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Work less but stay longer — Mature worker responses to a flexibility reform☆

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ABSTRACT

Many consider that reducing the eligibility age for pension benefits will discourage labour supply among mature workers. This paper analyses a recent Norwegian pension reform that effectively lowered the eligibility age for retirement for some workers from 67 to 62, and we find that the above proposition might not be true. For these workers, the option of flexibly claiming from age 62 is introduced while the expected present value of benefits is held constant through actuarial adjustment of the periodic pension payments. This reform provides us with a unique opportunity to isolate the impact of increased flexibility on labour supply. We employ an event-study difference-in-difference approach to study the labour supply response (measured by number of weekly working hours as well as labour earnings) on the intensive and extensive margin. For those aged from 62 to 66, the reform leads to an increase in the labour supply at the extensive margin of 1.3 percentage points with a reduction in inflow to disability, and a decrease in the intensive margin of labour supply as measured by weekly working hours. In addition, a shift in the distribution of labour earnings further supports the finding that there is a decrease in the intensive margin of labour supply. Our findings thus suggest that increased pension flexibility could promote a gradual exit from the labour market, allowing for greater individual choice and positive welfare effects. This flexibility could also be an important component of broader pension reform.

1. Introduction

The age at which retirement benefits can be accessed is generally seen as a key driver of the retirement decision. As life expectancy increases, more than a dozen countries in the OECD group have increased the access age to induce workers to postpone their withdrawal from the labour force (OECD, 2017). Without additional measures, such reforms may reduce flexibility in labour force withdrawal. This may potentially discourage workers from gradually transitioning into retirement as they age, and instead encourage abrupt retirement upon reaching the earliest access age or exit from the labour force by means of disability benefit claiming.

The 2011 Norwegian pension reform took an alternative approach. While the main elements of the reform, such as automatic longevity adjustments and the abolition of all earnings tests in the private sector, are intended to ensure the long-term fiscal balance of the public pension system, it introduced additional flexibility in the pension access age, including a significant reduction in the age at which access is possible, from 67 years to 62 years, for a key subgroup of workers (Stølen et al., 2020).

The introduction of flexibility aims to bring uniformity to the budget constraints faced by subgroups within the retirement window; prior to the reform, groups faced a diverse range of incentives and access ages. As a result of this flexibility, workers are able to adapt to the new and increased work incentives from abolishing the earnings tests and can balance out the longevity adjustments by delaying their exit from working life.

This paper studies the effect of increased retirement flexibility by analysing the decisions of a particular subgroup of Norwegian workers before and after the 2011 pension reform. For this subgroup, the retirement-pension access age was changed from being fixed at 67 to a flexible option for access starting at age 62. After the reform, the payments are not earnings-tested and are actuarially adjusted to maintain the constant present value of benefits, regardless of the actual age at

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which they are accessed. This paper studies the reform impacts on the labour supply over the age range 62–66 of this subgroup of workers, and assesses whether a more flexible pension system facilitated more gradual withdrawal from the labour market.

Our analysis is based on rich administrative data from various Norwegian registers. We use data on both the number of contracted weekly working hours and labour earnings to study the labour supply effects. For the weekly working hours, we study the effect on both the extensive margin and the intensive margin. In addition, we study the changes in labour earnings, which has two advantages. First, the labour earnings data are of higher quality than the hours worked, as they are derived from tax return registers. Second, labour earnings may reflect not only a reduction in the number of weekly hours worked, but also a change to a less demanding and lower-paid job without a corresponding change in the number of working hours.

We identify the reform effects, following Sun and Abraham (2021), by employing an event-study difference-in-difference approach. We find that the flexibility component of the reform leads to (i) an increase in labour supply at the extensive margin at later ages, and (ii) a decrease at the intensive margin as measured by both weekly working hours and labour earnings. The extensive margin and intensive margin response in terms of labour earnings yield a near-zero effect on total labour supply. But the welfare impact of the additional flexibility afforded by the reduced access age will be important, especially because at later ages, there is substantial heterogeneity in individual circumstances – family and health are as important as access to retirement income. The greater flexibility gives people a much wider range of choices to suit their circumstances.

2. Related literature

Increased flexibility in the pension system can significantly impact aggregate work effort among older workers by facilitating gradual retirement, even without altering their economic incentives for work. This flexibility allows individuals a broader range of choices regarding exit from the labour market while enabling simultaneous claims for part or full pension benefits. Several surveys (Dalen, 2016; Brown, 2005; Tuominen, 2013) indicate a widespread desire among workers to gradually reduce work hours and among employers to retain the skills of experienced employees. Ameriks et al. (2020) utilises data from strategic survey questions and finds that older Americans would likely work longer if job conditions were more flexible.

