40–71	LNU	1974/1981/1991	1981/1991/2000
T_1	70–85	SWEOLD	2004	2014
Number of people	n = 1064	n = 789	Total n = 1852	
Number of people alive at T_1	n = 740	n = 721	Total n = 1461	

the Swedish population between the ages of 18 and 75. It was first carried out in 1968, and subsequently in 1974, 1981, 1991, 2000, and 2010; the response rate varied between 91% in 1968 and 72.0% in 2010 (Evertsson and Magnusson, 2014; Fritzell and Lundberg, 2007). People from the LNU sample who have passed the upper age limit of 75 years are included in the SWEOLD study (Lennartsson et al., 2014). SWEOLD has been carried out in 1992, 2002, 2004, 2011, and 2014; response rates have varied between 84% and 95%. Together, LNU and SWEOLD allowed us to investigate the long-term effects of retirement age on late-life health in people of different socioeconomic positions while adjusting for a number of factors, including health, working conditions, and lifestyle before retirement.

The data structure is shown in Table 1. The first cohort was born between 1920 and 1934, and the second (overlapping) cohort between 1929 and 1944. We used pre-retirement measurements from LNU 1974, 1981, 1991, or 2000, as close to individual retirement timing as possible. These measurements are collectively called T_0. The health outcome, called T_1, was taken from SWEOLD 2004 for the first cohort and SWEOLD 2014 for the second cohort. After creating two longitudinal datasets, one for each cohort, they were appended, resulting in a sample size of 3,166, a number that includes those who died between T_0 and T_1. As the two birth cohorts overlapped, we excluded duplicates (n = 546), keeping the T_1 observation that was closest to age 75. To exclude those with long-term disability, we excluded people who had not accumulated more than nine years of labor market participation over their life course from our sample (n = 423). We also excluded individuals for whom information on retirement age was missing (n = 523); these were the youngest people in the sample, who might still be active in the labor market, or those who died before they retired. This resulted in an analytical sample of 1852 (used in the mortality analyses), of whom 1461 were alive when we measured the health outcome at T_1. This latter sample was used in all other analyses.

3.2. Measures

The outcome measures included mortality and four indicators of physical health: climbing stairs without difficulty, self-rated health (SRH), limitations in activities of daily living (ADL limitations), and musculoskeletal pain. The health outcomes were assessed with SWEOLD data from 2004 to 2014, when respondents were 70–84 years old. As there was an average of 12 years between retirement and the health outcomes, we also studied mortality to investigate potential selection bias due to a healthy surviving population.

Climbing stairs was a dichotomous variable. Respondents were asked if they could climb stairs without difficulty. Response alternatives were yes (0) and no (1). *SRH* was assessed with a single question: "How would you assess your own general state of health?" Response alternatives were good, poor, and neither good nor poor. The item was dichotomized (0 = good, 1 = less than good). *ADL limitations* were measured with five questions on the respondents' ability to perform various tasks without help from another person (Katz et al., 1963). The tasks were eating, using the toilet, dressing and undressing, getting into and out of bed, and hair washing. Those who were unable to perform all tasks independently were considered to have ADL limitations. *Musculoskeletal pain* was measured with the question "Have you had any of the following illnesses or ailments during the past 12 months?" The question was followed by a list of health problems, three of which concerned musculoskeletal pain (pain in the shoulders; in the back, hips, sciatic nerve; and in the hands, elbows, legs, or knees). Response alternatives for each item were no (0); yes, mild problems (1); and yes, severe problems (2). Responses were summed in an index ranging from 0 to 6; the variable was then dichotomized into 0 (no or one mild pain) and 1 (more than one mild pain or severe pain).

The main independent variable, *prolonging working life*, was assessed with annual income register data from Statistics Sweden, the Swedish national statistics agency. Retirement was defined as the age when annual individual income from pensions exceeded annual individual income from labor earnings for the first time. Income from pensions included all types of old-age pensions, occupational pension, early retirement pension, and disability benefits. Disability benefits are programs to support those who, because of long-term illness or for other reasons, cannot support themselves through paid work. Disability benefits are closely linked to the old-age pension system as recipients do not re-enter the labor market and are transferred directly to the guarantee pension when they reach age 65. Labor earnings included all types of income from work and from unemployment benefits. This definition has been used in earlier studies using Swedish register data (Barban et al., 2017; Stenberg et al., 2012; Svensson et al., 2015). Swedish pensions consist of a guaranteed pension, income pension, and premium pension. As of 1975, the eligibility age for the guaranteed pension, which is given to those with a low pension or who have no earnings-related pension, was 65 (Hagen, 2013), but the lower age limit for the income and premium pensions has been 61 since 2000. However, the social and cultural norm is still to retire at age 65. We thus defined *prolonging working life* as retiring at or above age 66. The exposure variable was dichotomized: 0 = retirement age equal to or below 65, 1 = retirement age equal to or above 66.

