ARC Centre of Excellence in Population Ageing Research

Working Paper 2019/3

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Erik Hernaes, Zhiyang Jia, John Piggott and Trond Christian Vigtel

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Erik Hernæs[†] Zhiyang Jia[‡] John Piggott[§] Trond Christian Vigtel[†]

This version: April 2, 2019

Abstract

This paper studies the effect on the labor supply decisions of senior workers of reducing the eligibility age of retirement combined with actuarial neutrality, based on one particular group of private sector workers. In the 2011 Norwegian pension reform they had a fixed pension access age of 67 replaced by a flexible access age from 62 with constant present value of benefits. In a non-linear difference-in-difference approach, exploiting the absence of earnings tests, we find no effect on labor force participation. Aggregate earnings fell, mostly driven by high earners reducing their earnings. The increased liquidity seems to facilitate phased retirement.

JEL Classification: J14; J23; J26

Keywords: Retirement; Pension; Flexibility

*Data leased from Statistics Norway have been essential for the paper. We also thank *Fellesordningen for AFP* for providing valuable information on the AFP affiliation of firms. The data are used in compliance with Norwegian law ("Personopplysningsloven"). We gratefully acknowledge support from the Research Council of Norway (*Working Life After the Pension Reform*, project number 220-746 and *Understanding Retirement Decisions*, project number 238-203) and the ARC Centre of Excellence in Population Ageing Research (CEPAR) at the University of New South Wales. We thank seminar participants at the 26th Colloquium on Pensions and Retirement Research and the 9th Annual APRU Research Conference on Population Aging for comments. We would also like to thank Stephen Nyce, of Willis Towers Watson, for providing useful background material.

[†]Ragnar Frisch Centre for Economic Research, Gaustadalleen 21, 0349 Oslo, Norway. E-mail addresses: erik.hernas@frisch.uio.no (Hernæs, corresponding author) and t.c.vigtel@frisch.uio.no (Vigtel).

[‡]Statistics Norway, Box 8131, 0033 Oslo, Norway. E-mail address: zhiyang.jia@ssb.no.

[§]ARC Centre of Excellence in Population Ageing Research (CEPAR), UNSW Business School, University of New South Wales, Sydney 2052, Australia. E-mail address: j.piggott@unsw.edu.au.

1 Introduction

The access age for retirement benefits is generally seen as a key driver of the timing of the retirement decision. Accordingly, as life expectancy increases, more than a dozen countries in the OECD group have increased access age to retirement benefits to induce workers to postpone their withdrawal from the labor force. Such reforms necessarily reduce flexibility in labor force withdrawal, and often involve changes both in economic incentives and in options. This paper uses the Norwegian pension reform in 2011 to study the introduction of additional flexibility in old-age pension access age, separate from changes in economic incentives. For a particular group of workers impacted by the reform package, a fixed retirement pension access age of 67 was replaced by a non-earnings tested old-age retirement pension available from age 62, where payments are adjusted to hold constant the present value of benefits.[1] The workers studied here were not exposed to any changes in economic incentives, only to a new flexible option providing additional liquidity from age 62. This allows for identification of the pure liquidity effect of a more flexible pension. We base our analysis on comprehensive administrative data on labor earnings and weekly working hours. Results are based on a non-linear difference-in-difference approach, where the earnings and weekly working hours behavior of those impacted by the reform are compared with workers whose retirement trajectories preceded the reform.

We find that the reform does not lead to an earlier, but rather to a somewhat more gradual exit from the labor market. In particular, some of the high earners reduced their annual earnings after becoming eligible for the new pension at age 62. Aggregate earnings over the age range 62-65 among male workers who were employed at age 59 fell by 18.7 percent (EUR 8,499), but we find no effect on labor force participation.[2] We also find that the fraction in part-time work increased by 7.8 percent, and the fraction in full-time work decreased by 8.7 percent, indicating that the reduction in earnings we find is caused by the gradual retirement of some high earners reducing their earnings by means of reducing their working hours.

The question of how best to facilitate partial or phased retirement is currently receiving policy attention (Eurofound 2016). A flexible pension can have important welfare effects by allowing individuals an expanded range of choice with regard to labor market exit, and we find a statistically significant increase in more gradual retirement. Since a majority of the eligible workers claim the pension, this involves a combination of work and pension benefits.[3] We find that the

1. The actuarial adjustment is designed to be neutral, but there is the same implicit discount rate in the system for all, and deviations from the subjective discount rate could give incentives for early or late claiming (see e.g. Brinch et al. 2018). For instance, there is a strong mortality gradient in income which could lead to deviations from the system's implicit discount rate.

2. There were very few females in the group of workers chosen for the analyses, in particular because too few females had sufficient public old-age pension entitlements to be able to claim the new pension from age 62.

3. For males aged 62-65 after the reform who claimed old-age public pension benefits, 60.7 percent combined claiming and having approximately the same earnings level as they had at age 61. The data for this statistic covers the birth cohorts 1946-1952 over the years 2011-2014, and where "approximately same earnings level" is defined as earnings greater than 80 percent of earnings at age 61. If we instead condition the outcome on not being retired,

net effect of the flexibility and the associated increase in gradual retirement is to reduce work effort among the elderly. Therefore, the notion that reduced work per period may make it easier to continue to work for longer, thus increasing the net amount of work, is not borne out in our setting. This is consistent with an international comparison by Börsch-Supan et al. (2018) who find little response to the introduction of gradual retirement schemes. Similarly, Eurofound (2016) examines gradual retirement and concludes that there was "no scheme [...] identified that unambiguously extended working lives for all participants" (p.1).

However, in many of these cases incentives and flexibility are not separated, most likely due to (i) complex institutional arrangements, (ii) lack of reforms that allow identification and (iii) lack of suitable data, all of which makes it difficult to identify the separate impacts of incentives and options. By contrast, the group we study face changes only in liquidity, not economic incentives. The more gradual retirement and reduced working hours that we find may be caused by liquidity constrained workers who will now have the option of leaving work earlier than they otherwise would have done, even though the pension is actuarially adjusted.[4]

There are a number of possible reasons why more widespread gradual labor market exit is not observed. Employers may or may not be willing to accommodate reduced hours. There may be fixed costs to having an employee such as office space; there may be certain tasks such as meetings that take the same amount of time for all and therefore a higher proportion for part-time employees; there may be a loss of skill through less practice in a part-time job; and there may be a loss of productivity for other employees because of interdependencies (see e.g. Hutchens and Grace-Martin 2006; Hutchens 2010; Blau and Shvydko 2011; Even and MacPherson 2004; Cahill et al. 2014).

For the individual there may be a fixed disutility to having a job, such as restrictions on leisure activities, commuting time and strains from work (Fan 2015; Angrisani et al. 2015; Böckerman and Ilmakunnas 2017). On the other hand, a job may entail a stimulating environment (Kantarci and Soest 2013). Several surveys (Dalen 2016; Brown 2005; Tuominen 2013) report a desire by workers to gradually reduce work and by employers to retain the competence of experienced employees. In short, some jobs are fit for part-time, others are not, and some workers want this, while others do not.

The rest of the paper is organized as follows. Section 2 provides some institutional background to the Norwegian reform to place the policy reform that we focus on in a broader context. Section 3 describes our data and the sample used. Section 4 and Section 5 reports our difference-in-difference approaches and estimation results in terms of earnings and weekly working hours, respectively. In Section 6 we discuss job changes, while Section 7 concludes.

73.5 percent combined old-age public pension benefit claiming and having approximately the same earnings level as they had at age 61.

4. See e.g. Shoven and Slavov (2013) and Pashchenko and Porapakkarm (2018) for explanations of the extensive Social Security claiming at the earliest eligibility age of 62 observed in the United States.

2 Institutional setting

2.1 Before the reform

Up to the 2011 pension reform, there was little flexibility in pension claiming in Norway. The occupational pensions in the public sector and the public old-age pension had an eligibility age of 67, and most of the private sector occupational pensions had an eligibility age of 67. Some occupations had lower retirement age. There were generally strict earnings tests after the eligibility age and no deferral of benefits, going a long way to define age 67 as the retirement age, when earnings stopped and pension was received.

There were only two exit routes of any importance before eligibility age, the first of which was permanent disability pension. At age 66, around 40 percent of the population were on permanent disability benefits. The other exit route before the reform was the early retirement pension (*avtalefestet pensjon*, henceforth AFP) which was introduced in 1989. The AFP covers the public sector and around half of the private sector, where firms have to choose to participate to give employees the option of early retirement. In addition, individuals have to meet a set of requirement with respect to earnings and employment history. Before 2011, the AFP scheme covered the age range from 62 and up to the general pension age of 67. Those eligible had three options of combining work and pension benefits, with the percentage of normal earnings and the percentage of a full pension adding up to 100 percent. However, any pension benefit that was not claimed was not preserved, implying a high total tax of continued work (Hernæs et al. 2016).[5]

2.2 After the reform

Flexibility was introduced in the 2011 pension reform (Kudrna 2017; Brinch et al. 2016). The public pension, any occupational pension and the private sector AFP could all be claimed with actuarial adjustment, and without tests against continued earnings, between age 62 and age 75. The AFP in the private sector was then transformed from an early retirement pension over the age range 62-66 into a life-long pension as a supplement to the public pension. The annual benefit level was reduced so as to preserve the present value of the AFP public subsidy. Since this is now distributed among all who are eligible for the AFP, the individual present value is reduced compared to what it would be for those who would have claimed the AFP before the reform, while it is a gain for those who would not have claimed.

The reformed AFP in the private sector was still available only to those working in an AFP firm and meeting individual employment and earnings history requirements. The only requirement for claiming the AFP was that the public pension was also claimed, and many did so. In order to claim the new flexible public pension, one also has to meet certain minimum requirements

5. An illustration of the retirement transitions for the pre-reform 1942 birth cohort is deferred to Appendix A1.

on employment and earnings history. Among those who claimed the private sector AFP in conjunction with the public pension, the actuarially adjusted pensions and removal of the earnings test caused more people to continue working (Hernæs et al. 2016). Some of those who were covered by the pre-reform AFP were not eligible for claiming the new public pension from age 62. This group lost their early retirement option, in addition to having a large increase in the incentives for further work. In the public sector, the AFP was kept in the previous form. The only change was the option of claiming the new public pension from age 62, but only by relinquishing the AFP option. There were no changes to the disability benefits scheme as a result of the reform.

For those workers not covered by the AFP scheme, the only change was the access to the new, flexible old-age public pension from 62 for those who met the minimum entitlement requirements. Since the annual level is actuarially adjusted to preserve a constant present value, there were no changes in economic incentives. Hence, this group experienced only a change in the flexibility of pension, and this is the group we will analyze for impact on work behavior.[6]

3 Data and descriptive statistics

3.1 Data

The empirical basis for the analyses are extensive sets of administrative register data, on lease from Statistics Norway, and a data set with all private firms offering AFP, received from the early retirement administration unit. The two are linked by encrypted firm identification numbers. The register data sets cover the whole population of Norway and are linked by unique encrypted personal identification numbers. The most important information is annual earnings, weekly hours in the most important job each year, industry affiliation of the firm, worker occupation, wealth, age, gender and education.[7]

3.2 Sample

The large and abrupt changes in options for potential retirees following the 2011 pension reform provide a unique opportunity to investigate the impact of pure flexibility without any influence of changed incentives. For the analyses below, we use pre- and post-reform groups that are constructed in the same way to make them differ only in the pension system they were exposed to: individuals in the post-reform group had access to the new flexible pension from age 62, while individuals in the pre-reform group only had access to the old public pension at age 67.

6. For those workers not covered by the AFP scheme and who did not meet the individual requirements for claiming the new old-age public pension, there were no changes, neither in economic incentives nor in options.

7. All monetary amounts used in this paper are derived from amounts measured in NOK. The amounts are first deflated by the annual growth in the consumer price index (CPI). We have then scaled the CPI-adjusted amounts to 2014-EUR with the average exchange rate between EUR and NOK in that year (1 EUR = 8.35 NOK).

None had access to the private sector AFP. The difference is therefore only the introduction of flexibility and there are no changes in incentives. In a difference-in-difference setup, we use ages 60-61 as controls for general labor market changes and ages 62-65 as treated. Specifically, the sample is constructed as described below and illustrated in Table 1 for the 1949 birth cohort.

Table 1: Sampling in the 1949 birth cohort

	All	Males	Females
(1) Birth cohort 1949 at age 59	52,495	27,240	25,255
(2) Working at age 59 and no disability benefits	39,578	22,091	17,487
(3) Not covered by early retirement (AFP) scheme	7,982	5,422	2,560
(4) Eligible for new old-age pension from age 62 after reform	5,943	5,012	931

Source: Authors' own calculations using data from Statistics Norway.
Note: Number of observations in 1949 birth cohort by sample restriction. Working is defined as earnings above EUR 10,000, while disability benefits consists of both temporary and permanent disability benefits.

In the first step we select from the birth cohorts 1944-1954 observed in the years 2009-2014. In the second step we restrict attention to those who in the year they become 59 (i) were employed, (ii) earned at least EUR 10,000 and (iii) did not receive disability benefits (either permanent or temporary). In the third step, we include from this group only those who were not eligible for AFP (due to their employer not participating in the AFP scheme). In the fourth step, we include only those who met the after-reform requirements for claiming the new public pension, with actuarial adjustment, at age 62. Before the reform, they would have access age at 67, with no deferral.[8] We only use males, since there are very few females in this category. Many women are employed in the public sector, and among those who are in private sector without AFP most do not meet the post-reform pension requirements. The year-age groups each comprise about 5,000 males, about 20 percent of the population group. As treatment group we use those aged 62-65, and as control group those aged 60-61. We include in the treatment group only those who became 62 after the reform. This will exclude observations at age 63 of the 1948 cohort, at age 64 of the 1947-1948 cohorts, and at age 65 of the 1946-1948 cohorts. In total, we end up with 140,729 observations consisting of 51,860 individuals.

While the pension point accrual formula allows for individuals to improve their annual old-age pension payout by working past the age of 62, there are several reasons why we do not expect this to be a driving force behind the observed labor supply behavior of our treatment group. Firstly, having conditioned the sample of individuals to be eligible for new old-age pension from age 62, most have the sufficient number of years of residence (40) to obtain the full basic old-age public pension (first pillar). Secondly, most have their 20 best earnings-years before

8. By including only those whom we have found not to be in the public sector or in a private sector firm with AFP at age 59, we are sure that they do not qualify for the AFP by the re-reform requirements. A small group of workers have a lower eligibility age, but we cannot identity these individuals in the data.