Despite the apparent demand for gradual retirement, it is generally underutilised. There are several possible reasons for this. Employers may be reluctant to accommodate reduced working hours for various reasons: fixed costs associated with employment, such as office space; the invariable time commitment demanded for tasks like meetings that consume a disproportionate amount of time for part-time employees; potential skill loss from less frequent practice in a part-time role; and diminished productivity among other employees due to interdependencies (Hutchens and Grace-Martin, 2006; Hutchens, 2010; Blau and Shvydko, 2011; Even and MacPherson, 2004; Cahill et al., 2014). Individuals might also face inherent drawbacks of employment, such as restrictions on leisure activities, commuting challenges, and work-related stress (Fan, 2015; Angrisani et al., 2015; Böckerman and Ilmakunnas, 2020).[1] The fixed costs for employers and employees may make the option of gradual retirement (through part-time work) as a lead-up to full retirement unappealing.

The impacts of increased flexibility on labour supply responses and earnings among older workers are empirical questions. Börsch-Supan et al. (2018) analyse data from nine OECD countries using a synthetic control method, finding that flexibility reforms since the 1990s have had positive, albeit small, effects on the rates of labour force participation (LFP) among older men aged 55–64 but decreased their weekly working hours, thus resulting in a negative (at best zero) impact on total labour supply. Similarly, Eurofound (2016) examines partial retirement and concludes that there was "no scheme [...] identified that unambiguously extended working lives for all participants" (p. 1). In another cross-country comparison, Baumann and Madero-Cabib (2021) use harmonised data to explore retirement trajectories in four countries with flexible retirement plans – Denmark, Sweden, the United States, and Chile – focusing on different welfare regimes and health conditions. The study finds no evidence that flexible retirement plans extend the working lives of seniors and the authors argue that more generous early retirement schemes encourage early retirement. Additionally, adverse health conditions do not necessarily hinder LFP in later life; rather, this participation is more significantly impacted by the institutional setup.

By contrast, simulation results from a structural model based on German data (Haan and Tolan, 2017) suggest that allowing access to partial retirement with pension payouts can increase employment volume and generate positive fiscal effects, especially when the access age is aligned with the early retirement age. Berg et al. (2020) analyse Germany's Altersteilzeit policy, which offers opportunities for partial retirement with pension benefits, and find significant increases in working lives, particularly among women.

Our study contributes to this literature by isolating the effect of the flexibility introduced to the pension benefit-claiming decision on the labour supply of individuals, with no confounding effects of changed labour-supply incentives. The detailed administrative data we use allow us to trace the effects beyond the usual average impact. At the aggregate level, our findings are quite similar to those of Hernæs et al. (2016), where the authors conclude that "as long as the size of the pension wealth is fixed, there are small – almost negligible – labour supply responses to changes in the access age" (p. 52). While their results on the impact of flexible pension access are based on the analysis of a different sample of workers, in which subgroups face different combinations of incentive and flexibility changes, our study is focused on a group exposed only to a change in access age, enabling a much cleaner identification of the effects of a flexibility change. In addition, our study suggests that there are clear heterogeneous responses in terms of the extensive margin decision and the intensive margin decision across the age range 62–66.

The remainder of the paper is organised as follows. In Section 3 we provide some institutional background to the Norwegian reform to place it in its broader context. Section 4 contains our description of data and sample used. We present our empirical approach in Section 5 and our estimation results in Section 6. In Section 7, we estimate the effect of the reform on disability benefit claiming. Finally, Section 8 concludes.

3. Institutional setting

3.1. Before the reform

The backbone of retirement provision in Norway is a mandatory benefit plan, the old-age pension through the National Insurance Scheme (NIS).[2] The benefit is dependent on a retiree's income history, but bounded from below and above. The accrual rate basis is capped at around the average full-time earnings and the pension is based on average accrual over the best 20 years, indexed to wages. At constant real earnings, generating a pension equal to the guaranteed minimum pension level, the replacement rate is 60%. At constant real earnings at the maximum level of pension accrual, the replacement rate is 33%.

[1] Conversely, employment can offer a stimulating environment (Kantarci and van Soest, 2013).

[2] Further details on the NIS can be found in a survey from the Norwegian Ministry of Labour and Social Affairs (2019).