Moreover, we included *occupational-based social class* as an independent variable. This measure follows the official Swedish socioeconomic classification (Andersson et al., 1981), which in many ways corresponds to the internationally well-known Erikson-Goldthorpe (EGP) social class scheme (Erikson and Goldthorpe, 1992). Social class was measured before retirement and based on the person's main occupation: unskilled manual workers (0); skilled manual workers, lower non-manual workers I, small farmers without employees, and self-employed workers with no employees (1); lower non-manual workers II, farmers with extensive land and/or employees, and self-employed people with 1–19 employees (2); and intermediate and higher non-manual workers, people with academic occupations, and self-employed people with at least 20 employees (3). This categorization of social class is common in the literature (e.g. Kåreholt et al., 2011; Lennartsson et al., 2018).

3.3. Analyses

In an attempt to estimate causal effects using non-experimental data, we invoked a causal framework wherein the effect was defined as the difference in outcome between the scenario in which an individual receives a treatment (in this case, prolonging working life) and the counterfactual scenario in which a similar individual does not receive the treatment (Morgan and Winship, 2014). Specifically, we used propensity score matching (PSM) (Rosenbaum and Rubin, 1983), which is widely considered a suitable alternative for estimating effects in the absence of randomized data (Caliendo and Kopeinig, 2008; Stuart and Rubin, 2008). Studies on data from other countries have typically used an instrumental variable approach that employs statutory retirement age as an instrument (e.g. Hernaes et al., 2013; Mazzonna and Peracchi, 2017; Rohwedder and Willis, 2010). However, we were not able to use this approach as Sweden does not have a statutory retirement age, and gradual retirement is possible. PSM has been used to examine a similar research question with data from Great Britain (Behncke, 2012).

The advantage of PSM is that it is a balancing score: based on the propensity score, the distribution of observed baseline covariates will be similar between treated and untreated subjects (Austin, 2011a). We defined the treatment group as those who retired at or above age 66 (prolonged their working life) and the control group as those who retired at or before age 65.

PSM relies on the assumption that the treatment is exogenous and that the differences between the treatment group and the control group are due to the treatment (Rosenbaum and Rubin, 1983). This assumption is called the conditional independence assumption (CIA). To make the CIA more plausible, PSM generates propensity scores, enabling researchers to compare subjects with similar scores. The propensity score represents "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin, 1983). Estimating the propensity score is the first step in PSM and consists of a logistic regression that explains the determinants of employment transitions. Radius matching was used according to Austin's suggestion of optimal caliper width, where optimal width equals 0.2 of the standard deviation of the logit of the propensity score (Austin, 2011b); the caliper width was 0.027 in all matching procedures for the physical health outcomes and 0.025 in the mortality analysis. In addition, the common support condition guarantees that only people with suitable control cases are considered (Dehejia and Wahba, 1999). In the second step of PSM, algorithms form "statistical twins" with similar propensity scores. We used a rich set of control variables that are expected to influence both retirement age and late-life health. These variables were measured before the "treatment" in order to avoid endogeneity problems. Specifically, we used birth year, gender, years of education, limited financial resources, the socioeconomic status (SES) of the first occupation that lasted more than 6 months, partner's labor market status, physical working conditions, psychosocial working demands and psychosocial working control, overall occupational complexity, mobility limitations, musculoskeletal pain, psychological well-being, gastric problems and circulatory problems, the number of visits to a doctor in the past 12 months, smoking, physical activity, the period (outcome measured in 2004 or 2014), and the spell length (as the period between interviews in T_0 and the timing of retirement differs between individuals). The matching procedure was carried out separately for each outcome variable.

We present the results as average treatment effects on the treated (ATT). ATTs are defined as the expected difference in outcomes between the treated group and the control group. That is, they represent the health effect of prolonging working life for those who actually prolonged their working life. We used bootstrapping (200 repetitions) to create a sampling distribution of ATTs from which we could calculate the standard error and the 95% confidence intervals. All PSM analyses were performed using the Stata command *ado psmatch2* (Leuven and Sianesi, 2018) using Stata 15.

In order to analyze the heterogeneity of the treatment effects by occupational-based social class, we conducted the PSM analysis using the *teffects psmatch* function (Stata Press, 2013) with nearest neighbor (5) matching, that is, the five closest controls to the treated subject in its estimated propensity score (Austin, 2011a). To analyze whether the treatment effects differed by the propensity to prolong working life to age 66 or above, we used the smoothing-differencing (SD) method (Xie et al., 2012) using local polynomial regression of degree 1, common support, and the Epanechnikov kernel function. The SD method follows three steps: (1) it estimates the propensity score for all units; (2) it fits a separate, nonparametric regression of the dependent variable on the propensity score for the control group and the treatment group; and (3) it calculates the difference in the nonparametric regression line between the treatment and the control groups at different levels of the propensity score, enabling the researcher to obtain the pattern of the treatment effect heterogeneity as a function of the propensity score.

4. Results

Average retirement age in the full sample, including those who died before follow-up at T_1, was 62.5. Average retirement age in the surviving sample was 62.7 (median 64 years) (Table 2). About 20% of the sample worked to age 66 or above. The average age at outcome for the surviving sample was 75.3 years (Std. Error 0.1, range 70–85). A total of 29% of the sample had held an unskilled manual occupation, 29% a skilled manual occupation, 13% a lower non-manual occupation, and 29% an upper non-manual occupation. Table 2 shows the frequencies of the five outcomes and the distribution of the covariates used in the matching procedure.