Table 2: Observations, LFP rate and annual earnings, by year and age

Age	2009	2010	2011	2012	2013	2014
	4,872	4,630	4,288	4,597	4,637	4,688
60	0.949	0.941	0.947	0.952	0.948	0.955
	64,651	65,971	69,642	70,506	73,722	74,762
	4,704	4,872	4,630	4,288	4,597	4,637
61	0.888	0.883	0.889	0.907	0.908	0.907
	60,101	59,632	63,867	68,372	67,803	70,077
	4,753	4,704	4,872	4,630	4,288	4,597
62	0.849	0.840	0.842	0.851	0.874	0.861
	55,884	55,571	56,888	61,220	64,682	63,461
	5,119	4,753	0	4,872	4,630	4,288
63	0.765	0.752	.	0.764	0.782	0.787
	49,184	49,182	.	52,379	56,186	57,819
	4,718	5,119	0	0	4,872	4,630
64	0.653	0.684	.	.	0.708	0.724
	40,272	43,638	.	.	48,824	50,976
	4,854	4,718	0	0	0	4,872
65	0.584	0.569	.	.	.	0.640
	35,839	34,132	.	.	.	42,365

Source: Authors' own calculations using data from Statistics Norway.
Note: Each cell shows number of observations (first row), LFP rate (second row) and average earnings in EUR (third row). The sample consists of those working at age 59 (without receiving disability benefits) and not covered by AFP, but meeting the requirements for claiming the new public pension at age 62. The observations numbers are slightly less than indicated in Table 1 (for the 1949 birth cohort), because of missing values for some of the covariates. We assign zero earnings for those not in the labor force.

the age of 62, which means that there is no further accumulation of the earnings-related old-age public pension (second pillar) beyond the age of 62. Thirdly, as demonstrated by Brinch et al. (2017) in a different but closely related setting, there is a lack of salience in the accrual incentives such that individuals do not take the old-age pension accrual into account when determining their labor supply.

3.3 Descriptive statistics

Table 2 gives the number of observations, the labor force participation (LFP) rate and the average annual earnings in the sample analyzed, with lines drawn to illustrate the difference-in-

Table 3: Descriptive statistics

	Control		Treatment	
	Before	After	Before	After
Labor force participation	0.92	0.93	0.71	0.78
Annual earnings				
Average	62,568	69,870	45,458	55,278
75th percentile	78,796	86,543	67,235	77,014
Median	56,963	63,276	45,600	52,763
25th percentile	41,667	46,926	0	18,688
Covariates, average values				
Years of education	12.8	12.9	12.7	12.8
Annual earnings ages 30-59	56,605	59,785	53,953	57,497
Net liquid wealth at age 59	37,245	29,978	59,064	58,236
Number of observations	19,078	36,362	38,738	46,551

Source: Authors' own calculations using data from Statistics Norway.
Note: Descriptive statistics for the treatment and control group, before and after the reform. Labor force participation is defined as labor earnings above EUR 10,000. Annual earnings and net liquid wealth are expressed in EUR.

difference structure and the exclusion of the "cross-over" cohorts who were 62 before 2011 and who became 63-65 after 2011. The control group is above the solid horizontal line and the treatment group below. Within the treated group, those to the right of the dashed "staircase" have had new option from eligibility age 62. Even at age 65, over half of those working at age 59 still worked. From Table 3 we notice an increase in the LFP rate in the control group after the reform, but an even stronger increase after the reform among the treated. Furthermore, both average and median earnings increased more in the treatment group than in the control group.

3.4 Measuring work

Our main measure of work, and therefore the way in which we measure gradual labor market exit, is annual earnings. Annual earnings year-by-year will capture aspects of work which weekly working hours do not, among them changes to less demanding and lower paid jobs, without a corresponding reduction in working hours. Our data give contracted weekly working hours of all jobs held within the year. However, even if we constructed average hours in all jobs over the year, this would only be on a monthly basis. We have therefore used total earnings in all jobs over the year as our main outcome measure, as it reflects work effort.[9]

9. While the hourly wage rate might be the most accurate measure of effort per unit of work, we do not have data on this for our whole sample. However, merging our sample from Table 2 with the Norwegian Wage Statistics for the years 2009-2014, which consists of a representative sample of private sector firms with precise information on hourly wages, we find little indicative evidence of substantial downward adjustment in mean (real) hourly wages for male workers aged 59 to 65, with an average decline of only 6.6 percent from age 59 to age 65.

However, we use the weekly working hours in the main job each year as a supplementary measure of work, since the extent of multiple job holdings is very low for both pre- and post-reform birth cohorts. The low incidence of multiple job holdings allows us to use the weekly working hours measure from the main job as a measure of the aggregate weekly working hours an individual works. To illustrate this, Table 4 shows the fraction of total earnings that comes from the main job over ages 59-65, separately for the pre-reform birth cohort 1945 and the post-reform birth cohort 1949.

Table 4: Earnings from main job, by birth cohort and age

	At age						
	59	60	61	62	63	64	65
Cohort 1945	96.6	97.0	96.9	96.4	96.7	96.6	95.8
Cohort 1949	96.4	97.1	97.0	95.9	95.3	95.5	95.5

Source: Authors' own calculations using data from Statistics Norway.
Note: Fraction of total annual earnings derived from the main job by age, for birth cohorts 1945 and 1949 from the sample described in Table 3.

Table 4 shows that there is hardly any difference between the birth cohorts, with earnings from the main job as a percent of total earnings declining only marginally over age and being in the range of 95-97 percent. As a result, we use the weekly working hours from the main job to measure the individual's aggregate weekly working hours to supplement our earnings measure of work and shed light on the gradual retirement process.

4 Earnings

4.1 Mean earnings

In order to first derive the impact of introducing the flexible old-age pension on mean labor earnings, we estimate the following linear difference-in-difference equation:

$$y_{i,a} = \alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \eta \Delta_{i,a} + \varepsilon_{i,a} \tag{1}$$

Here $y_{i,a}$ is the annual pre-tax earnings of individual i at age a. $X_{i,a}$ includes controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. $\Delta_{i,a}$ indicates the treatment variable, and equals one when individual i is in the treatment group after the reform and zero otherwise. D_a are dummy variables for age (with age 60 as the reference age) and D_t are dummy variables for year (with 2009 as the reference year). In order to explore the age-dependent effects of introducing the

Table 5: OLS estimation results

	A. Aggregate effect (1)	(2)	B. Age-specific effects (3)	(4)
Treatment effect (η)	-1875	-902		
	(806)	(626)		
Treatment effect at:				
Age 62 (η_{62})			-1449	-673
			(730)	(586)
Age 63 (η_{63})			-2219	-1554
			(902)	(729)
Age 64 (η_{64})			-1570	-375
			(1073)	(866)
Age 65 (η_{65})			-2672	-962
			(1290)	(1047)
Year and age dummies	✓	✓	✓	✓
Control variables	×	✓	×	✓
Adjusted R^2	0.049	0.302	0.049	0.302
Individuals (N)	51,860	51,860	51,860	51,860
Sample size ($N \times T$)	140,729	140,729	140,729	140,729

Source: Authors' own calculations using data from Statistics Norway.
Note: OLS estimation results of the aggregate reform effect on earnings (Panel A) from Equation (1) and the age-specific reform effects on earnings (Panel B) from Equation (2). Control variables are pre-determined and include linear controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. Standard errors (in parentheses) are clustered on the individual level, based on 200 non-parametric bootstraps.

flexible pension on mean earnings, we also estimate the following model:

$$y_{i,a} = \alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \sum_{a=62}^{65} \eta_a D_a \Delta_{i,a} + \varepsilon_{i,a} \tag{2}$$

Table 5 shows the OLS estimates for the aggregate effect on the earnings (Panel A) and the age-specific effects on earnings (Panel B). When only controlling for age and year dummies, there is a negative and significant effect in the aggregate (Column 1) while the age-dependent effects are significant for all ages except age 64 (Column 3). However, adding the pre-determined covariates (Column 2 and Column 4) makes the estimates of the reform effect statistically insignificant, with the exception of age 63.

Splitting the sample of workers into those above and those below the median in the earnings distribution at age 59, separately for the treatment and control group, we use a linear difference-in-difference approach to elicit how the reduced access age affected the behavior of these two

Table 6: OLS estimation results, by earnings at age 59

	A. Below median earnings		B. Above median earnings	
	(1)	(2)	(3)	(4)
Treatment effect at:				
Age 62 (η_{62})	-1556	-2056	-1459	-2470
	(475)	(546)	(1201)	(1301)
Age 63 (η_{63})	-2767	-3290	-3301	-3844
	(475)	(676)	(1544)	(1598)
Age 64 (η_{64})	-1531	-1989	-2235	-2840
	(748)	(792)	(1820)	(1845)
Age 65 (η_{65})	-1526	-2189	-3164	-4122
	(923)	(957)	(2142)	(2189)
Year and age dummies	✓	✓	✓	✓
Matched sample	×	✓	×	✓
Adjusted R^2	0.051	0.048	0.041	0.037
Individuals (N)	26,563	25,902	25,297	23,853
Sample size ($N \times T$)	70,364	63,175	70,365	59,072

Source: Authors' own calculations using data from Statistics Norway.
Note: OLS estimation results of the age-specific reform effects on earnings from Equation (2), separately for individuals below the median of the earnings distribution at age 59 (Panel A) and above the median of the earnings distribution age 59 (Panel B), based on propensity score matching. The propensity score matching is based on education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. Analytical standard errors (in parentheses) are clustered on the individual level.

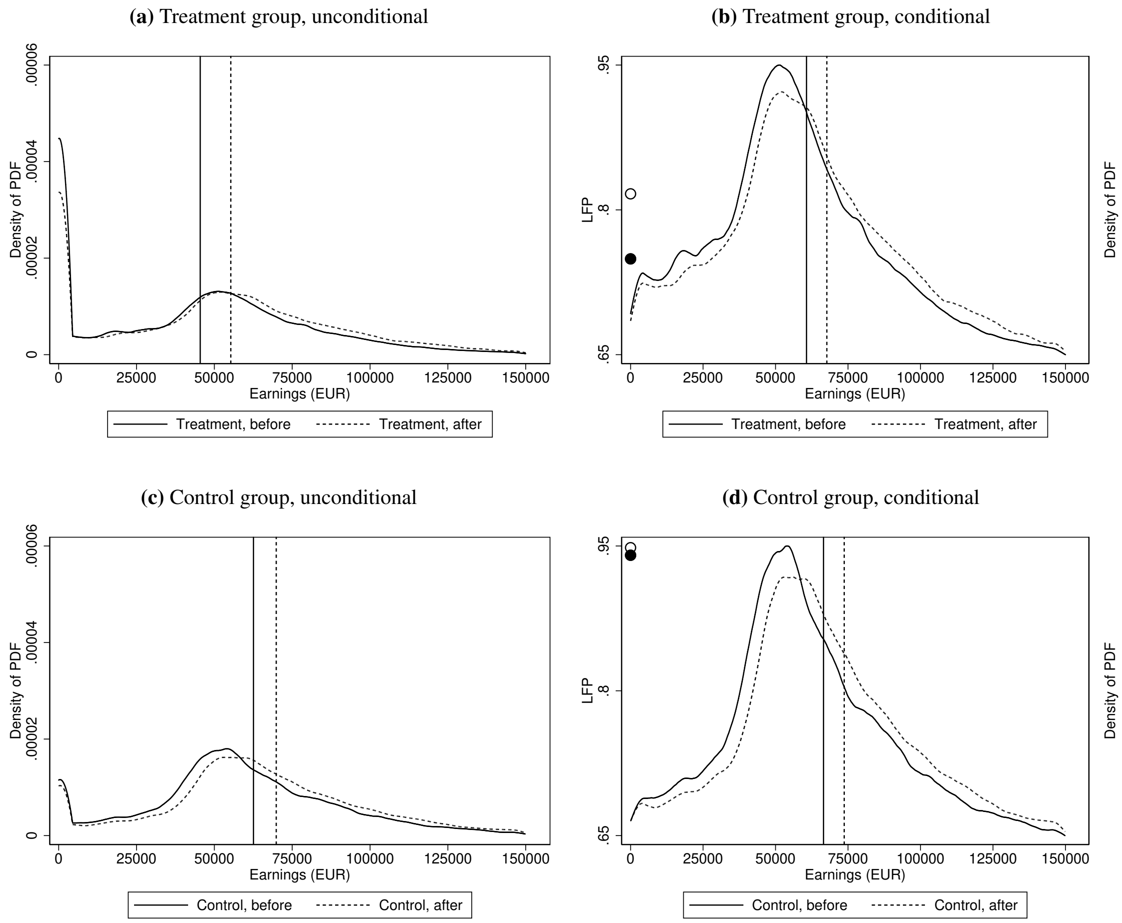
groups of workers. To do so, we estimate Equation (2) without covariates separately for those above and below the median, based on propensity score matching on the pre-determined covariates from Equation (2). The results from the estimated model are reported in Table 6.[10] In general, the reduction in earnings is stronger for the above-median group of workers (Panel B) compared to the below-median group of workers (Panel A).

While the results in Table 5 generally suggests no significant effects on the mean earnings when controlling for pre-determined covariates, the significant age-specific estimates in Table 6 suggest there might be differential effects across the earnings distribution. As a first overview, Figure 1 shows the shift in the empirical probability density functions (PDF) for the treatment group before and after the reform (Panel (a) for the PDF and Panel (b) for the PDF conditional on positive earnings) and the control group before and after the reform (Panel (c) for the PDF and Panel (d) for the PDF conditional on positive earnings). As is evident, there is no large treatment effect on the mean (illustrated by the shift from the solid to the dashed vertical line).

10. Details on the propensity score matching procedure, as well as the balancing test of the pre-determined characteristics after the matching procedure is performed, are available from the authors upon request.

However, there are differential changes to the shape of the earnings distributions, suggesting that the mean-outcome approaches in Table 5 and Table 6 are not sufficient to capture the full effect of introducing the flexible old-age pension.[11] We now turn to this question.

Figure 1: Changes in earnings distribution, by group



Source: Authors' own calculations using data from Statistics Norway.
Note: Shifts in the empirical PDFs of the earnings distribution, separately for the treatment (Panel (a) and Panel (b)) and control group (Panel (c) and Panel (d)). Panel (a) and Panel (c) show the PDFs unconditional on positive earnings, while Panel (b) and Panel (b) show the PDFs conditional on positive earnings. In Panel (b) and Panel (d), the right-hand axis shows the density of the PDF, while the left-hand axis shows the labor force participation rate (solid circle is before the reform, hollow circle is after the reform). Densities are estimated using an Epanechnikov kernel function with bandwidth of EUR 2,000 over a support of 1,000 points. The vertical lines show the mean of the earnings distributions before (solid) and after (dashed) the reform.