In 2017, the NIS old-age pension provided two-thirds of total income received by individuals aged 75, an age at which labour earnings play a minimal role in total income. Occupational pensions account for another 20% of total income at age 75, and capital income for most of the remainder.

Prior to the 2011 pension reform, there was little flexibility in claiming a pension in Norway. The occupational pensions in the private and public sector and the NIS old-age pension had an eligibility age of 67. In general, there were strict earnings tests applicable after the eligibility age and no deferral of benefits; these features went a long way to defining age 67 as the retirement age, when earnings stopped and a pension was received.

In the pre-reform era, before reaching the eligibility age, there were only two exit routes of any importance. The first was a permanent disability pension. At age 66, around 40% of the population were receiving permanent disability benefits. The other exit option (before the reform) was the early retirement pension (*avtalefestet pensjon* in Norwegian, henceforth the AFP), which was introduced in 1989. The AFP covers the public sector and around half of the private sector; firms in the private sector must "opt in" for employees to have the option of early retirement. In addition, individuals have to meet a set of requirements with respect to their earnings and employment history. Before 2011, the AFP scheme covered the age range from 62–67. Those eligible had the option of combining work and pension benefits, with the percentage of normal earnings and the percentage of a full pension adding up to 100%. However, any pension benefit that was not claimed was not preserved, implying a high total tax on continued work (Hernæs et al., 2016).

3.2. After the reform

Flexibility was introduced in the 2011 pension reform (Brinch et al., 2017; Kudrna, 2017; Stølen et al., 2020). The NIS old-age pension, all occupational pensions, and the private sector AFP could each be claimed from between age 62 and age 75, with actuarial adjustment and without tests against continued earnings. There was also an option of claiming only part of the benefits. However, claiming the NIS old-age pension required entitlements that gave an annual pension level at least equal to the guaranteed minimum pension from age 67.[3]

In the private sector, the AFP was transformed from a strictly earnings-tested early retirement pension over the age range 62–66 into a non-tested, life-long pension that had to be claimed in combination with the NIS old-age pension. Since the earnings test was repealed, there was a sharp increase in work incentives for those with AFP, delaying their labour market exit (Hernæs et al., 2016). The annual benefit level was reduced so as to preserve the present value of the AFP public subsidy.

For workers with sufficient entitlements to be eligible for the new NIS old-age pension from age 62, and who were not covered by the AFP scheme, the only change was their access to the new old-age public pension, and in most cases of any occupational pension, from age 62. The level of annual NIS old-age pension benefits was actuarially adjusted to preserve a constant present value. Hence, the main change for this group was a change in the flexibility of pension, and this is the group we will analyse for the impact on work behaviour.

The actuarial adjustment of the annual old-age pensions described above is designed to be neutral, but the adjustment implies the same implicit discount rate for all. If the subjective discount rate deviates from the internal rate in the pension system, this could give rise to incentives to claim early or late. For instance, there is a strong mortality gradient in income, which could lead to deviations from the system's implicit discount rate (see, e.g., Brinch et al. (2018) for an empirical study). It is important to emphasise, however, that while this may alter the incentives for *claiming*, it does not alter the incentives for *work*.[4]

[3] The reform did not change the incentives for individuals to claim old-age pensions instead of disability benefits or vice versa.

4. Data and descriptive statistics

4.1. Data

The empirical foundation of our analyses is an extensive set of administrative register data from Statistics Norway, which covers the entire population of Norway. Our primary source is the linked employer-employee data sets, encompassing all wage workers and employers and which include contracted weekly working hours in their primary job each year, industry of the firm, and worker occupations. Another important source is the tax return register, which contains information on labour earnings, wealth and taxes. Additionally, we have access to a dataset of all private firms offering the AFP, provided by the early retirement administration unit. The employer-employee data and the AFP data are linked using encrypted firm identification numbers. We also employ several other registers covering the entire population and linked via unique encrypted personal identification numbers to obtain other individual characteristics, such as education levels.[5] Contracted weekly working hours are reported to an employment register at the start of an employment spell. If the contracted weekly working hours are changed, the end of the ongoing spell and the start of a new spell should be reported. The number of employees with an employment spell corresponds well to the number of employed in the Labour Force Survey. Earnings are reported to the tax authorities from employers on all payments and benefits related to the employment relationship. The amounts are the basis for taxation and are considered reliable.

4.2. Sample

The substantial and sudden changes in the options available for potential retirees following the 2011 pension reform provide a unique opportunity to investigate the impact of allowing pure flexibility, isolated from any change in economic incentives. Notably, none of the individuals in our sample had access to the private sector AFP. Thus, the reform that impacts the workers in our sample introduces only flexibility of pension access starting from age 62, without any changes to economic incentives. Starting at age 67, all individuals in each birth cohort gain access to the NIS pension and are no longer eligible to receive disability benefits. Consequently, the available alternatives change on the 67th birthday.