Fig. 1 shows the mean standardized bias before and after matching for each covariate for all five outcomes. It is evident that the unmatched treatment and control groups differed substantially in terms of important confounding factors before matching. Those who retired before or at 65 were more likely to be women; to smoke; and to have fewer years of education, limited financial resources, poorer physical health and psychological well-being, a poorer physical working environment, less psychosocial control at work, and lower occupational complexity. There were no differences between the treated and control groups with regard to physical activities, psychosocial working demands, or partner's labor market status. The comparison of the mean standardized bias before and after PSM shows that matching significantly reduced the mean standardized bias for all covariates for all five outcomes to below the standard threshold of 5% (Caliendo and Kopeinig, 2008).

After reducing the mean standardized bias for the covariates, we assessed the ATT to ascertain whether there was an association between prolonged working life and late-life mortality and physical health (Fig. 2). The average effects of prolonging working life to 66 years or above were small and statistically non-significant on all five outcomes. Working to age 66 or above decreased the likelihood of dying before follow-up (T_1) by 3.9 percentage points. It also reduced the likelihood of being unable to climb and the likelihood of having ADL limitations by 2.3 percentage points. It had no effects on self-rated health or musculoskeletal pain.

In the next step, we stratified the results by occupational social class to assess whether the effects differed by occupational social class (Fig. 3). For skilled manual workers, working to age 66 or above decreased the likelihood of dying by 7 percentage points; this result was statistically significant. The results also suggested that prolonging working life may have protective effects on mortality in the other occupational social classes, but the effects were small and did not reach statistical significance.

The ability to climb stairs without difficulty was significantly higher (14 percentage points) among lower non-manual workers who had prolonged their working life. No effects were found for the other three occupational social classes. No significant effects on SRH, ADL limitations, or musculoskeletal pain were observed in any of the occupational social classes. Overall, the effect of prolonging working life on late-life mortality and physical health did not vary by occupational social class, with two exceptions: the protective effects on mortality among skilled manual workers and on the ability to climb stairs among lower non-manual workers.

Finally, we used the smoothing-differencing method (SD) to analyze whether the treatment effects varied by the propensity to prolong working life to age 66 or above (Xie et al., 2012). Fig. 4 shows the treatment effect across all propensity score strata for the five outcomes. The results showed that the effects were close to zero and did not reach statistical significance at any level of the propensity score. The exception is the gradual and negative slope of the SD curve on musculoskeletal pain at T_1, reaching statistical significance at 50% propensity, which suggests that the higher the propensity to prolong working life, the more beneficial the effects on musculoskeletal pain in late life.

Table 2
Sample descriptive of the full analytic sample (n = 1852) and the surviving sample at T_1 (n = 1461).

	Full sample at T_0		Surviving sample at T_1	
	Range/N	**%/Mean**	**Range/N**	**%/Mean**
Retirement age	40–74	62.5	40–74	62.7
Median (SD)		64 (4.24)		64 (4.16)
Prolonging working life				
Retire ≤ 65 (control)	1502	81.1	1163	79.6
Retire > 65 (treatment)	350	18.9	298	20.4
Age at outcome			70–85	75.2
Gender (ref. men)				
Men	956	51.6	706	48.3
Women	896	48.4	755	51.7
Vital status at T1				
Alive	1461	78.9	1461	100.0
Deceased	391	21.1		
Climbing stairs without difficulties at T1				
Yes			998	76.3
No			310	23.7
Self-rated health at T1				
Good			734	56.0
Less than good			578	44.1
ADL limitations at T1				
No limitations			1204	92.1
One or more limitations			104	8.0
Musculoskeletal pain at T1				
No or one mild pain			674	52.3
Yes, more than one mild pain			615	47.7
Covariates measured before retirement and used in matching:				
Occupational social class				
Unskilled manual workers	525	29.1	405	27.9
Skilled manual workers	524	29.1	397	27.3
Lower non-manual workers	225	12.5	194	13.4
Upper non-manual workers	528	29.3	457	31.5
Education (years of education)	0–34	9.7	0–34	10.0
Limited financial resources				
No	1348	77.3	1103	80.1
Yes	396	22.7	274	19.9
SES of first occupation				
Not working	19	1.1	1.16	1.2
Unskilled manual workers	997	58.1	766	55.5
Skilled manual workers	214	12.5	168	12.2
Lower non-manual workers	257	15.0	231	16.7
Lower/middle non-manual workers	21	1.2	16	1.2
Middle/higher non-manual workers	204	11.9	181	13.1
Higher non-manual workers	3	0.2	3	0.2
Partners labor market status				
Active in labor market	1389	78.5	1101	78.7
Out of labor market	260	14.7	204	14.6
Not cohabiting	121	6.8	94	6.7
Physical working conditions (0 = excellent)	0–17	3.4	0–17	3.2
Psychosocial working conditions; control (0 = much)	0–3	1.4	0–3	1.3
Psychosocial working conditions; demands (0 = easy)	0–20	1.1	0–20	1.1
Occupational complexity (0 = low complexity)	0–10	4.3	0–10	4.4
Mobility limitations (0 = no limitations)	0–4	0.5	0–4	0.5
Musculoskeletal pain (0 = no problems)	0–6	1.4	0–6	1.4
Psychological well-being (1 = no problems)	1–16	2.8	1–16	2.8
Gastric index (0 = no problems)	0–8	0.5	0–8	0.5
Circulatory index (0 = no problems)	0–11	0.8	0–11	0.7
Number of visits to doctor in past 12 months	0–40	2.3	0–40	2.1
Smoking				
No	797	47.6	656	49.6
Quit	217	13.0	195	14.7
≥ 10 cigarettes per day	323	19.3	204	15.4
< 10 cigarettes per day	336	20.1	268	20.3
Physically active				
Active at least once a week	1159	66.4	935	67.9
Active less often than once a week	586	33.6	443	32.2
Number of years between initial interview T_0 and retirement age	1–10	5.6	1–10	5.7
Number of years between retirement age and T_1			4–30	12.6