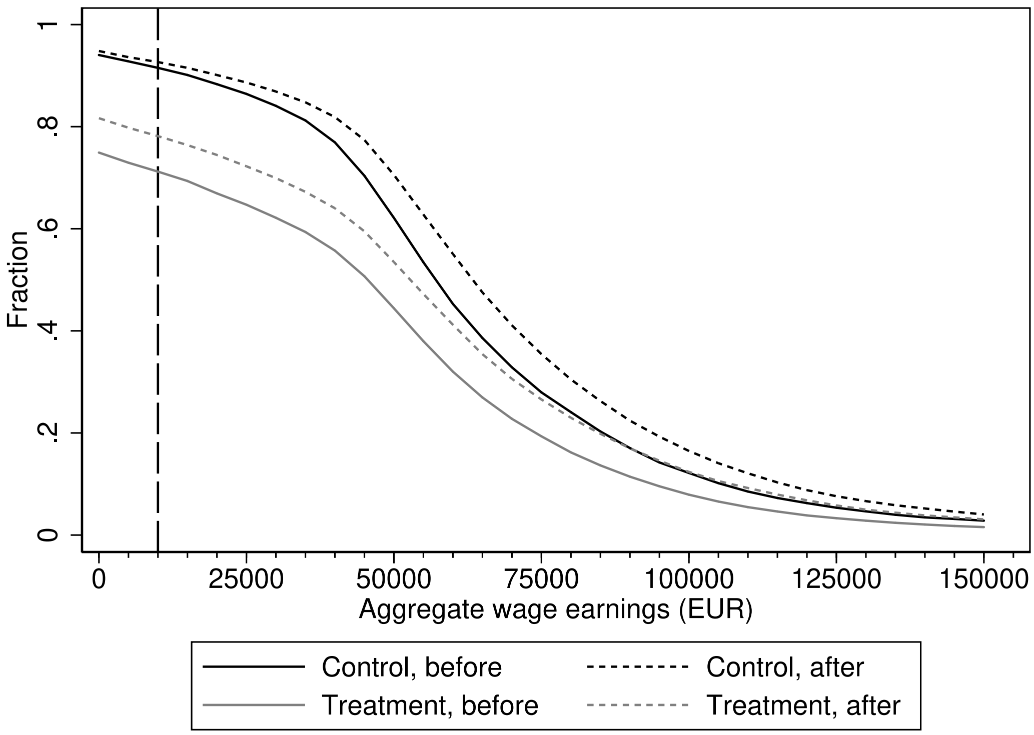
4.2 Earnings distribution

4.2.1 The CCDF method

To investigate the changes over the whole earnings distribution, we use the Complementary Conditional Distribution Function (CCDF). A CCDF is defined as 1 minus the cumulative dis-

11. To illustrate this with a conventional approach, we show the effect of introducing the flexible pension on earnings at different deciles of the (unconditional) earnings distribution using the recentered influence function difference-in-difference (RIF-DiD) approach, and compare this to the mean (OLS) impact of the reform from Table 5. The complete estimation results are available from the authors upon request.

Figure 2: Earnings distribution, CCDF



Source: Authors' own calculations using data from Statistics Norway.
Note: Empirical plot of the Complementary Conditional Distribution Function (CCDF) of aggregate wage earnings (expressed in EUR) over ages 60-65 using the sample described in Table 3 without controlling for covariates. The vertical dashed line indicates EUR 10,000.

tribution function. The construction of such graphs and the accompanying regression analyses of shifts and the impact of covariates are described in Hernæs and Jia (2013) and Brinch et al. (2017). When analyzing shifts and effects which can vary across the distribution, this is an alternative to quantile regressions (Lingxin and Naiman 2007). Intuitively, while quantile analysis models the horizontal shifts in the cumulative function, CCDF models the vertical shifts. Both are well suited for analyses of shifts which vary over the distribution, but the CCDF method is less cumbersome numerically.[12]

For an initial overview of our sample, Figure 2 shows the CCDF in the treatment and control group, before and after the reform. The graphs give the fractions with earnings above the horizontal axis values in steps of EUR 5,000. The starting point of a graph therefore shows the labor force participation (LFP) rate in a group, and the shape of the graph describes the distribution of earnings. The decline of the graph over an interval of the horizontal axis shows the fraction of the group in that interval, since the endpoints of the interval show the fractions above at the start and at the end of the interval. The steeper the graph is, the larger is the concentration of individuals in that part of the graph.

12. A simulation exercise illustrating the qualitative equivalence of the two methods is available from the authors upon request.

In the treatment group, the after-reform graphs start higher than the before-reform graph, indicating that the reform has increased the LFP rate. The shift is around 5 percentage points, which is in contrast to the control group where the LFP rate changes only slightly. In the control group the earnings distribution has shifted so that fewer are in the first segment (up to about EUR 50,000) and more individuals are above this segment, as the after-reform graph is steeper than the before-reform graph. In the treatment group, the distance between the two graphs is fairly constant up to this level, before the graphs converge (as they must).

One possible interpretation is that the increased labor supply in the treatment group is found in the lower part of the distribution. Another interpretation is that some who would previously have had high earnings, now have lower earnings. There could also be other differences, but we are not able to distinguish between the different interpretations using only earnings data. What we observe is the net result, with more people working and more people in the lower end of the earnings distribution. Visual inspection cannot go much further. In the following we use the control group systematically and also include covariates and dummies for time and age, which are not used in the construction of the graphs in Figure 1 and Figure 2.

4.2.2 Empirical approach

To extract more precise information, we ran a series of logit regressions of the probability of having earnings above a series of steps, each of length EUR 5,000, up to EUR 150,000. Intuitively, this explores vertical shifts in the distribution as illustrated in Figure 2. With a constant treatment effect for all years, but allowing for different treatment effects over ages 62-65, we assume that for each earnings level $y_{i,a}$ for individual i at age $a = 60,\dots,65$ this can be expressed as:

$$P\left(y_{i,a} > y_k\right) = F\left(\alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \sum_{a=62}^{65} \eta_a D_a \Delta_{i,a}\right) \tag{3}$$

Here $y_{i,a}$ is the annual pre-tax labor earnings of individual i at age a. $X_{i,a}$ includes controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. $\Delta_{i,a}$ indicates the treatment variable, and equals one when individual i is in the treatment group after the reform and zero otherwise. D_a are dummy variables for age (with age 60 as the reference age) and D_t are dummy variables for year (with 2009 as the reference year). We estimate the logit specification 31 separate times, letting y_k vary from EUR 0 to EUR 150,000 by increments of EUR 5,000 such that $y_k \in \{0, 5000, 10000, \dots, 145000, 150000\}$. The coefficients γ_a for $a = 61,\dots,65$ measure the age effects, while η_a for $a = 62,\dots,65$ measure the age-specific treatment effects. The age-specific treatment effects will capture any gradually increasing impact.[13]

13. While the assumption of a constant treatment effect for all years might seem restrictive, estimating a similar model where we instead hold age-specific treatment effects constant and allow for different treatment effects

The graphs in the following are all based on 31 separate estimations, one for each of the earnings levels described above. For each estimation, we find the marginal effect of treatment evaluated at the covariate values equal to the average of the treatment group in the post-reform period ($\bar{X}_{i,a}$). These are used to simulate the CCDF of the earnings distributions of the type in Figure 2, which then shows the marginal effects equal to the difference in the post-reform and pre-reform probability of earnings more than a given level y_k: $P(y_{i,a} > y_k|\bar{X}_{i,a}, \Delta_{i,a} = 1) - P(y_{i,a} > y_k|\bar{X}_{i,a}, \Delta_{i,a} = 0)$. As some individuals are in the control group one year and later on in the treatment group, we need to take this into account when computing standard errors. The standard errors reported are bootstrapped standard errors, with individuals as the unit.

4.2.3 Average reform effect

The first graph (Figure 3) show the difference between a simulation with $\Delta_{i,2010} = \Delta_{i,2011} = \Delta_{i,2012} = \Delta_{i,2013} = \Delta_{i,2014} = 1$ and a simulation with $\Delta_{i,2010} = \Delta_{i,2011} = \Delta_{i,2012} = \Delta_{i,2013} = \Delta_{i,2014} = 0$.[14] The graph shows the average effect over all ages and years of the reform on the probability of having earnings above the earnings levels of the horizontal axis, with controls and covariates for individual characteristics (set at average of the treatment group in the post-reform period). Negative values mean the after-reform graph is below the pre-reform graph. When the graph falls, the concentration is higher in the post-reform group, and the steeper it is, the larger is the difference in the concentration. When the graph increases, the concentration is larger in the control group. The area around the graph with point estimates gives the 95% confidence interval. Since fewer and fewer have earnings above the horizontal axis values when we move to the right in the graph, all simulated distributions must go towards zero, as will the difference.

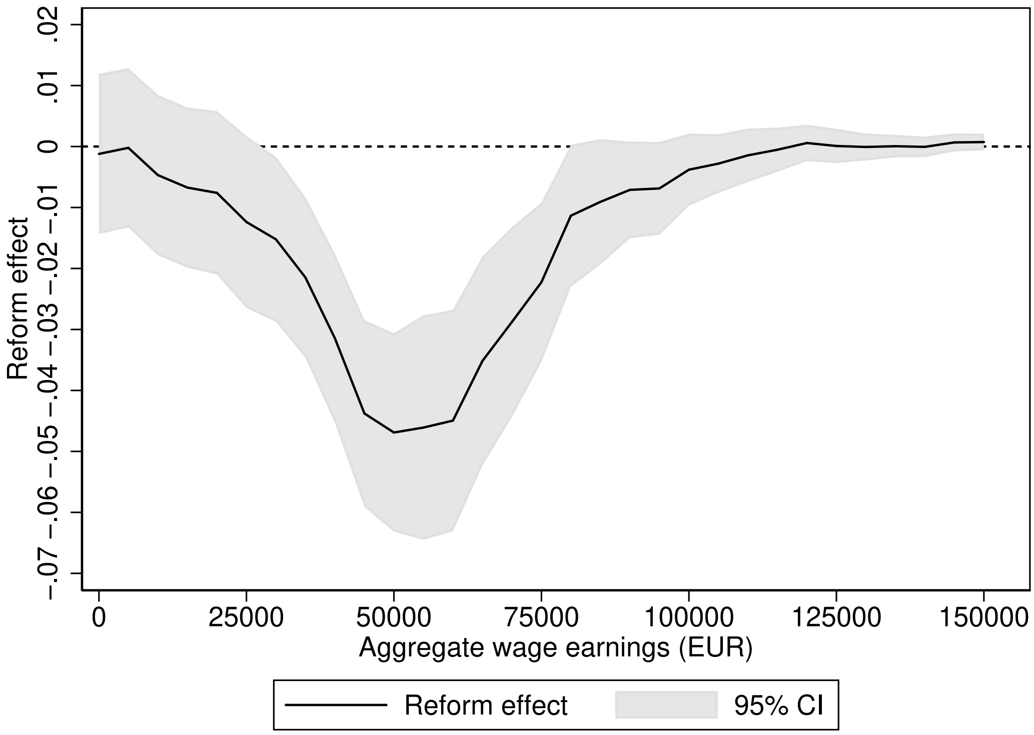
The point estimates in Figure 3 start a little below zero, which means that the reform has resulted in fewer people with positive earnings and therefore in the labor force. However, the estimates are not significant for earnings below EUR 20,000.[15] At EUR 50,000 (approximately 3/4 of average full-time earnings for men in 2014), the point estimate is significantly negative at -4.7 percentage points, with a 95% confidence interval from -3.1 to -6.3 percentage points. That means the reform has increased the fraction of the sample with earnings up to about EUR 50,000 by about 5 percentage points, which is mirrored by a reduction in the fraction above EUR 50,000. Since the difference between the treatment and control group in the CCDF at EUR 50,000 is negative, the difference in the CDF at the same earnings level is positive. This

over years yields fairly similar effects across years. The empirical approach for the year-by-year approach and associated estimation results are deferred to Appendix A2.1.

14. The following model is estimated: $P(y_{i,a} > y_k) = F\left(\alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \eta\Delta_{i,a}\right)$.

15. While tabulating the raw difference in the rates from Table 3 suggests a positive reform effect on labor force participation, controlling for year and age dummies in a linear difference-in-difference approach with an indicator for labor force participation as outcome variable indicates that much of this difference is driven by age effects (results not reported).

Figure 3: Reform effect on earnings distribution, all years



Source: Authors' own calculations using data from Statistics Norway.
Note: Simulation results from estimation of an age-aggregated version of Equation (3), showing the difference in the CCDF for the treatment group and the control group. The gray-shaded area shows the 95 percent confidence intervals (based on 200 non-parametric bootstraps for each estimation, clustered on individual level). Aggregate earnings are expressed in EUR. Marginal effects and the associated standard errors are deferred to Appendix A2.2.

translates into a greater mass in the earnings distribution below EUR 50,000 as a result.

The corresponding shifts in the probability of being in different intervals of the earnings distribution are shown in Table 7. The shifts in the earnings distribution are compatible with more gradual retirement.

Table 7: Probability of being in selected earnings intervals

Earnings interval	Density before reform	Reform effect: Estimate	Reform effect: Standard error
Non-participation (EUR 0)	0.251	0.001	(0.007)
EUR 0 - EUR 30,000	0.128	0.014	(0.010)
EUR 30,000 - EUR 50,000	0.177	0.032	(0.011)
EUR 50,000 - EUR 80,000	0.282	-0.036	(0.010)
> EUR 80,000	0.162	-0.011	(0.006)

Source: Authors' own calculations using data from Statistics Norway.
Note: Probability of being in different earnings intervals and the reform effect on the probability of being in the different intervals. The reform effect is generated using the marginal effects from Figure 3, with pooled standard errors in parentheses. The procedure is described in detail in Appendix A2.3.

4.2.4 Age-specific reform effects

The age-by-age results from estimating Equation (3) are illustrated in Figure 4. Looking at the starting points in Figure 4, the LFP rate is 0.4 percentage points higher for age 64 and 0.8 percentage points for age 65 but not statistically significant. In all of the panels, the point estimates show larger effects on the lower part of the earnings distribution, up to about EUR 50,000. It seems that the flexible claiming option gradually increases labor market withdrawal (by means of reducing earnings) as far as we can follow, which is up to age 65. From the age-by-age analysis, there also seems to be a state dependence in labor supply. However, this state dependence translates into reduced earnings among those working over the ages 62-65, since more people are found in the below-average earnings range. This suggests that the flexible claiming option to some degree facilitates gradual retirement, a point which we will explore further in Section 5. The changes in the probabilities of being in different earnings intervals by age are illustrated in Figure 5, and shows the dynamic effects of the reform.

4.2.5 Placebo and robustness checks

As a placebo exercise, we use a year-by-year version of Equation (3) to derive the year-specific reform effects (as detailed in Appendix A2.1) and estimate the reform effect across the whole earnings distribution in the pre-reform year 2010. As the reform had not been implemented at this stage, we should expect there to be no effect on the changes in the earnings distribution. The estimation results from this exercise are illustrated in Figure 6, and show only marginally significant effects at the lower part of the earnings distribution.[16]

While the largely insignificant effects in the placebo exercise suggest that our difference-in-difference approach is valid, it is worthwhile examining the robustness of our results further. As a robustness check, we follow Brinch et al. (2017) and include group-specific linear time trends (estimated on pre-reform data covering the period 2006-2010) in Equation (3) to discern whether there are secular group-specific trends driving our results.[17] The estimation results (reported in Appendix A2.5) are largely similar to the estimation results for the average reform effect reported in Section 4.2.3, suggesting that there are no significant differential trends in earnings that are driving our results.

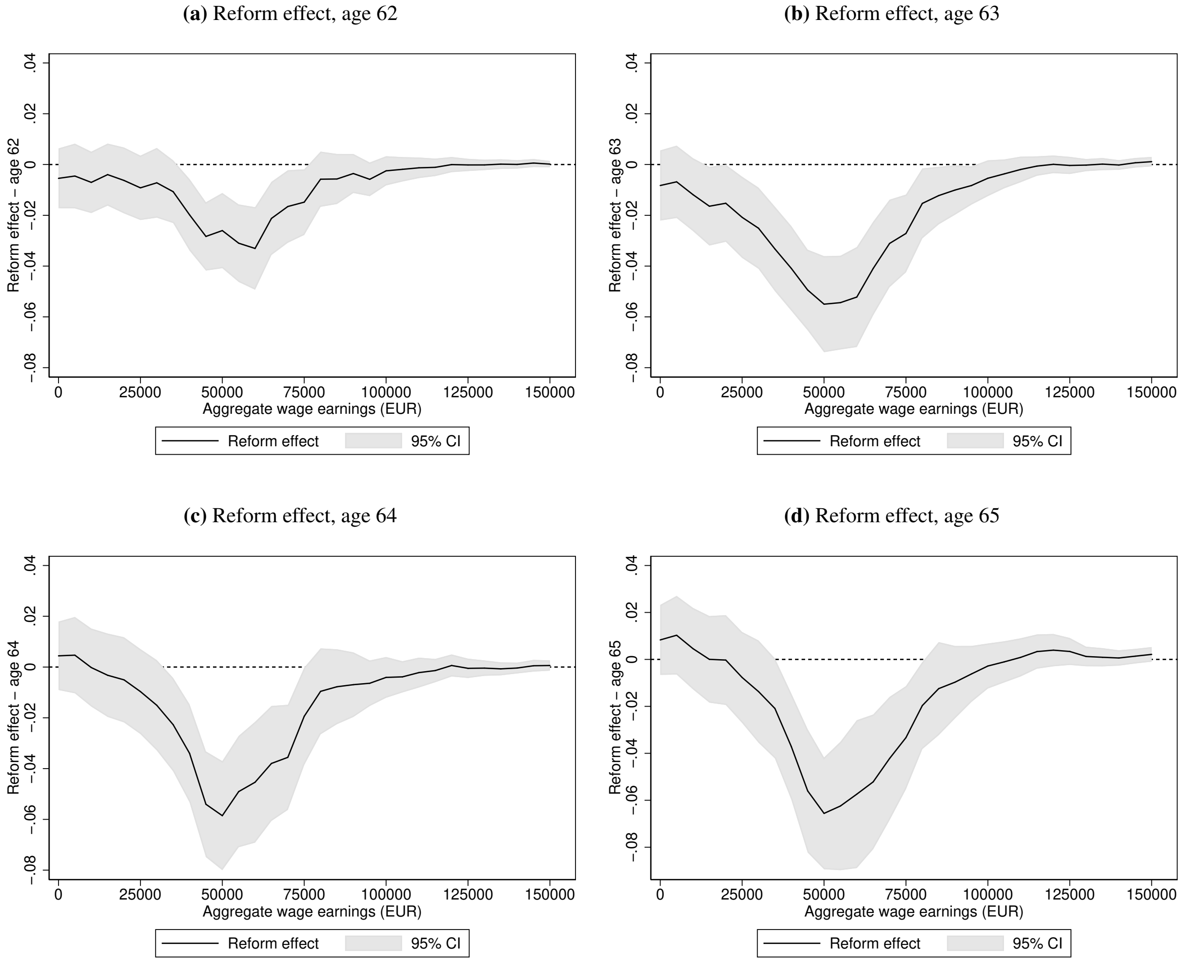
4.3 Aggregate earnings

In terms of the total effect of the reform on earnings over ages 62-65, which we think of as the effect of the reform on total labor supply, we calculate the sum of age-specific differences in the CCDF (denote this $\Delta CCDF_{s,a}$ = treatment CCDF minus control CCDF, evaluated at the average

16. Estimating a year-by-year linear specification similar to that in Equation (2) yields statistically insignificant reform effects for the pre-reform year 2010. These estimation results are reported in Appendix A3.1.

17. Including group-specific time trends for our sample period (2009-2014) directly into our main specification in Equation (3) instead yields qualitatively the same results. These results are reported in Appendix A2.5.

Figure 4: Reform effect on earnings distribution, by age



Source: Authors' own calculations using data from Statistics Norway.
Note: Simulation results from estimation of Equation (3), showing the difference in the CCDF for the treatment group and the control group. The gray-shaded area shows the 95 percent confidence intervals (based on 200 non-parametric bootstraps for each estimation, clustered on individual level). Aggregate earnings are expressed in EUR. Marginal effects and the associated standard errors are deferred to Appendix A2.4.

covariate values of the treatment group in the post-reform period) at each earnings increment multiplied by the step size of each of the 31 increments (EUR 5,000) as follows:

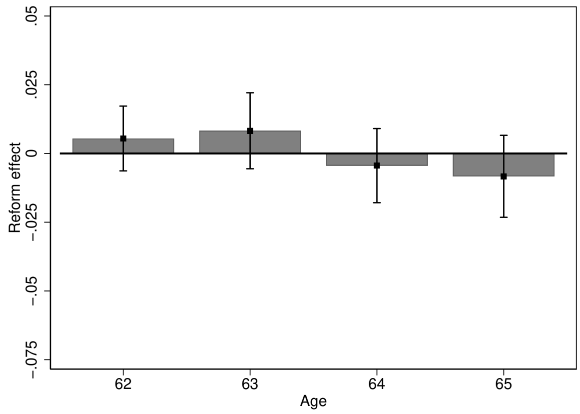
$$\Delta E = \sum_{a=62}^{65} \sum_{s=1}^{31} \Delta CCDF_{s,a} \times 5000 \tag{4}$$

From this exercise we find that the reform decreased the earnings over ages 62-65 by EUR 8,499, or equivalently 18.7 percent of pre-reform earnings of the same age group. The associated standard error of the change in earnings over ages 62-65 ($SE\,(\Delta E) =$ EUR 3,856) suggests that the negative effect on the total labor supply is statistically significant (t-statistic of 2.20, p-value of 0.03).[18]

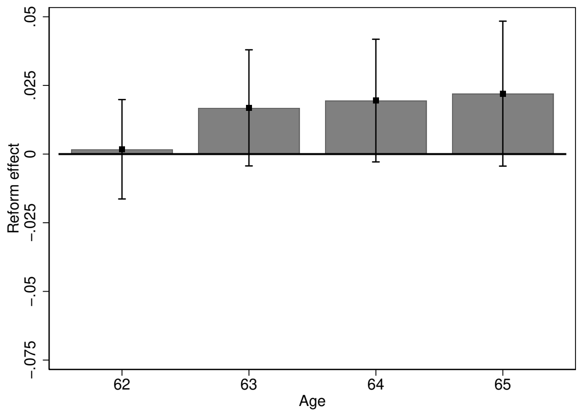
18. Given that we bootstrap each estimation with individuals as the unit of clustering, this should also alleviate the issue of the age-dependent estimates being correlated. This allows us to derive the standard error of the aggregate effect as the simple pooled standard error of the age-dependent effects.

Figure 5: Change in probability of being in selected earnings intervals, by age

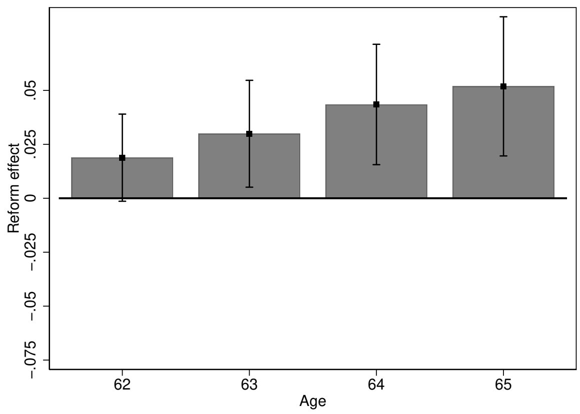
(a) Non-participation (EUR 0)



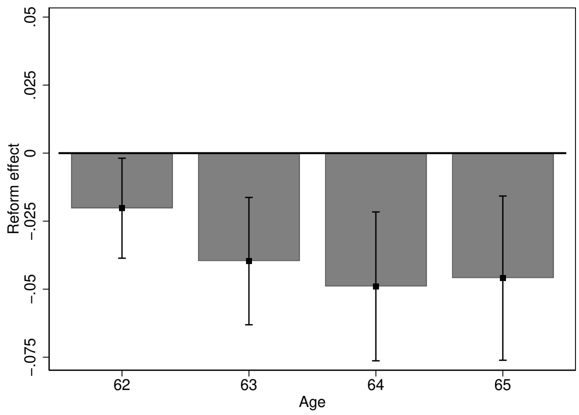
(b) EUR 0-30,000



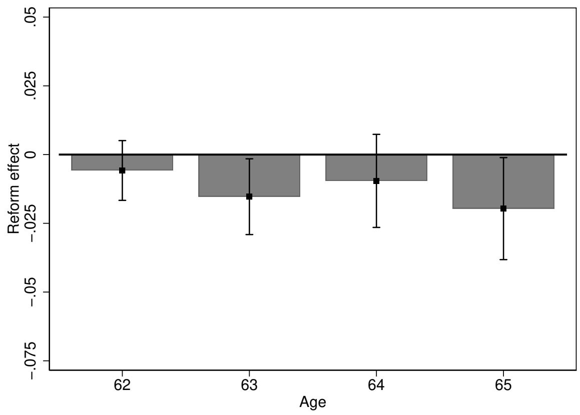
(c) EUR 30,000-50,000



(d) EUR 50,000-80,000



(e) > EUR 80,000

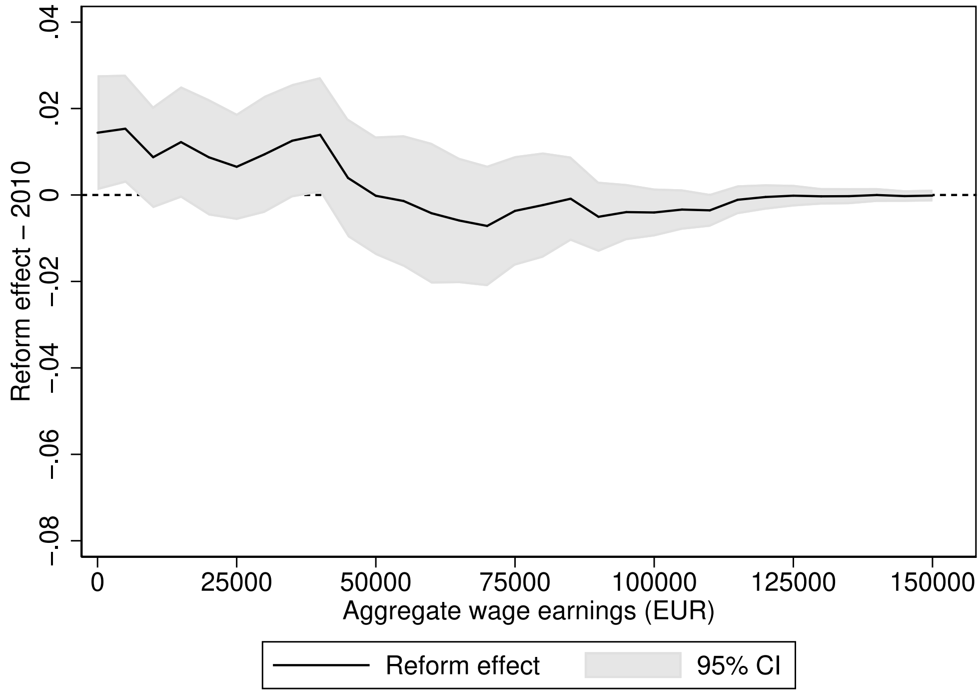


Source: Authors' own calculations using data from Statistics Norway.
Note: The reform effect on the probability of being in the different intervals, by age. The reform effects are generated using the marginal effects from Figure 4, with the caped lines showing the 95 percent confidence intervals. The procedure for deriving reform effects and the associated standard errors is described in detail in Appendix A2.3.

4.4 Earnings transitions

To investigate if the results are due to increased gradual retirement, we compare the cohort-specific transitions between different parts of the earnings distribution from age 59 to age 65

Figure 6: Reform effect, 2010



Source: Authors' own calculations using data from Statistics Norway.
Note: Simulation results from a year-by-year version of Equation (3), showing the difference in the CCDF for the treatment group and the control group in 2010. The gray-shaded area shows the 95 percent confidence intervals (based on 200 non-parametric bootstraps for each estimation, clustered on individual level). Aggregate earnings are expressed in EUR. Marginal effects and the associated standard errors are deferred to Appendix A2.1.

for the pre-reform birth cohort 1945 and the post-reform birth cohort 1949 based on the position in the earnings distribution at age 59. The transition rates, reported in Table 8, indicate that (i) the exit fractions are lower for all earnings intervals, (ii) earnings persistence increases and (iii) transitions to lower earnings are higher (but also transitions to higher earnings are somewhat higher) for the post-reform cohort than for the pre-reform cohort.[19]

5 Weekly working hours

5.1 Hours distribution

Weekly working hours is another measure of work activity we draw on, although we treat it as a supplementary measure to earnings. Since most of the changes in hours were in the tails of the distribution, we grouped workers into to four weekly-hours groups: (i) not working: 0 hours, (ii) short part-time: 0-20 hours, (iii) long part-time: 20-34 hours, and (iv) full-time/overtime: 34 hours or more. Most work full-time at age 60 (Figure 7, Panel (a)) and only a small fraction have left the labor force. At age 65 (Figure 7, Panel (b)) there is an increase in the

19. Note that we do not control for year effects in Table 8, which could explain the increase in the labor force participation rate.