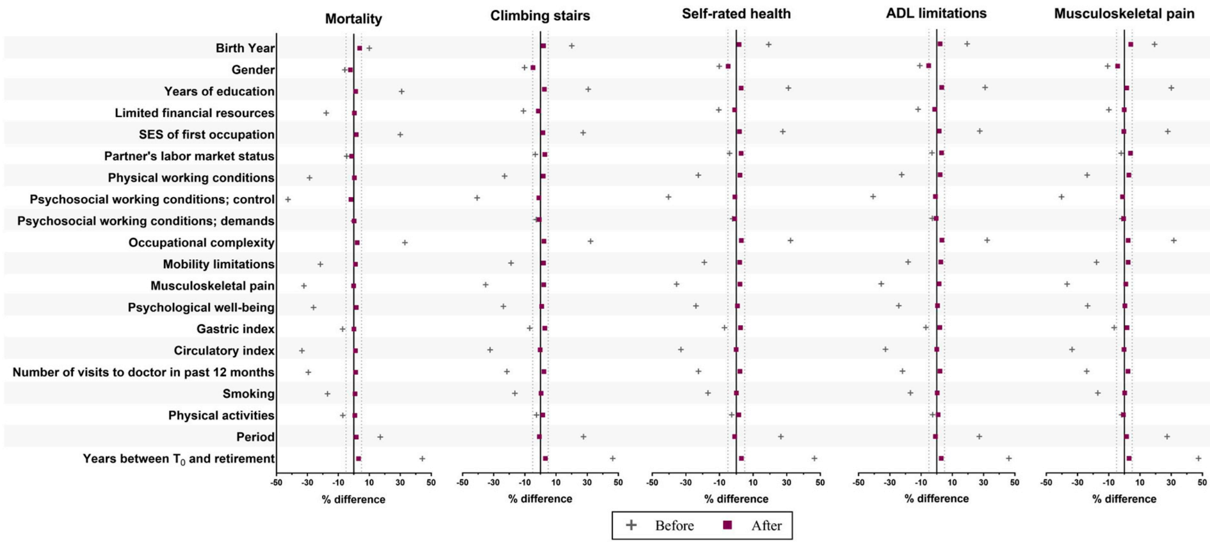

Fig. 1. Estimates for the matching on the propensity score for all five outcomes. The mean standardized bias before (grey plus sign) and after (purple square) matching for each covariate. The dotted vertical lines show the standard threshold of 5%. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

5. Discussion

This study examined the effects of prolonging working life on late-life mortality and physical health in a representative sample of the Swedish population. In addition, we addressed the question of whether the effects varied by occupational social class or by the propensity to prolong working life. To the best of our knowledge, this is the first study to use propensity score matching to examine middle- and long-term effects of prolonging working life on health outcomes. Using longitudinal, nationally representative Swedish data and PSM, we found no significant average effects of working to age 66 or above on mortality, the ability to climb stairs without difficulty, self-rated health, ADL limitations, or musculoskeletal pain an average of 12 years after retirement. As there were no significant effects on mortality in the overall analysis this implies that there was no or only little health-selection in the surviving sample. The analyses by occupational social class showed that a prolonged working life decreased the likelihood of dying for skilled manual workers. This might have led to a healthier surviving sample among the skilled manual workers who prolonged their working life, resulting in the effects on late life physical health outcomes appearing to be more positive than the true effects. The effects for all the physical health outcomes for this occupational social class were very close to zero with large confidence intervals; we do not believe that they would have reached statistical significance but might have indicated more negative effects of prolonging working life for skilled manual workers. Lower non-manuals workers had a better ability to climb stairs if they prolonged their working life. Lower non-manual occupations are e.g. lower office and sales jobs with low level of physical strain. Prolonging working life might allow them to maintain regular and social activities, positively affecting their physical abilities such as climbing stairs, supporting the psychosocial-materialist hypothesis. As the occupational social classes are quite small in size, we suspect there might be power issues and random error in these results, making it difficult to draw convincing conclusions. Overall, we found no systematic differences between the social classes in the health effects of prolonging working life.