Table 8: Transition rates within earnings distribution

		Position in earnings distribution at age 65					
		No earnings	EUR 0 - EUR 10K	EUR 10K - 25th pctl	25th-50th pctl	50th-75th pctl	75th pctl and above
Cohort 1945	EUR 10K - 25th pctl	43.9	6.3	17.8	22.6	6.9	2.5
	25th-50th pctl	40.1	3.7	11.9	23.7	17.5	3.1
	50th-75th pctl	38.3	4.7	9.1	9.7	25.5	12.7
	75th pctl and above	33.7	1.8	5.8	5.0	11.1	42.6
Cohort 1949	EUR 10K - 25th pctl	39.7	7.0	20.9	24.2	6.6	1.6
	25th-50th pctl	30.5	4.8	14.4	26.1	22.0	2.2
	50th-75th pctl	28.9	3.9	8.9	12.5	29.5	16.3
	75th pctl and above	26.4	2.8	5.9	5.8	10.6	48.5

Source: Authors' own calculations using data from Statistics Norway.
Note: Percentage in each interval of the earnings distribution at age 59 and age 65, for the pre-reform birth cohort 1945 and the post-reform birth cohort 1949 from the sample described in Table 3. The gray cells indicate the diagonal of the transition matrix.

fraction working short and long part-time (as well as full-time/over-time) and more people participating. This gives an indication of increased work after the reform.[20]

5.2 Reform effect on weekly hours

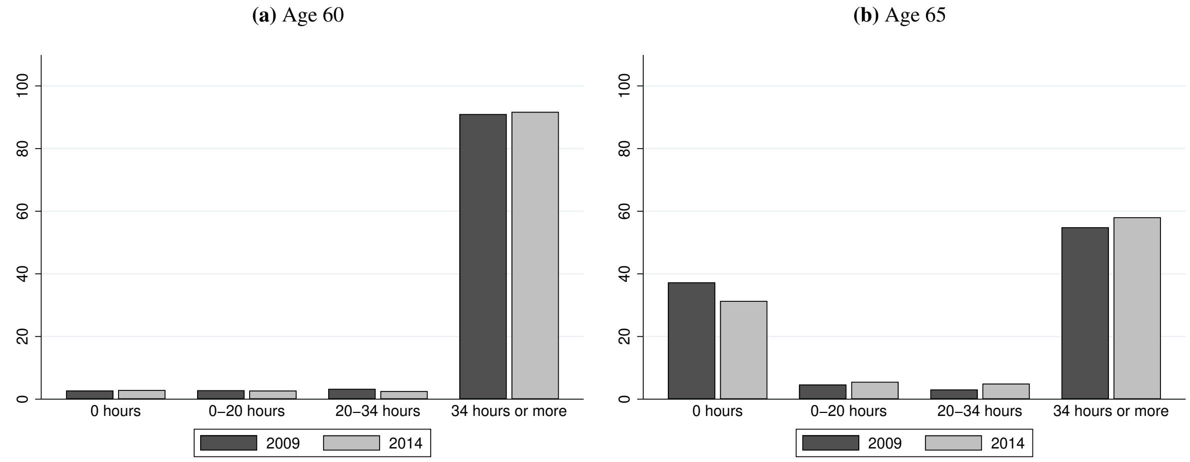
To derive the age-specific effects of the reform on the number of weekly working hours, and allow for covariates and dummies for age and year, we estimate the following non-linear difference-in-difference model for the alternatives $j \in \{$not working, short part-time, long part-time, full-time/over-time$\}$:

$$P\left(y_{i,a}=j\right)=\frac{\exp\left(\eta_{i,j}\right)}{\sum_{j=1}^{4}\exp\left(\eta_{i,j}\right)},\quad \eta_{i,j}=\alpha+X_{i,a}\beta+\sum_{a=61}^{65}\gamma_a D_a+\sum_{t=2010}^{2014}\lambda_t D_t+\sum_{a=62}^{65}\delta_a D_a \Delta_{i,a} \tag{5}$$

The variables in the linear index ($\eta_{i,j}$) in Equation (5) are defined as in Equation (3). The results from this exercise are illustrated in Figure 8. The figure shows the marginal effect of the treatment evaluated at the covariate values equal to the average of the treatment group in

20. Note that we do not control for year effects in Figure 7, which could explain the increase in the labor force participation rate.

Figure 7: Number of weekly working hours, age 60 and age 65



Source: Authors' own calculations using data from Statistics Norway.
Note: Weekly working hours in main job at age 60 (Panel (a)) and age 65 (Panel (b)) in 2009 and 2014, using the sample described in Table 3.

the post-reform period. The results indicate that more people work long part-time (Panel (c)), doing so by working less full-time/over-time (Panel (d)). The results are statistically significant and generally increasing over the age range which we can follow (ages 62-65), which suggests a substantial state dependence in labor supply.[21]

To be consistent, the earnings analysis and the hours analysis should give the same effect on the labor supply, and they seem to do so. The earnings analysis gives more people below EUR 50,000 and fewer between EUR 50,000 and EUR 80,000, while the hours analyses gives an increase in long part-time work and a decrease in full-time work.

5.3 Aggregate hours

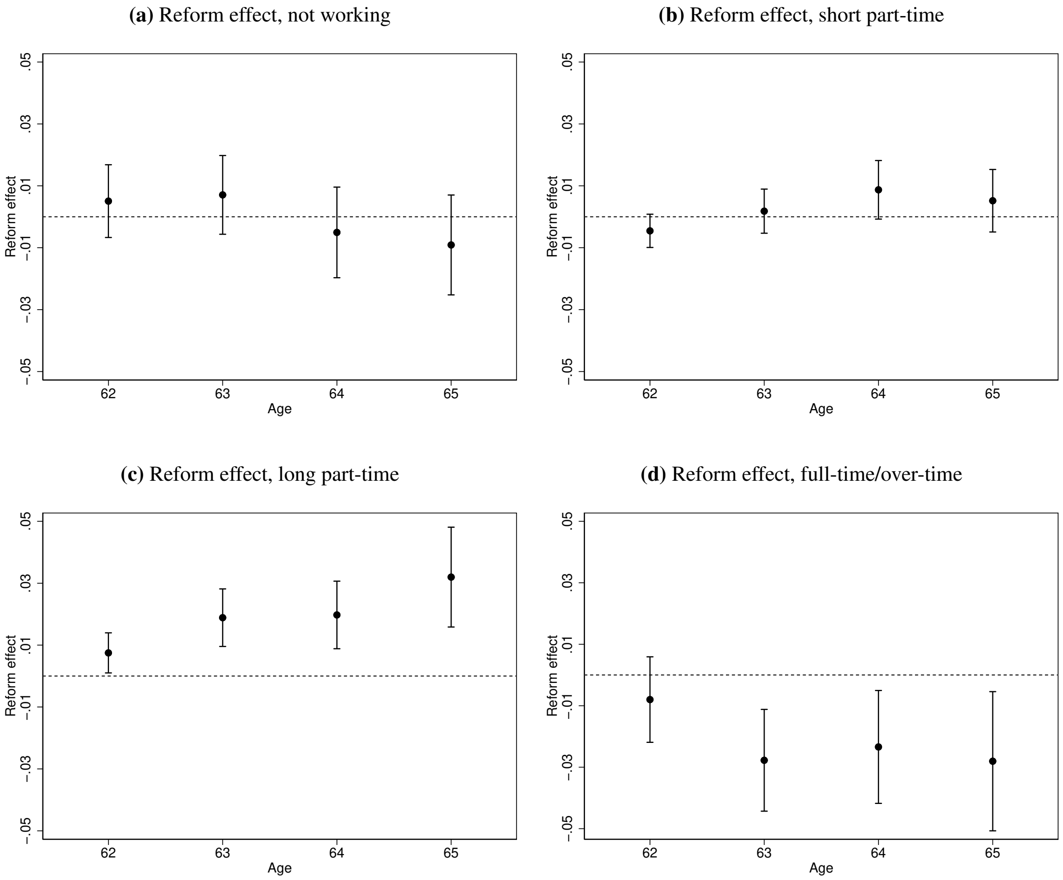
In terms of the total effect of the reform on weekly working hours over ages 62-65, we calculate the sum of age-specific marginal effects (denote this $\Delta P\left(y_a = j\right)$) of being in different weekly-hours groups (evaluated at the average covariate values of the treatment group in the post-reform period) for each of the four weekly-hours groups $j \in \{$not working, short part-time, long part-time, full-time/over-time$\}$ as follows:

$$\Delta H_j = \sum_{a=62}^{65} \Delta P\left(y_a = j\right) \tag{6}$$

From this exercise we find that the reform increased the fraction working long part-time over ages 62-65 by 7.8 percentage points and decreased the fraction working full-time/over-time over ages 62-65 by 8.7 percentage points. The associated standard error of the probabilities

21. Estimation results for a similar model to Equation (5) with year-specific reform effects are deferred to Appendix A4.1.

Figure 8: Reform effect on weekly hours, by age



Source: Authors' own calculations using data from Statistics Norway.
Note: Results from estimation of Equation (5), showing the estimated marginal reform effects by age and the associated 95 percent confidence intervals (caped lines, based on 200 non-parametric bootstraps for each estimation, clustered on individual level). The weekly-hours groups are defined as follows: (i) not working: 0 hours, (ii) short part-time: 0-20 hours, (iii) long part-time: 20-34 hours, and (iv) full-time/over-time: 34 hours or more. Marginal effects and the associated standard errors are deferred to Appendix A4.2.

over ages 62-65 ($SE\left(\Delta H_{\text{long part-time}}\right) = 2.8$ percentage points and $SE\left(\Delta H_{\text{full-time/over-time}}\right) = 3.6$ percentage points) suggests that the effects are also statistically significant (t-statistic of 3.65 and p-value of < 0.01 for long part-time and t-statistic of 2.44 and p-value of 0.01 for full-time/over-time).[22,23]

5.4 Hours transitions

Table 9 shows indicative evidence of cohort-specific transitions between weekly working hours at age 59 to age 65. Here, we compare the pre-reform birth cohort 1945 to the post-reform birth cohort 1949 and show the transition matrix between the four weekly hours-groups. Clearly, moves out the labor force have become less frequent after the reform, while more people stay in the same weekly-hours group or reduce their weekly hours. This lends support to the estimation

22. Given that we bootstrap each estimation with individuals as the unit of clustering, this should also alleviate the issue of the age-dependent estimates being correlated, see Footnote 18.

23. The estimated effects for the two alternatives $\{\text{not working}, \text{short part-time}\}$ are statistically insignificant at conventional levels, and are therefore not reported here.

Table 9: Transition between weekly hours-groups, by birth cohort

		Hours at age 65			
		Short part-time	Long part-time	Full-time/ over-time	Not working
Cohort 1945	Short part-time	23.5	6.0	24.0	46.5
	Long part-time	6.8	27.4	14.4	51.4
	Full-time/over-time	3.1	2.4	56.2	38.3
Cohort 1949	Short part-time	31.7	7.9	20.1	40.3
	Long part-time	9.0	34.7	13.2	43.1
	Full-time/over-time	4.7	3.9	60.7	30.7

Source: Authors' own calculations using data from Statistics Norway.
Note: Percentage of individuals in each weekly hours-group at age 59 transiting to weekly hours-group at age 65, for the pre-reform birth cohort 1945 and the post-reform birth cohort 1949 from the sample described in Table 3. The weekly-hours groups are defined as follows: (i) not working: 0 hours, (ii) short part-time: 0-20 hours, (iii) long part-time: 20-34 hours, and (iv) full-time/over-time: 34 hours or more. The gray cells indicate the diagonal of the transition matrix.

results of the reform effect on weekly working hours illustrated in Figure 8, as well as the notion that the reform led to more gradual retirement.[24]

6 Job changes

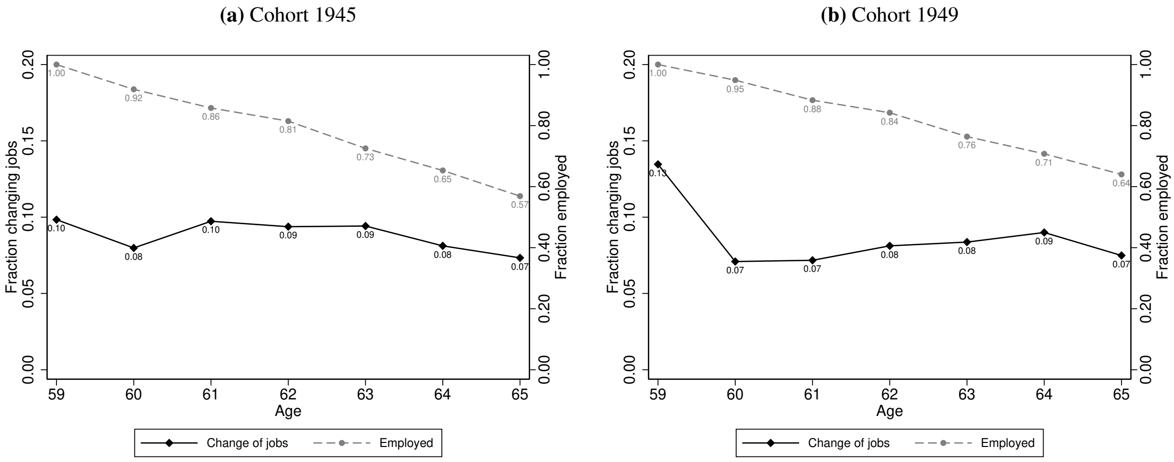
To look at what potential mechanisms might be behind the increased mass in the lower end of the earnings distribution beyond that of reduced weekly working hours, we also looked at job transitions. Workers may systematically change jobs at the end of the working career as a part of partial retirement, by switching from the career job to another less demanding and lower paid job and thereby reducing their earnings (Kantarci 2013). Here we follow the extent of job changes from age 59 and until age 65. The fraction of the pre-reform birth cohort 1945 and the post-reform birth cohort 1949 that change jobs and the fraction working during ages 59-65 are shown in Panel (a) and Panel (b) in Figure 9, respectively. Clearly, there are few job changes taking place at these ages (ranging between 7-13 percent each year) and, more importantly, very little difference between the pre- and post-reform birth cohorts.[25] Thus, it seems that the reduction in earnings and weekly working hours we find are not driven by workers switching employers at the end of their working life (partial retirement) but rather by reducing earnings and working hours at their career employer (phased retirement).[26]

24. Note that we do not control for year effects in Table 9, which could explain the increase in the labor force participation rate.

25. Firm is defined on the establishment level, and we are not able to identify job changes within the firm.

26. Estimating a similar model to Equation (1) with an indicator for job change as the outcome variable on our sample in Table 3 yields a statistically insignificant reform effect (results not reported), confirming the job change patterns documented in Figure 9.

Figure 9: Job changes, by birth cohort and age



Source: Authors' own calculations using data from Statistics Norway.
Note: Percent of pre-reform birth cohort 1945 (Panel (a)) and post-reform cohort 1949 (Panel (b)) from the sample described in Table 3 that are employed (gray line, right-hand axis) and changing jobs (black line, left-hand axis), by age. See Appendix A5 for the definition of a job change.

7 Conclusion

We study the impact of reforming a pension by reducing the access age while retaining the expected present value of the benefit stream constant, regardless of claiming age. One component of the comprehensive Norwegian pension reform of 2011 did exactly this, reducing a long-standing access age of 67 to age 62. The impact of this change is studied by identifying a group that was exposed only to the new option, without any changes in economic incentives. After taking into account the strong upwards trend in employment of the elderly population, we find no identifiable net effect of the reform on labor force participation. However, aggregate earnings fell by 18.7 percent and the earnings distribution shifted downwards. Analyses of transitions over age, both between earnings and between working hours' groups, show this to be caused mainly by high earners reducing their earnings through reduced hours, with an increase in part-time work of 7.8 percent and a decrease in full-time work of 8.7 percent.

While we have focused on the supply side of the labor market, there is good reason to believe there are restrictions on the demand side in terms of accommodating for gradual retirement (Midtsundstad 2018; Clark et al. 2019). This would in turn imply that our results constitute a lower bound on the effect of introducing a flexible pension on gradual retirement.

Stated preference analysis indicates that workers prefer gradual retirement with decreasing labor supply over several years before entering full retirement over abrupt full retirement (Kantarci and Soest 2017), a finding backed up by a host of surveys (OECD 2017). While we cannot identify the reasons why some individuals undertake gradual retirement, surveys conducted in Finland (Takala and Väänänen 2016) suggest that older workers opt for gradual retirement to (i) devote more time to hobbies and family and (ii) saying that they had been working full-time for too long. Thus there seems to be a latent desire for phased retirement. Our results indicated that a reduced access age, holding the overall value of the pension constant, can facilitate such a transition to full retirement.