The heterogeneity analysis by propensity score strata showed that those who had a high propensity to prolong working life were less likely to have musculoskeletal pain in late life. People with high propensity scores had held higher non-manual occupations, more than 20 years of education and good working conditions, finding support for the psychosocial-materialist hypothesis for this privileged group only. The

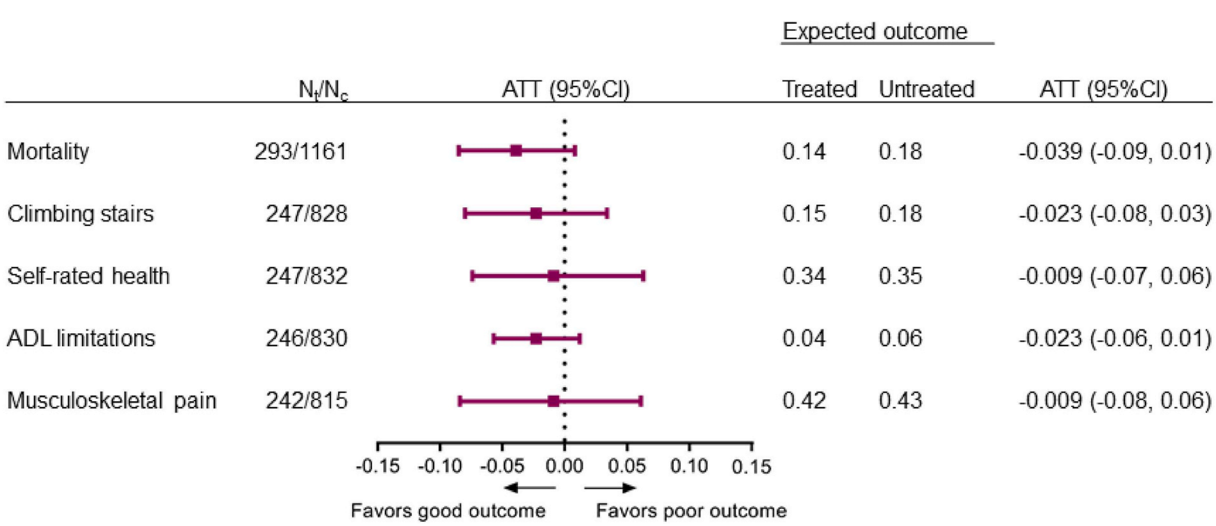

Fig. 2. Average treatment effects on the treated (ATT) and 95% confidence intervals (obtained by bootstrapping (200 repetitions)). N_t = the number of treated individuals, N_c = the number of controls.

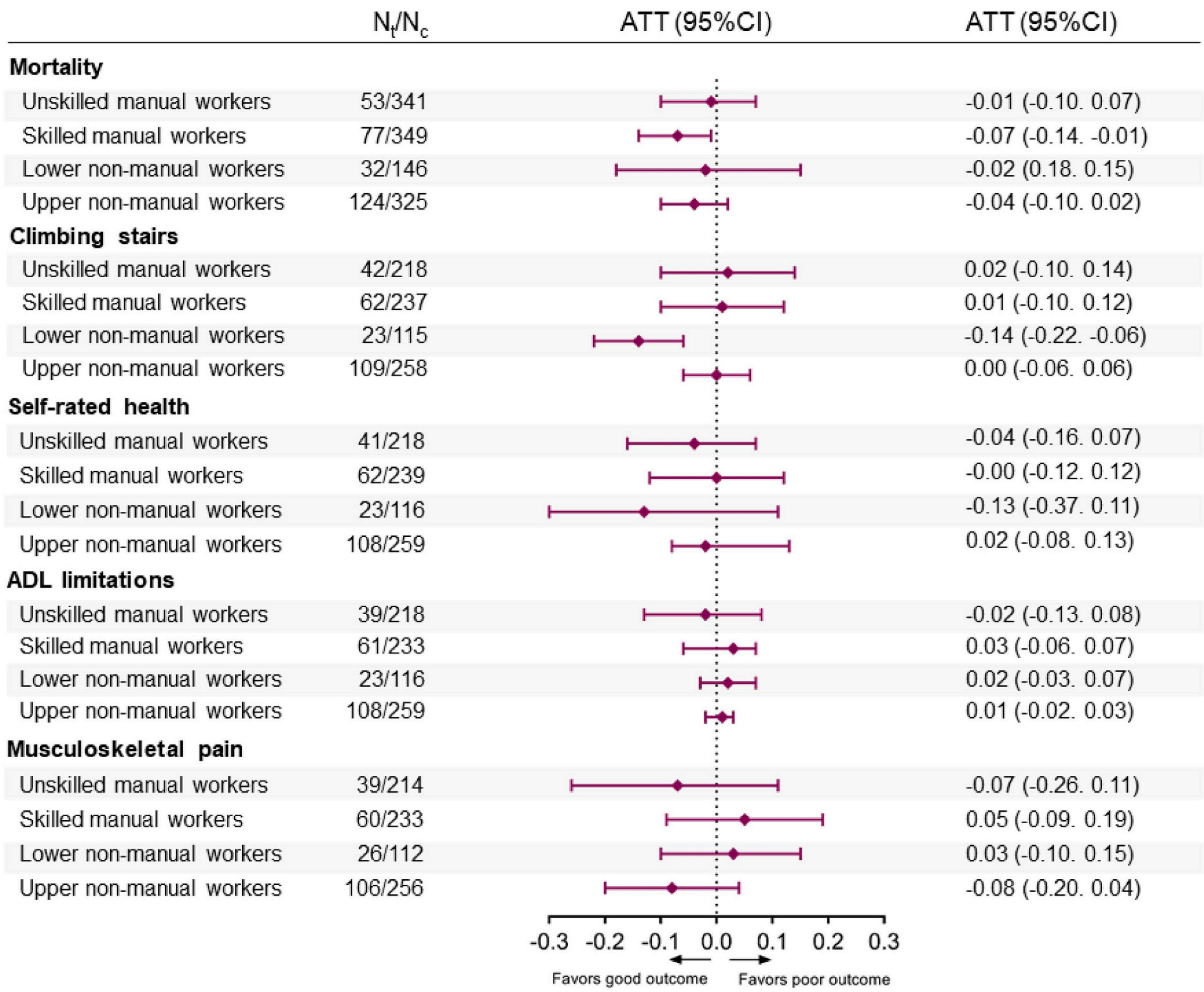

Fig. 3. Average treatment effects on the treated (ATT) and 95% confidence intervals by occupational social class (teffects method). N_t = the number of treated individuals, N_c = the number of controls.

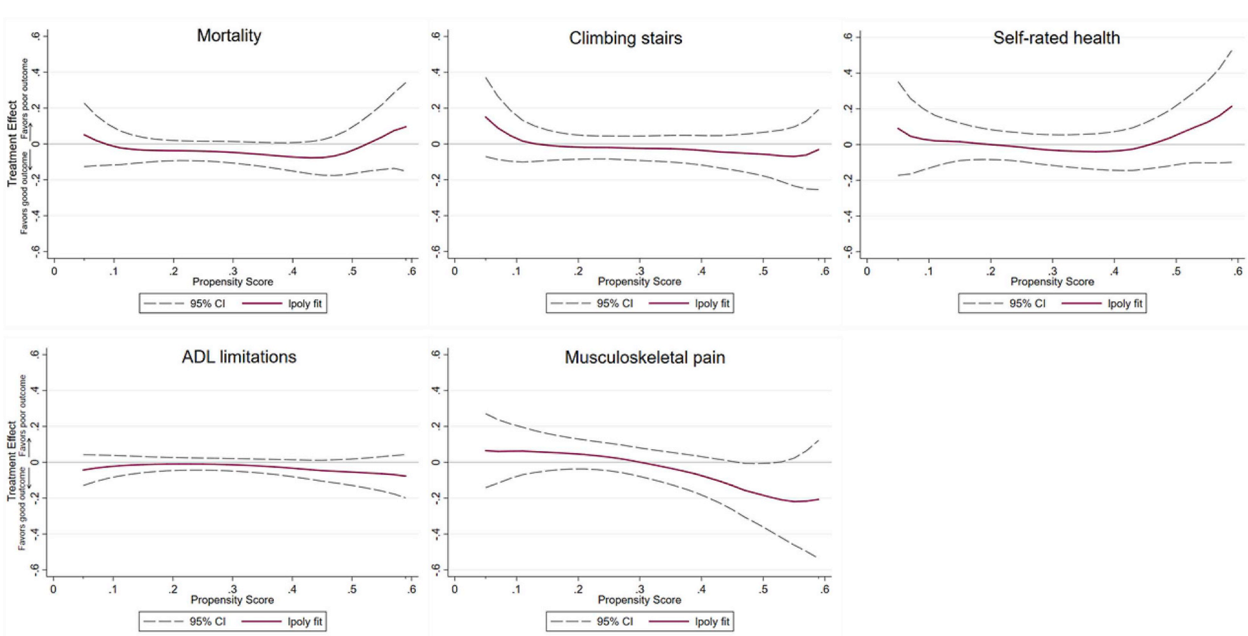

Fig. 4. Heterogeneous treatment effects (smoothing-differencing method) of prolonged working life on mortality, climbing stairs, self-rated health, ADL limitations, and musculoskeletal pain. Solid purple lines show the average treatment effect; dashed lines show the 95% confidence interval. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

level of propensity did not affect any other outcomes in this study, which leads us to conclude that working to age 66 or above did not have a considerable effect on mortality or late-life physical health.