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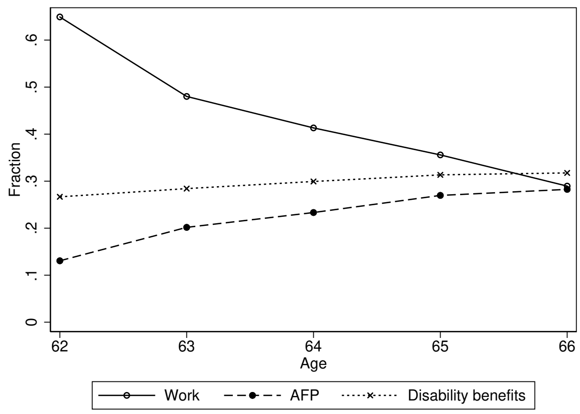
A1 Gradual retirement before the reform

To illustrate the extent of gradual retirement before the 2011 Norwegian pension reform, we consider the (pre-reform) 1942 birth cohort. Before the reform, disability program enrollment and early retirement (AFP) for those eligible were the only exit routes before age 67, and these exit routes are illustrated in Figure A1. The drop in labor force participation closely mirrors the increase in disability benefit receipt and AFP claiming. AFP was the only option that combined reduced earnings with claiming part of the pension, and that carried a proportional earnings test. As a result of the strict earnings test, among those claiming the AFP benefits about 85 percent claimed the full pension and quit work.

Figure A1: Gradual retirement, 1942 birth cohort

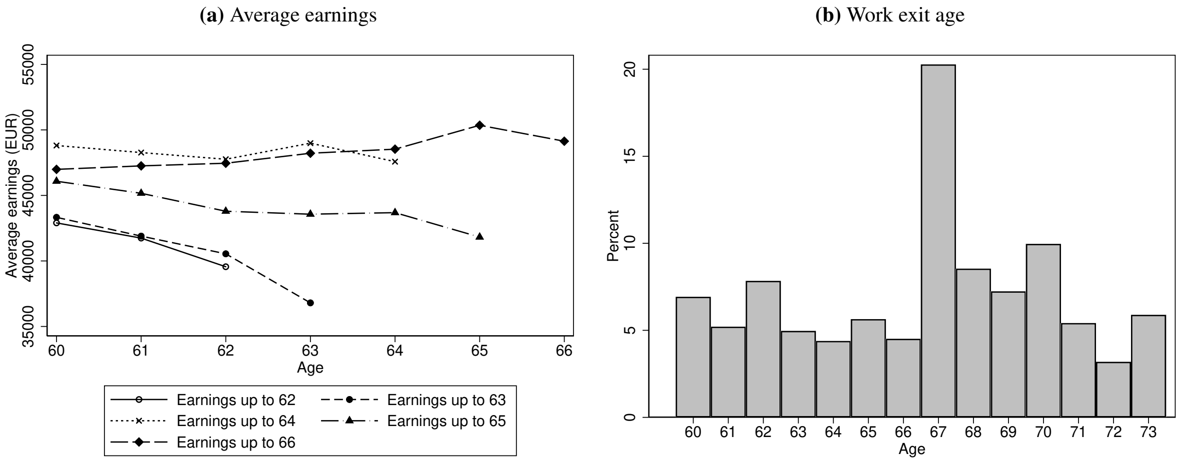


Source: Authors' own calculations using data from Statistics Norway.
Note: Fraction of 1942 birth cohort in the labor force (hollow circles), claiming AFP benefits (solid circles) and receiving permanent disability benefits (crosses), by age. Work is defined as earnings over EUR 10,000, while AFP claiming and disability benefit claiming is defined as any positive amount claimed.

Earnings among those who continued to work either stayed fairly constant or fell very modestly up to the year before retirement, as illustrated in Panel (a) in Figure A2. To focus on gradual retirement, we have include only those who at age 66 neither received permanent disability benefits nor claimed AFP benefits. There is a somewhat larger drop among those who left the labor market early, with earnings the last year before the transition year dropping up to 20 percent. This was also a group with lower earnings at age 60. For the birth cohort as a whole, almost one quarter leave work at age 67 (have the second-to-last earnings year at age 66), but apart from that, exit is spread out over a range of ages (illustrated in Panel (b) in Figure A2).

We conclude that the reduction in the labor force participation rate and earnings before the reform among those working at age 60 was almost exclusively related to claiming AFP benefits. Work reduction played a minor role, as only about 3 percent of those working at age 60 took a part-time AFP. That carried a strict earnings test and required combining part pension benefits and part earnings. Apart from that, about a quarter went on disability benefits and the rest continued at the earnings level they had at age 60 until they quit.

Figure A2: Gradual retirement, 1942 birth cohort



Source: Authors' own calculations using data from Statistics Norway.
Note: Average earnings (expressed in EUR, conditional on positive earnings) of 1942 birth cohort (up to and including the year prior to retirement) for retirement ages 63-67, by age and age of retirement (Panel (a)) and distribution of work exit age for 1942 birth cohort, where work exit age is defined as the last age with positive earnings (Panel (b)). The sample is conditional on not receiving permanent disability benefits or claiming AFP benefits at age 66.

A2 CCDF

A2.1 Year-by-year effect

We assume that for each earnings level $y_{i,t}$ for individual i in year $t = 2009, \ldots, 2014$:

$$P\left(y_{i,t} > y_k\right) = F\left(\alpha + X_{i,t}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \sum_{t=2010}^{2014} \eta_t D_t \Delta_{i,t}\right) \tag{A1}$$

Here, the coefficients λ_t for $t = 2010, \ldots, 2014$ measure the year effects, while η_t for $t = 2010, \ldots, 2014$ measure the year-specific treatment effects. Figure A3 shows the results of simulations of the reform impact for each year separately.

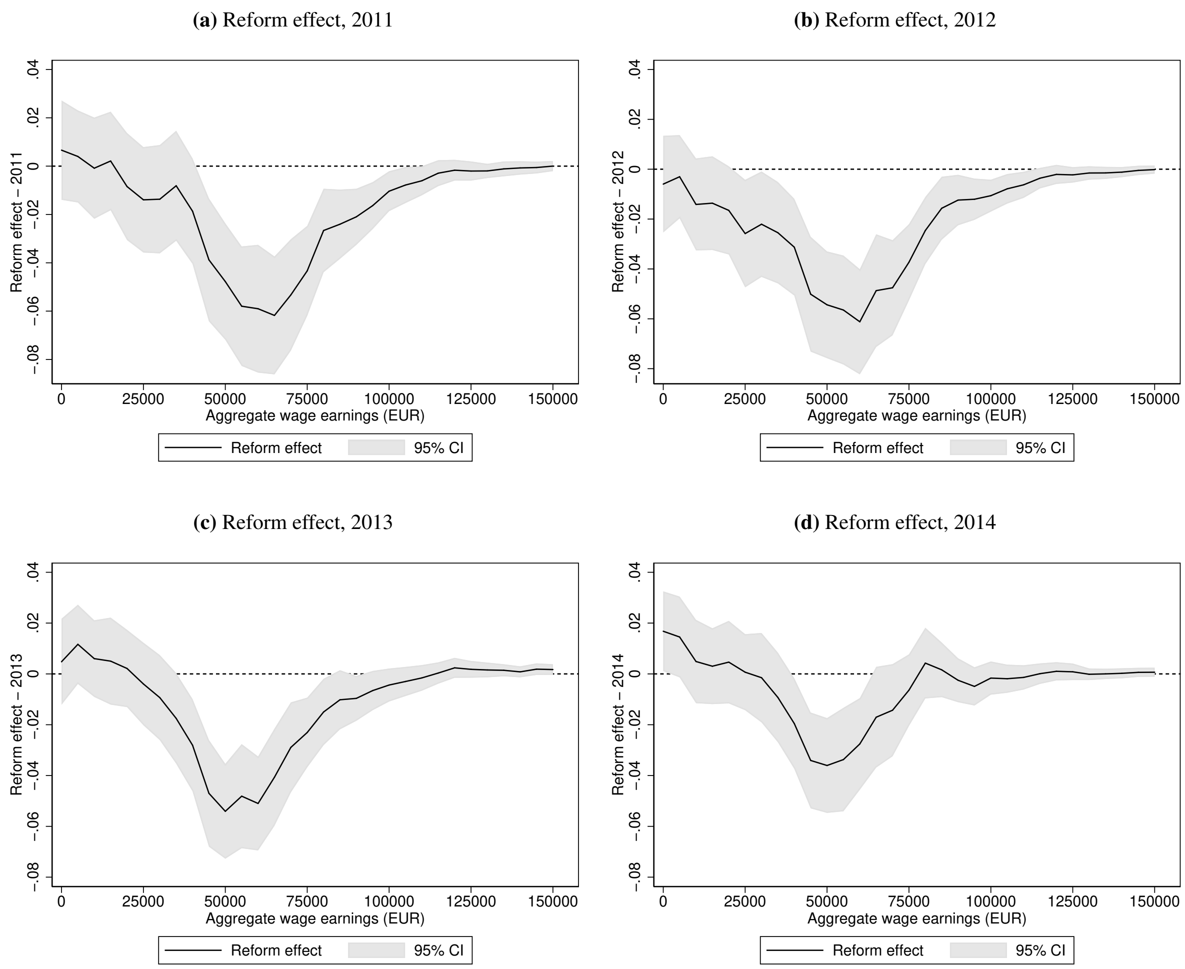
Table A1 in Section A2.6 shows the marginal effects (ME) and the associated standard errors (SE) for each of the estimations of Equation (A1) used to simulate the year-by-year reform effects on the earnings distribution (shown in Figure 6 and Figure A3).

A2.2 Average effect

Panel A in Table A3 in Section A2.6 shows the marginal effects and the associated standard errors for each of the estimations of the following empirical approach (shown in Figure 3):

$$P\left(y_{i,a/t} > y_k\right) = F\left(\alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \eta \Delta_{i,a}\right) \tag{A2}$$

Figure A3: Reform effect on earnings distribution, by year



Source: Authors' own calculations using data from Statistics Norway.
Note: Simulation results from estimation of Equation (A1), showing the difference in the CCDF for the treatment group and the control group. The gray-shaded area shows the 95 percent confidence intervals (based on 200 non-parametric bootstraps for each estimation, clustered on individual level). Aggregate earnings are expressed in EUR. Marginal effects and the associated standard errors are shown in Table A1.

A2.3 Probability of being in different earnings intervals

The change in the probabilities of being in different earnings intervals (shown in Table 7) are derived from the marginal effects reported in Panel A in Table A3 in Section A2.6. Letting $T \in \{0,1\}$ denote treatment status, $F(\cdot)$ denote the CDF and $\Delta CCDF_k$ denote the marginal effect at the threshold $y = y_k$, the change in the probability of being in an earnings interval $\left[\underline{y}, \bar{y}\right]$ can be expressed as follows using the definition of the CDF and the CCDF:

$$
\begin{aligned}
P\left(\underline{y} < y < \bar{y} | T = 1\right) - P\left(\underline{y} < y < \bar{y} | T = 0\right) &= \left[F\left(\bar{y}|T=1\right) - F\left(\underline{y}|T=1\right)\right] \\
&- \left[F\left(\bar{y}|T=0\right) - F\left(\underline{y}|T=0\right)\right] \\
&= \left[\left[1 - P\left(y > \bar{y}|T=1\right)\right] - \left[1 - P\left(y > \underline{y}|T=1\right)\right]\right] \\
&- \left[\left[1 - P\left(y > \bar{y}|T=0\right)\right] - \left[1 - P\left(y > \underline{y}|T=0\right)\right]\right] \\
&= P\left(y > \underline{y}|T=1\right) - P\left(y > \underline{y}|T=0\right) \\
&- \left[P\left(y > \bar{y}|T=1\right) - P\left(y > \bar{y}|T=0\right)\right] \\
&= \Delta CCDF_{\underline{y}} - \Delta CCDF_{\bar{y}}
\end{aligned} \tag{A3}
$$

Based on the formula in Equation (A3), we find the following changes in probabilities:

$$
\begin{aligned}
P(y=0|T=1)-P(y=0|T=0) &= -\Delta CCDF_0 \\
&= 0.001 \\
P(0<y<30,000|T=1)-P(0<y<30,000|T=0) &= \Delta CCDF_0-\Delta CCDF_{30,000} \\
&= (-0.001)-(-0.015) \\
&= 0.014 \\
P(30,000<y<50,000|T=1)-P(30,000<y<50,000|T=0) &= \Delta CCDF_{30,000}-\Delta CCDF_{50,000} \\
&= (-0.015)-(-0.047) \\
&= 0.032 \\
P(50,000<y<80,000|T=1)-P(50,000<y<80,000|T=0) &= \Delta CCDF_{50,000}-\Delta CCDF_{80,000} \\
&= (-0.047)-(-0.011) \\
&= -0.036 \\
P(y>80,000|T=1)-P(y>80,000|T=0) &= \Delta CCDF_{80,000} \\
&= -0.011
\end{aligned}
\tag{A4}
$$

The standard errors are derived using the conventional pooled variance of the estimated marginal effects, where we assume independence of the estimates:

$$
SE(\Delta CCDF_s-\Delta CCDF_j)=\sqrt{[SE(\Delta CCDF_s)]^2+[SE(\Delta CCDF_j)]^2} \tag{A5}
$$

Using the general formula in Equation (A5), we derive the following standard errors:

$$
\begin{aligned}
SE(\Delta CCDF) &= 0.007 \\
SE(\Delta CCDF_0-\Delta CCDF_{30,000}) &= \sqrt{[0.007]^2+[0.007]^2}=0.010 \\
SE(\Delta CCDF_{30,000}-\Delta CCDF_{50,000}) &= \sqrt{[0.007]^2+[0.008]^2}=0.011 \\
SE(\Delta CCDF_{50,000}-\Delta CCDF_{80,000}) &= \sqrt{[0.008]^2+[0.006]^2}=0.010 \\
SE(\Delta CCDF_{80,000}) &= 0.006
\end{aligned}
\tag{A6}
$$

The same method is used for the probabilities and the associated standard errors for Figure 5.

A2.4 Age-by-age effect

Table A2 in Section A2.6 shows the marginal effects and the associated standard errors for each of the estimations of Equation (3) used to simulate the age-by-age reform effects on the earnings distribution (shown in Figure 4), where we assume a constant treatment effect across years.