This study differs from many others in that we investigated prolongation of working life beyond traditional retirement age, whereas most previous studies have examined how either retiring early or at the statutory retirement age affects health. Previous studies using quasi-experimental approaches have focused on early retirement reforms, and their results have supported the psychosocial-environmental hypothesis, the hypothesis that suggests that early retirement has beneficial health effects (e.g. Bloemen et al., 2017; Eibich, 2015; Hallberg et al., 2015; Hessel, 2016). Based on those results, one could assume that prolonging working life would contribute to health deterioration due to additional years of negative job stressors. Such an assumption might, however, be misleading, as the effects of early retirement might differ from the effects of late retirement. The current study, which had a long follow-up time, showed that, overall, prolonging working life had no significant effects on mortality or physical health in late life. Our results thus do not support the psychosocial-environmental hypothesis. The analyses of whether the results varied by occupational social class or the propensity to prolong working life were inconclusive but suggestive of a positive effect of prolonging working life on health outcomes for the more privileged groups in society, supporting the psychosocial-environmental hypothesis. As the occupational social classes and the group with very high propensity scores are quite small in size, we suspect there might be power issues and random error in these results, making it difficult to draw convincing conclusions. Our results are in line with those of a Swedish study that analyzed the short-term effects of increasing the normal retirement age on the purchase of prescription drugs, hospitalization, and mortality (Hagen, 2018). That study, which focused on women in low- and medium-income public sector jobs, used data from the Longitudinal Database on Education, Income and Employment (LOUISE) and an instrumental variable approach. The researcher found that raising retirement age did not affect the outcomes. The results of our study, which used a sample of the Swedish population, add the information that prolonging working life did not have long-term effects on mortality or physical health for any socioeconomic group. The findings of the present study and the study by Hagen (2018) indicate that the timing of retirement does not substantially affect health in old age. A potential explanation for these results may be that universal access to health care in Sweden, as well as other welfare benefits. Accordingly, inequalities i.e. in terms of income or occupation are buffered by almost equal access to welfare provisions.

To make sure that the matching process had not adjusted for socioeconomic differences prior to the analysis of the effects by occupational social class, we conducted two sensitivity analyses. In the first, we excluded years of education from the matching process. In the second, we excluded other indicators of socioeconomic position from the matching process, including years of education, limited financial resources, SES of first occupation, physical working conditions, psychosocial working conditions (control and demand), and overall occupational complexity. Socioeconomic position can be measured in different ways. Common indicators include education, social class, income, and occupational complexity, which, in turn, are all inherently associated (Grand and Tåhlin, 2013; Mirowsky and Ross, 2005). A Swedish study found that there are overlapping properties between different socioeconomic position indicators, such as income, occupational social class, education, occupational complexity and a SES-index, in relation to late-life health (Darin-Mattsson et al., 2017). The results of the sensitivity analysis, found in Table A1 in the Appendix, were similar to the results of the analysis on heterogeneity by occupational social class (Model 1 in Table A1 and Fig. 3). This suggests that the potential socioeconomic differences in treatment effects had not been adjusted for in the matching process (prior to the heterogeneity analysis by occupational social class) and that the results shown in Fig. 3 are therefore reliable.

Calvo et al. (2013) found that early retirement had negative effects on both emotional and physical self-reported health, but retirement at the statutory retirement age or later had no such effects. They highlighted the importance of the cultural-institutional hypothesis, which suggests retiring closer to the culturally and institutionally expected retirement age results in health benefits, whereas retiring outside the normative time frame can have deleterious health effects. Our study looked at people who worked to age 66 or above, beyond the culturally and institutionally expected retirement age in Sweden (age 65). Those who work beyond this age might have very positive work-related experiences (Stattin, 2009) or experience feelings of accomplishment, both of which could have beneficial effects on their health in late life. The cultural-institutional hypothesis might hold true for earlier but not for later retirement transitions: earlier retirement transitions might have negative health consequences, whereas later transitions might not (Calvo et al., 2013).

This study makes a methodological contribution. Many earlier studies on retirement and subsequent health have important methodological limitations that may result in conflicting findings and thus, conflicting conclusions and theories. Applying a quasi-experimental method that adjusts for potential reverse causality (in which health affects retirement age) and exploring effects by occupational social class in a nationally representative sample allowed us to investigate the effects of a prolonged working life on mortality and late-life health and examine whether these effects differed by socioeconomic position. We therefore suggest that such quasi-experimental methods could be useful in future studies on the association between retirement age and consequent health.

5.1. Strengths and limitations

An important strength of our study was the use of highly reliable register data in combination with nationally representative survey data, all of which covered the years before and after retirement. Survey response rates were high in all waves (~90%). In addition, the mortality register data allowed us to analyze whether health selection during the study period led to a selected sample at follow-up. We believe that using tax register data to estimate retirement age identifies people's actual exit from paid employment as well as self-reported retirement age. Moreover, we applied PSM, a method that addresses the endogeneity problems that studies on retirement age face. This method creates a causal framework that isolates the effects of prolonging working life on health in late life from confounding factors. However, although PSM has a number of desirable characteristics, it is not without limitations. The PSM matching procedure was successful for all five outcomes in this study, but some important unobserved covariates might be missing, which could result in biased ATT estimates. Examples of such covariates could include personality traits or genetic predisposition, two factors that might influence both the timing of retirement and health outcomes in late life. PSM is commonly criticized because it can be difficult to find matches for some subjects in the treatment group. However, this should be considered a strength of the method rather than a limitation, because the estimation of the treatment effects is limited to the region of common support, i.e. individuals that are not matched are not included in the analyses (Crown, 2014). In this study we did not have difficulty finding matches for everybody in the treatment group. However, the small sample size might explain the non-significant results. Moreover, we acknowledge that quasi-experimental methods do not guarantee that causal inference is possible.