A2.5 Robustness

In order to include a pre-reform trend in our main specification, we first estimate treatment-specific trends in each of the earnings intervals using data covering the pre-reform period (2006-

2010) with the following linear model:[1]

$$P\left(y_{i,t} > y_k\right) = \alpha + \tau t + \gamma \mathbf{1}\left\{a_{i,t} \geq 62\right\} + \omega\left(t \times \mathbf{1}\left\{a_{i,t} \geq 62\right\}\right) + \varepsilon_{i,t} \tag{A7}$$

With the saturated model in Equation (A7), we obtain an estimated slope of the time trend for the treatment group (individuals aged 62-65) relative to the control group (individuals aged 60-61), denoted $\hat{\omega}$, which we then include into our main specification as follows:

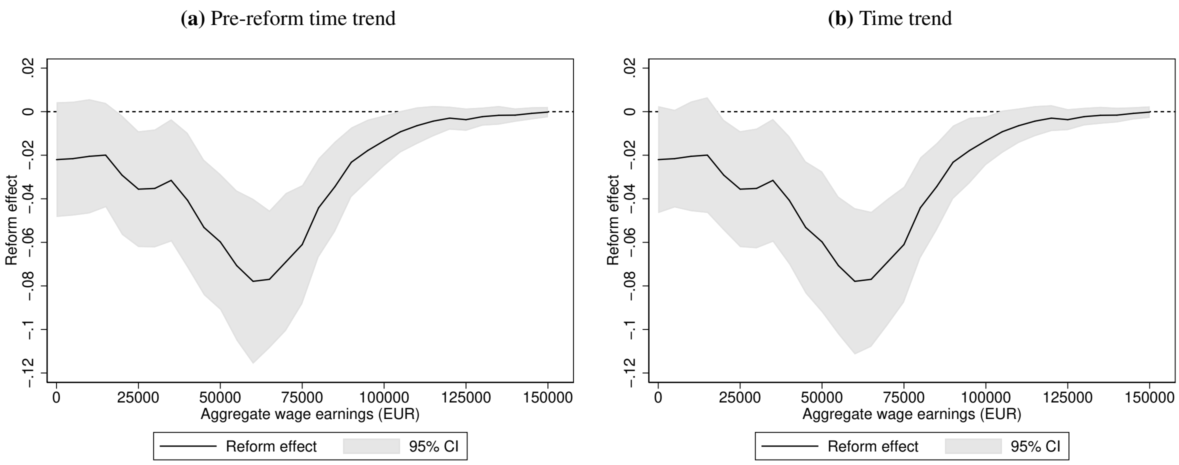
$$P\left(y_{i,a/t} > y_k\right) = F\left(\alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \varphi \hat{\omega}_g\left(t \times \mathbf{1}\left\{a_{i,a} \geq 62\right\}\right) + \eta \Delta_{i,a}\right) \tag{A8}$$

To instead include a linear time trend estimated using the sample years we use otherwise in the analysis (2009-2014), we estimate the following version of our main specification:

$$P\left(y_{i,a/t} > y_k\right) = F\left(\alpha + X_{i,a}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \varphi\left(t \times \mathbf{1}\left\{a_{i,a} \geq 62\right\}\right) + \eta \Delta_{i,a}\right) \tag{A9}$$

The marginal effects and associated standard errors from estimating Equation (A8) and Equation (A9) are shown in Panel (a) and Panel (b) in Figure A4, and reported in Panel B and Panel C in Table A3 in Section A2.6.

Figure A4: Reform effect on earnings distribution, time trend



Source: Authors' own calculations using data from Statistics Norway.
Note: Simulation results from estimation of Equation (A8) (Panel (a)) and Equation (A9) (Panel (b)), showing the difference in the CCDF for the treatment group and the control group. The gray-shaded area shows the 95 percent confidence intervals (based on 200 non-parametric bootstraps for each estimation, clustered on individual level). Aggregate earnings are expressed in EUR. Marginal effects and the associated standard errors are reported in Table A3.

1. Note that the data set used in this exercise does not include the birth cohorts 1941 and 1942, since our (current) main data set starts in 2002 and the sample restriction criteria are applied at age 59.

A2.6 Tables

Table A1: Marginal effects, CCDF, reform effect by year

Outcome	2010		2011		2012		2013		2014	
	ME	SE	ME	SE	ME	SE	ME	SE	ME	SE
$P(y_{i,t} > 0)$	0.014	(0.007)	0.007	(0.010)	-0.006	(0.010)	0.005	(0.009)	0.017	(0.008)
$P(y_{i,t} > 5,000)$	0.015	(0.006)	0.004	(0.010)	-0.003	(0.009)	0.012	(0.008)	0.015	(0.008)
$P(y_{i,t} > 10,000)$	0.009	(0.006)	-0.001	(0.011)	-0.014	(0.009)	0.006	(0.008)	0.005	(0.008)
$P(y_{i,t} > 15,000)$	0.012	(0.007)	0.002	(0.010)	-0.014	(0.010)	0.005	(0.009)	0.003	(0.008)
$P(y_{i,t} > 20,000)$	0.009	(0.007)	-0.008	(0.011)	-0.017	(0.009)	0.002	(0.008)	0.005	(0.008)
$P(y_{i,t} > 25,000)$	0.007	(0.006)	-0.014	(0.011)	-0.026	(0.011)	-0.004	(0.008)	0.001	(0.008)
$P(y_{i,t} > 30,000)$	0.009	(0.007)	-0.014	(0.011)	-0.022	(0.011)	-0.009	(0.009)	-0.001	(0.009)
$P(y_{i,t} > 35,000)$	0.013	(0.007)	-0.008	(0.012)	-0.025	(0.010)	-0.018	(0.009)	-0.009	(0.009)
$P(y_{i,t} > 40,000)$	0.014	(0.007)	-0.019	(0.011)	-0.031	(0.010)	-0.028	(0.009)	-0.020	(0.009)
$P(y_{i,t} > 45,000)$	0.004	(0.007)	-0.039	(0.013)	-0.050	(0.012)	-0.047	(0.011)	-0.034	(0.010)
$P(y_{i,t} > 50,000)$	0.000	(0.007)	-0.048	(0.012)	-0.054	(0.011)	-0.054	(0.010)	-0.036	(0.010)
$P(y_{i,t} > 55,000)$	-0.001	(0.008)	-0.058	(0.013)	-0.056	(0.011)	-0.048	(0.010)	-0.034	(0.010)
$P(y_{i,t} > 60,000)$	-0.004	(0.008)	-0.059	(0.013)	-0.061	(0.011)	-0.051	(0.009)	-0.028	(0.009)
$P(y_{i,t} > 65,000)$	-0.006	(0.007)	-0.062	(0.012)	-0.049	(0.012)	-0.041	(0.010)	-0.017	(0.010)
$P(y_{i,t} > 70,000)$	-0.007	(0.007)	-0.053	(0.012)	-0.048	(0.010)	-0.029	(0.009)	-0.014	(0.009)
$P(y_{i,t} > 75,000)$	-0.004	(0.006)	-0.043	(0.009)	-0.037	(0.008)	-0.023	(0.007)	-0.006	(0.007)
$P(y_{i,t} > 80,000)$	-0.002	(0.006)	-0.027	(0.009)	-0.025	(0.007)	-0.015	(0.007)	0.004	(0.007)
$P(y_{i,t} > 85,000)$	-0.001	(0.005)	-0.024	(0.007)	-0.016	(0.006)	-0.010	(0.006)	0.002	(0.005)
$P(y_{i,t} > 90,000)$	-0.005	(0.004)	-0.021	(0.006)	-0.012	(0.005)	-0.010	(0.005)	-0.002	(0.004)
$P(y_{i,t} > 95,000)$	-0.004	(0.003)	-0.016	(0.005)	-0.012	(0.004)	-0.007	(0.004)	-0.005	(0.004)
$P(y_{i,t} > 100,000)$	-0.004	(0.003)	-0.010	(0.004)	-0.011	(0.003)	-0.004	(0.003)	-0.002	(0.003)
$P(y_{i,t} > 105,000)$	-0.003	(0.002)	-0.008	(0.004)	-0.008	(0.003)	-0.003	(0.003)	-0.002	(0.003)
$P(y_{i,t} > 110,000)$	-0.004	(0.002)	-0.006	(0.003)	-0.006	(0.003)	-0.002	(0.003)	-0.001	(0.002)
$P(y_{i,t} > 115,000)$	-0.001	(0.002)	-0.003	(0.003)	-0.004	(0.002)	0.000	(0.002)	0.000	(0.002)
$P(y_{i,t} > 120,000)$	0.000	(0.002)	-0.002	(0.002)	-0.002	(0.002)	0.002	(0.002)	0.001	(0.002)
$P(y_{i,t} > 125,000)$	0.000	(0.001)	-0.002	(0.002)	-0.002	(0.002)	0.002	(0.002)	0.001	(0.002)
$P(y_{i,t} > 130,000)$	0.000	(0.001)	-0.002	(0.001)	-0.002	(0.001)	0.002	(0.001)	0.000	(0.001)
$P(y_{i,t} > 135,000)$	0.000	(0.001)	-0.001	(0.002)	-0.001	(0.001)	0.001	(0.001)	0.000	(0.001)
$P(y_{i,t} > 140,000)$	0.000	(0.001)	-0.001	(0.001)	-0.001	(0.001)	0.001	(0.001)	0.000	(0.001)
$P(y_{i,t} > 145,000)$	0.000	(0.001)	-0.001	(0.001)	0.000	(0.001)	0.002	(0.001)	0.001	(0.001)
$P(y_{i,t} > 150,000)$	0.000	(0.001)	0.000	(0.001)	0.000	(0.001)	0.002	(0.001)	0.001	(0.001)

Source: Authors' own calculations using data from Statistics Norway.
Note: Marginal effects (ME) evaluated at the covariate values equal to the average of the treatment group in the post-reform period and associated standard errors (SE) for each of the estimations of Equation (A1). Standard errors are based on 200 non-parametric bootstraps for each estimation, clustered on individual level.

Table A2: Marginal effects, CCDF, reform effect by age

Outcome	Age 62		Age 63		Age 64		Age 65	
	ME	SE	ME	SE	ME	SE	ME	SE
$P(y_{i,a} > 0)$	-0.005	(0.006)	-0.008	(0.007)	0.004	(0.007)	0.008	(0.008)
$P(y_{i,a} > 5,000)$	-0.005	(0.007)	-0.007	(0.007)	0.005	(0.008)	0.010	(0.009)
$P(y_{i,a} > 10,000)$	-0.007	(0.006)	-0.012	(0.007)	0.000	(0.008)	0.005	(0.009)
$P(y_{i,a} > 15,000)$	-0.004	(0.006)	-0.016	(0.008)	-0.003	(0.008)	0.000	(0.009)
$P(y_{i,a} > 20,000)$	-0.006	(0.007)	-0.015	(0.008)	-0.005	(0.009)	0.000	(0.010)
$P(y_{i,a} > 25,000)$	-0.009	(0.006)	-0.021	(0.008)	-0.010	(0.009)	-0.008	(0.010)
$P(y_{i,a} > 30,000)$	-0.007	(0.007)	-0.025	(0.008)	-0.015	(0.009)	-0.014	(0.011)
$P(y_{i,a} > 35,000)$	-0.011	(0.006)	-0.033	(0.008)	-0.023	(0.009)	-0.021	(0.011)
$P(y_{i,a} > 40,000)$	-0.020	(0.007)	-0.041	(0.009)	-0.034	(0.010)	-0.037	(0.012)
$P(y_{i,a} > 45,000)$	-0.028	(0.007)	-0.049	(0.008)	-0.054	(0.011)	-0.056	(0.013)
$P(y_{i,a} > 50,000)$	-0.026	(0.008)	-0.055	(0.010)	-0.059	(0.011)	-0.066	(0.012)
$P(y_{i,a} > 55,000)$	-0.031	(0.008)	-0.054	(0.009)	-0.049	(0.011)	-0.062	(0.014)
$P(y_{i,a} > 60,000)$	-0.033	(0.008)	-0.052	(0.010)	-0.045	(0.012)	-0.057	(0.016)
$P(y_{i,a} > 65,000)$	-0.021	(0.007)	-0.041	(0.009)	-0.038	(0.012)	-0.052	(0.015)
$P(y_{i,a} > 70,000)$	-0.017	(0.007)	-0.031	(0.009)	-0.036	(0.011)	-0.042	(0.013)
$P(y_{i,a} > 75,000)$	-0.015	(0.007)	-0.027	(0.008)	-0.019	(0.010)	-0.033	(0.011)
$P(y_{i,a} > 80,000)$	-0.006	(0.006)	-0.015	(0.007)	-0.010	(0.009)	-0.020	(0.009)
$P(y_{i,a} > 85,000)$	-0.006	(0.005)	-0.012	(0.006)	-0.008	(0.008)	-0.012	(0.010)
$P(y_{i,a} > 90,000)$	-0.004	(0.004)	-0.010	(0.005)	-0.007	(0.006)	-0.010	(0.008)
$P(y_{i,a} > 95,000)$	-0.006	(0.003)	-0.008	(0.004)	-0.006	(0.005)	-0.006	(0.006)
$P(y_{i,a} > 100,000)$	-0.002	(0.003)	-0.005	(0.004)	-0.004	(0.004)	-0.003	(0.005)
$P(y_{i,a} > 105,000)$	-0.002	(0.002)	-0.004	(0.003)	-0.004	(0.003)	-0.001	(0.004)
$P(y_{i,a} > 110,000)$	-0.001	(0.002)	-0.002	(0.003)	-0.002	(0.003)	0.001	(0.004)
$P(y_{i,a} > 115,000)$	-0.001	(0.002)	-0.001	(0.002)	-0.001	(0.002)	0.003	(0.004)
$P(y_{i,a} > 120,000)$	0.000	(0.002)	0.000	(0.002)	0.001	(0.002)	0.004	(0.004)
$P(y_{i,a} > 125,000)$	0.000	(0.001)	0.000	(0.002)	0.000	(0.002)	0.003	(0.003)
$P(y_{i,a} > 130,000)$	0.000	(0.001)	0.000	(0.001)	0.000	(0.002)	0.001	(0.002)
$P(y_{i,a} > 135,000)$	0.000	(0.001)	0.000	(0.001)	-0.001	(0.001)	0.001	(0.002)
$P(y_{i,a} > 140,000)$	0.000	(0.001)	0.000	(0.001)	0.000	(0.001)	0.001	(0.002)
$P(y_{i,a} > 145,000)$	0.001	(0.001)	0.001	(0.001)	0.001	(0.001)	0.001	(0.002)
$P(y_{i,a} > 150,000)$	0.000	(0.001)	0.001	(0.001)	0.001	(0.001)	0.002	(0.002)

Source: Authors' own calculations using data from Statistics Norway.
Note: Marginal effects (ME) evaluated at the covariate values equal to the average of the treatment group in the post-reform period and associated standard errors (SE) for each of the estimations of Equation (3). Standard errors are based on 200 non-parametric bootstraps for each estimation, clustered on individual level.

Table A3: Marginal effects, CCDF, average reform effect

Outcome	A. Average reform effect		B. Linear time trend, pre-reform years (2006-2010)		C. Linear time trend, sample years (2009-2014)	
	ME	SE	ME	SE	ME	SE
$P\left(y_{i,a/t} > 0\right)$	-0.001	(0.007)	-0.022	(0.013)	-0.022	(0.013)
$P\left(y_{i,a/t} > 5,000\right)$	0.000	(0.007)	-0.022	(0.013)	-0.022	(0.011)
$P\left(y_{i,a/t} > 10,000\right)$	-0.005	(0.007)	-0.021	(0.013)	-0.021	(0.013)
$P\left(y_{i,a/t} > 15,000\right)$	-0.007	(0.007)	-0.020	(0.012)	-0.020	(0.014)
$P\left(y_{i,a/t} > 20,000\right)$	-0.008	(0.007)	-0.029	(0.014)	-0.029	(0.013)
$P\left(y_{i,a/t} > 25,000\right)$	-0.012	(0.007)	-0.036	(0.014)	-0.036	(0.014)
$P\left(y_{i,a/t} > 30,000\right)$	-0.015	(0.007)	-0.035	(0.014)	-0.035	(0.014)
$P\left(y_{i,a/t} > 35,000\right)$	-0.021	(0.007)	-0.032	(0.014)	-0.032	(0.014)
$P\left(y_{i,a/t} > 40,000\right)$	-0.031	(0.007)	-0.041	(0.016)	-0.041	(0.015)
$P\left(y_{i,a/t} > 45,000\right)$	-0.044	(0.008)	-0.053	(0.016)	-0.053	(0.015)
$P\left(y_{i,a/t} > 50,000\right)$	-0.047	(0.008)	-0.060	(0.016)	-0.060	(0.017)
$P\left(y_{i,a/t} > 55,000\right)$	-0.046	(0.009)	-0.071	(0.018)	-0.071	(0.016)
$P\left(y_{i,a/t} > 60,000\right)$	-0.045	(0.009)	-0.078	(0.019)	-0.078	(0.017)
$P\left(y_{i,a/t} > 65,000\right)$	-0.035	(0.009)	-0.077	(0.016)	-0.077	(0.016)
$P\left(y_{i,a/t} > 70,000\right)$	-0.029	(0.008)	-0.069	(0.016)	-0.069	(0.015)
$P\left(y_{i,a/t} > 75,000\right)$	-0.022	(0.007)	-0.061	(0.014)	-0.061	(0.014)
$P\left(y_{i,a/t} > 80,000\right)$	-0.011	(0.006)	-0.044	(0.012)	-0.044	(0.012)
$P\left(y_{i,a/t} > 85,000\right)$	-0.009	(0.005)	-0.034	(0.011)	-0.034	(0.010)
$P\left(y_{i,a/t} > 90,000\right)$	-0.007	(0.004)	-0.023	(0.008)	-0.023	(0.009)
$P\left(y_{i,a/t} > 95,000\right)$	-0.007	(0.004)	-0.018	(0.007)	-0.018	(0.008)
$P\left(y_{i,a/t} > 100,000\right)$	-0.004	(0.003)	-0.013	(0.006)	-0.013	(0.006)
$P\left(y_{i,a/t} > 105,000\right)$	-0.003	(0.002)	-0.009	(0.005)	-0.009	(0.005)
$P\left(y_{i,a/t} > 110,000\right)$	-0.001	(0.002)	-0.006	(0.004)	-0.006	(0.004)
$P\left(y_{i,a/t} > 115,000\right)$	-0.001	(0.002)	-0.004	(0.004)	-0.004	(0.004)
$P\left(y_{i,a/t} > 120,000\right)$	0.001	(0.002)	-0.003	(0.003)	-0.003	(0.003)
$P\left(y_{i,a/t} > 125,000\right)$	0.000	(0.001)	-0.004	(0.003)	-0.004	(0.002)
$P\left(y_{i,a/t} > 130,000\right)$	0.000	(0.001)	-0.002	(0.002)	-0.002	(0.002)
$P\left(y_{i,a/t} > 135,000\right)$	0.000	(0.001)	-0.002	(0.002)	-0.002	(0.002)
$P\left(y_{i,a/t} > 140,000\right)$	0.000	(0.001)	-0.002	(0.002)	-0.002	(0.002)
$P\left(y_{i,a/t} > 145,000\right)$	0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)
$P\left(y_{i,a/t} > 150,000\right)$	0.001	(0.001)	0.000	(0.001)	0.000	(0.001)

Source: Authors' own calculations using data from Statistics Norway.
Note: Marginal effects evaluated at the covariate values equal to the average of the treatment group in the post-reform period and associated standard errors for each of the estimations of Equation (A2) (Panel A), Equation (A8) (Panel B) and Equation (A9) (Panel C). Standard errors are based on 200 non-parametric bootstraps for each estimation, clustered on individual level.

A3 OLS

A3.1 Year-by-year effect

In order to derive the year-by-year impact of introducing the flexible old-age pension on mean labor earnings, we estimate the following linear difference-in-difference equation:

$$y_{i,t} = \alpha + X_{i,t}\beta + \sum_{a=61}^{65} \gamma_a D_a + \sum_{t=2010}^{2014} \lambda_t D_t + \sum_{a=2010}^{2014} \eta_t D_t \Delta_{i,t} + \varepsilon_{i,t} \tag{1}$$

Here $y_{i,t}$ is the annual pre-tax earnings of individual i in year t. $X_{i,t}$ includes controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. $\Delta_{i,t}$ indicates the treatment variable, and equals one when individual i is in the treatment group after the reform and zero otherwise. D_a are dummy variables for age (60 as reference) and D_t are dummy variables for year (2009 as reference).

Table A4: OLS estimation results

	A. Aggregate effect		B. Year-specific effects	
	(1)	(2)	(3)	(4)
Treatment effect (η)	-1875	-902		
	(806)	(626)		
Treatment effect at:				
Year 2010 (η_{2010})			-61	-235
			(651)	(527)
Year 2011 (η_{2011})			-3489	-2590
			(1367)	(1137)
Year 2012 (η_{2012})			-2647	-2048
			(1130)	(864)
Year 2013 (η_{2013})			-761	-124
			(922)	(719)
Year 2014 (η_{2014})			-1721	-444
			(867)	(678)
Year and age dummies	✓	✓	✓	✓
Control variables	×	✓	×	✓
Adjusted R^2	0.049	0.302	0.049	0.302
Individuals (N)	51,860	51,860	51,860	51,860
Sample size ($N \times T$)	140,729	140,729	140,729	140,729

Source: Authors' own calculations using data from Statistics Norway.
Note: OLS estimation results of the aggregate reform effect on earnings (Panel A) from Equation (1) and the year-specific reform effects on earnings (Panel B) from Equation (1). Control variables are pre-determined and include linear controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. Standard errors (in parentheses) are clustered on the individual level, based on 200 non-parametric bootstraps.

A4 Weekly working hours

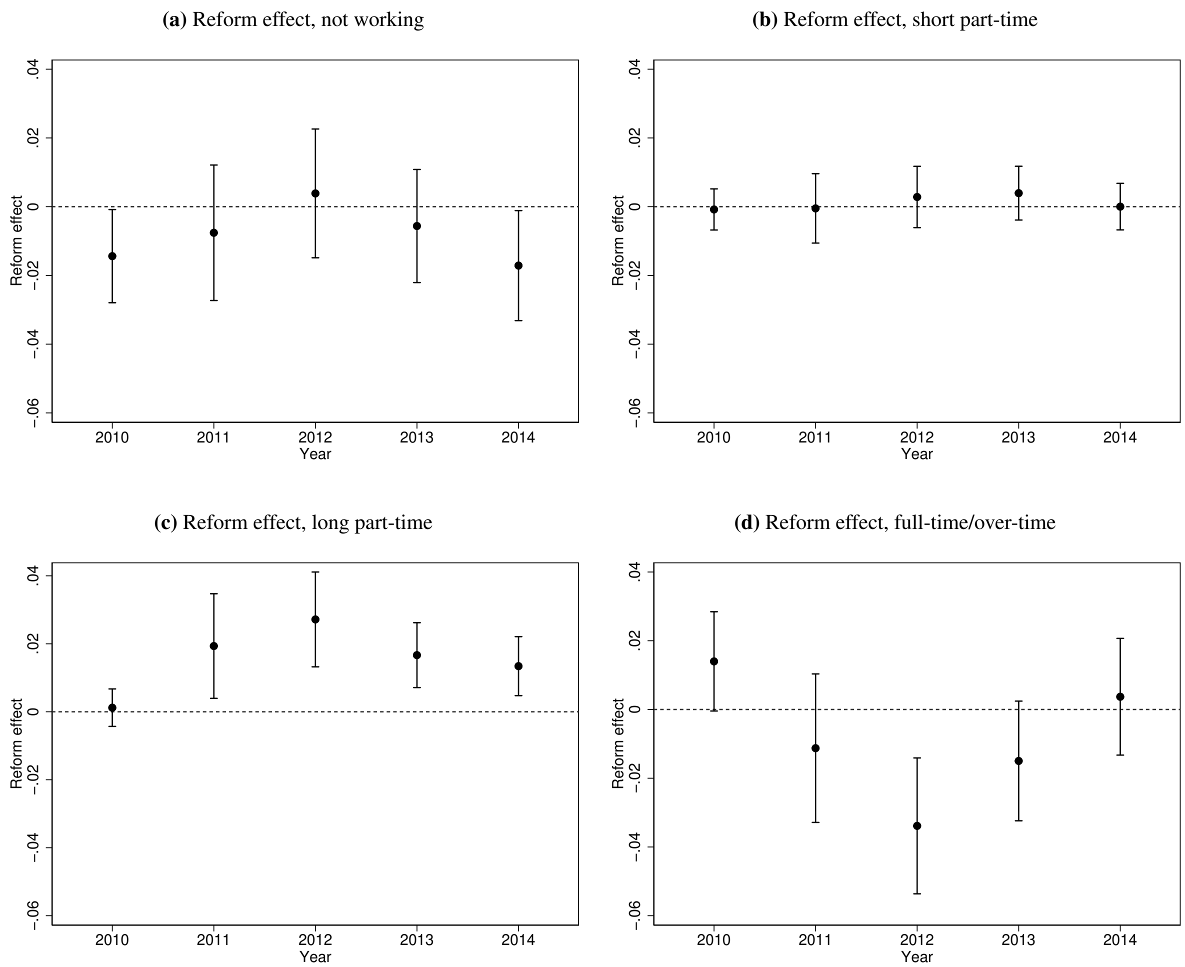
A4.1 Year-by-year effect

In order to include covariates and consider the year-by-year reform effects, we estimate the following multinomial logit model for the four weekly-hours groups keeping the age-specific treatment effects constant:

$$P(y_{i,t}=j)=\frac{\exp(\eta_{i,j})}{\sum_{j=1}^{4}\exp(\eta_{i,j})},\quad \eta_{i,j}=\alpha+X_{i,t}\beta+\sum_{a=61}^{65}\gamma_a D_a+\sum_{t=2010}^{2014}\lambda_t D_t+\sum_{t=2010}^{2014}\delta_t D_t \Delta_{i,t} \tag{A10}$$

Here the alternatives are $j \in \{\text{not working}, \text{short part-time}, \text{long part-time}, \text{full-time/over-time}\}$. The results from this exercise are illustrated in Figure A5.

Figure A5: Reform effect on weekly hours, by year



Source: Authors' own calculations using data from Statistics Norway.
Note: Results from estimation of Equation (A10), showing the estimated marginal reform effects by year and the associated 95 percent confidence intervals (caped lines, based on 200 non-parametric bootstraps for each estimation, clustered on individual level). The weekly-hours groups are defined as follows: (i) not working: 0 hours, (ii) short part-time: 0-20 hours, (iii) long part-time: 20-34 hours, and (iv) full-time/over-time: 34 hours or more. Marginal effects and the associated standard errors are shown in Table A5.

Table A5 shows the marginal effects (evaluated at the covariate values equal to the average of the treatment group in the post-reform period) and the associated standard errors for each of the estimations of Equation (A10) used to simulate the year-by-year reform effects.

Table A5: Marginal effects, reform effect by year

Year	Not working		Short part-time		Long part-time		Full-time/over-time	
	ME	SE	ME	SE	ME	SE	ME	SE
2010	-0.014	(0.007)	-0.001	(0.003)	0.001	(0.003)	0.014	(0.007)
2011	-0.008	(0.010)	0.000	(0.005)	0.019	(0.008)	-0.011	(0.011)
2012	0.004	(0.009)	0.003	(0.004)	0.027	(0.007)	-0.034	(0.010)
2013	-0.006	(0.008)	0.004	(0.004)	0.017	(0.005)	-0.015	(0.009)
2014	-0.017	(0.008)	0.000	(0.003)	0.013	(0.004)	0.004	(0.008)

Source: Authors' own calculations using data from Statistics Norway.
Note: Marginal effects (ME) evaluated at the covariate values equal to the average of the treatment group in the post-reform period and associated standard errors (SE) for each of the estimations of Equation (A10). Standard errors are based on 200 non-parametric bootstraps for each estimation, clustered on individual level.

A4.2 Age-by-age effect

Table A6 shows the marginal effects (evaluated at the covariate values equal to the average of the treatment group in the post-reform period) and the associated standard errors for each of the estimations of Equation (5) used to simulate the age-by-age reform effect on the earnings distribution (shown in Figure 8).

Table A6: Marginal effects, reform effect by age

Age	Not working		Short part-time		Long part-time		Full-time/over-time	
	ME	SE	ME	SE	ME	SE	ME	SE
62	0.005	(0.006)	-0.005	(0.003)	0.008	(0.003)	-0.008	(0.007)
63	0.007	(0.006)	0.002	(0.004)	0.019	(0.005)	-0.028	(0.008)
64	-0.005	(0.007)	0.009	(0.005)	0.020	(0.005)	-0.024	(0.009)
65	-0.009	(0.008)	0.005	(0.005)	0.032	(0.008)	-0.028	(0.011)

Source: Authors' own calculations using data from Statistics Norway.
Note: Marginal effects (ME) evaluated at the covariate values equal to the average of the treatment group in the post-reform period and associated standard errors (SE) for each of the estimations of Equation (5). Standard errors are based on 200 non-parametric bootstraps for each estimation, clustered on individual level.

A5 Job transitions

Job changes in Figure 9 are defined as follows:

1. Starting with the population of workers (excluding self-employed), the main employer for each year 2001-2014 is defined as the establishment identifier from which the worker earns the greatest annual wage income.

2. Using the population of workers and comparing year t with year $t-1$, we define the following states for a worker in year t based on the worker's observed state in year $t-1$:

   (a) Job-to-job transition in year t (new hire): this is defined as worker i's transition from the main establishment identifier in year $t-1$ (denote this j) to another main establishment identifier in year t (denote this k), where the main establishment refers to the establishment identifier of the main employer. The reported starting month in year t is recorded.

      - If we observe that worker i is also registered at establishment identifier k in year $t-1$ as a secondary employer (with a reported starting month later than June and at least 33 percent of total annual wage income derived from establishment k in year $t-1$), we redefine the match to have taken place in year $t-1$ (and not in year t) and the starting month in year t is replaced with the starting month in year $t-1$.
      - This takes into account the possibility that worker i started in the new job already in year $t-1$, but that the new employer was not the main employer (based on the annual wage income) during year $t-1$.

   (b) Same job in year t (not a new hire): if the affiliated main establishment identifier of worker i in year t is identical to the affiliated main establishment identifier in year $t-1$ (meaning that $j=k$), worker i is defined not to be a new hire in year t.

   (c) Transition from out-of-work to job in year t (new hire): if worker i is not in state (a) or state (b) in year t, the worker has come from an out-of-work state.

3. The data set on worker states in year t relative to year $t-1$ is merged with a sample of workers from birth cohorts 1945 and 1949 fulfilling our sample requirements (working at age 59 with no disability benefit receipt, no AFP and being eligible to access to the old-age pension from age 62). This sample is then followed from age 59 until age 65.