5.2. Implications

The policy implications of our results should be interpreted with caution. We did not find any overall effects of prolonging working life to age 66 or above on mortality or physical health in late life. Analyses of whether the results varied by occupational social class or the propensity to prolong working life were inconclusive but suggestive of a positive effect of prolonging working life. Nevertheless, raising the eligibility age for pensions may create a pocket of workers who struggle to continue working to that age (Riley et al., 1994), which might prove troublesome as the financial incentives to prolong working life increase. Those who are unable to work to the new age of retirement eligibility would then have lower pension income for the remainder of their lives. Studies have shown that the associations between income and health and mortality remain after retirement; increased disparities in pension income could reinforce inequalities in late-life health and mortality (Korda et al., 2014; Rehnberg and Fritzell, 2016).

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Appendix. Sensitivity analysis

Table A1
Heterogeneous effects (teffects method) of prolonging working life on late-life mortality and physical health by occupational social class

	N_t/N_c	Model 1	Model 2	Model 3
		ATT (95%CI)	ATT (95%CI)	ATT (95%CI)
Mortality				
Unskilled manual workers	53/341	−0.01 (−0.10, 0.07)	−0.05 (−0.16, 0.06)	−0.03 (−0.14, 0.08)
Skilled manual workers	77/349	**−0.07 (-0.14, -0.01)**	−0.08 (−0.13, 0.02)	−0.05 (−0.14, 0.03)
Lower non-manual workers	32/146	−0.02 (0.18, 0.15)	−0.03 (−0.22, 0.15)	0.04 (−0.10, 0.18)
Upper non-manual workers	124/325	−0.04 (−0.10, 0.02)	−0.04 (−0.10, 0.02)	−0.03 (−0.09, 0.03)
Climbing stairs				
Unskilled manual workers	42/218	0.02 (−0.10, 0.14)	0.05 (−0.14, 0.15)	−0.01 (−0.14, 0.11)
Skilled manual workers	62/237	0.01 (−0.10, 0.12)	0.03 (−0.09, 0.14)	0.01 (−0.11, 0.12)
Lower non-manual workers	23/115	**−0.14 (-0.22, -0.06)**	**−0.16 (-0.28, -0.04)**	**−0.20 (-0.33, -0.07)**
Upper non-manual workers	109/258	0.00 (−0.06, 0.06)	0.01 (−0.05, 0.07)	−0.01 (−0.08, 0.07)
Self-rated health				
Unskilled manual workers	41/218	−0.04 (−0.16, 0.07)	−0.03 (−0.17, 0.11)	−0.05 (−0.19, 0.10)
Skilled manual workers	62/239	−0.00 (−0.12, 0.12)	0.00 (−0.11, 0.11)	−0.03 (−0.14, 0.07)
Lower non-manual workers	23/116	−0.13 (−0.37, 0.11)	−0.15 (−0.37, 0.08)	−0.14 (−0.36, 0.08)
Upper non-manual workers	108/259	0.02 (−0.08, 0.13)	−0.02 (−0.14, 0.10)	−0.02 (−0.13, 0.09)
ADL limitations				
Unskilled manual workers	39/218	−0.02 (−0.13, 0.08)	−0.04 (−0.14, 0.06)	−0.01 (−0.11, 0.08)
Skilled manual workers	61/233	0.03 (−0.06, 0.07)	0.01 (−0.05, 0.07)	−0.03 (−0.11, 0.06)
Lower non-manual workers	23/116	0.02 (−0.03, 0.07)	0.03 (−0.03, 0.09)	**0.03 (0.00, 0.06)**
Upper non-manual workers	108/259	0.01 (−0.02, 0.03)	−0.00 (−0.02, 0.01)	−0.02 (−0.05, 0.02)
Musculoskeletal pain				
Unskilled manual workers	39/214	−0.07 (−0.26, 0.11)	**−0.13 (-0.25, -0.01)**	**−0.13 (-0.25, -0.08)**
Skilled manual workers	60/233	0.05 (−0.09, 0.19)	0.01 (−0.12, 0.15)	0.01 (−0.12, 0.14)
Lower non-manual workers	26/112	0.03 (−0.10, 0.15)	−0.01 (−0.22, 0.21)	0.03 (−0.03, 0.09)
Upper non-manual workers	106/256	−0.08 (−0.20, 0.04)	−0.05 (−0.18, 0.08)	−0.00 (−0.02, 0.01)

Average treatment effects on the treated (ATT) and 95% confidence intervals by occupational social class. Nt = the number of treated individuals, Nc = the number of controls. Bold font indicates statistically significant estimates at the 95% level.
Model 1: Heterogeneity analysis by occupational social class (corresponding to Fig. 3). Covariates used for matching: birth year, gender, years of education, limited financial resources, the socioeconomic position (SES) of the first occupation that lasted more than 6 months, partner's labor market status, physical working conditions, psychosocial working demands and psychosocial working control, overall occupational complexity, mobility limitations, musculoskeletal pain, psychological well-being, gastric problems and circulatory problems, the number of visits to doctor in the past 12 months, smoking, physical activity, the period (outcome measured in 2004 or 2014) and the spell length (as the period between interviews in T_0 and retirement differs between individuals).
Model 2: Excluding years of education from matching process.
Model 3: Excluding years of education, SES of first occupation, limited financial resources, physical working conditions, psychosocial working control and demand, and overall occupational complexity from the matching process.

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