The sample is constructed as described below and illustrated in Table 1 for the 1949 birth cohort. Initially, we select individuals from birth cohorts 1943–1953 who were observed to be working in at least one of the years between 2009 and 2014.[6] Next, we focus on individuals who, in the year they turned 59, (i) were employed as wage earners, (ii) earned at least EUR 10,000, and (iii) were not receiving any disability benefits, whether permanent or temporary. We further narrow the

[4] In Norway, combining pension and employment income may increase the tax levied on the labour income with the same work effort if the combined income is in a higher tax bracket. In this case, the work incentive will be lower than before and our estimates can be considered the lower bound of the effects of a pure flexibility reform.

[5] All monetary amounts in this paper are initially measured in NOK, deflated using the consumer price index, and then converted to 2014-EUR using the average EUR/NOK exchange rate in that year (1 EUR = 8.35 NOK). Self-employed workers are not included in our sample.

[6] We exclude birth cohorts from 1954 onwards due to a new pension accrual system introduced in that year, which could have influenced their labour supply (Halvorsen et al., 2024). The years prior to 2009 are not considered in our main analysis due to the phasing in of mandatory occupational pensions for all employers starting in July 2006, as part of a previous partial pension reform.

Table 1
Sampling in the 1949 birth cohort.
Source: Authors' own calculations using data from Statistics Norway.

	All	Males	Females
Birth cohort 1949 at age 59	52,585	27,302	25,283
Working at age 59 and no disability benefits	39,608	22,123	17,485
Not covered by AFP early retirement scheme	9,078	5,838	3,240
Eligible for new old-age early pension from age 62 after reform	6,518	5,391	1,127

Note: Number of observations in 1949 birth cohort by sample restriction. Working is here defined as having labour earnings above EUR 10,000; disability benefits include temporary and permanent disability benefits.

Table 2
Exposure to reform, by birth cohort and age.

	60	61	62	63	64	65	66
1943							
1944							
1945							2011
1946						2011	2012
1947					2011	2012	2013
1948				2011	2012	2013	2014
1949			2011	2012	2013	2014	
1950			2012	2013	2014		
1951			2013	2014			
1952			2014				
1953							

Note: Each cell shows the year in which each birth cohort could access the new old-age pension, by age. The grey cells indicate the data we use, which span the years 2009–2014.

group by including only those who were ineligible for AFP because their employer had not participated in the scheme. Finally, we include only those who met the post-reform requirements for claiming the new public old-age pension, with actuarial adjustments, at age 62. Prior to the reform, the access age was 67, without the option for deferral.[7]

While the formula for pension point accrual allows 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 labour supply behaviour of those in the 62–66 age group. First, having conditioned the sample of individuals as eligible for the new old-age pension from age 62, most individuals have a sufficient number of years of residence (40) to obtain the full basic old-age public pension (first pillar). Second, most have their 20 best earnings-years before 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.[8] Thirdly, as demonstrated by Brinch et al. (2017) in a different but related setting, the accrual incentives lack salience, leading to a scenario in which individuals do not take the old-age pension accrual into account when determining their labour supply.

Table 2 shows in what year and at what age the birth cohorts we study could access the new old-age pension, where the grey cells highlight the data used (spanning the years 2009–2014). The 1943 and 1944 birth cohorts were not affected by the reform, as they reached 67 prior to 2011. The 1953 birth cohort was also unaffected by the reform assessed in our data, as they turned 62 in 2015, which is beyond our sample window. Those in the 1945 birth cohort could first access the new old-age pension in 2011 at age 66, while the 1950 birth cohort first had access to the new old-age pension in 2012 at age 62. None of the birth cohorts had access to the new old-age pension before the age of 62.

4.3. Measuring work

Our main measure of work is contracted weekly working hours, which allows us to study both the extensive and intensive margin responses. The contracted weekly working hours reflects the anticipated labour supply specified in the work contract and not the actual number of hours worked.[9] Contracted hours of work are reported by the employer, with no serious legal repercussions for inaccuracies. As a result, changes in contracted hours often go unreported. Thus, we choose to complement this measure by also using the annual labour earnings as an outcome. In contrast, the earnings data come from tax return registers, where misreporting carries serious legal consequences. Consequently, the quality of reported contracted weekly working hours is lower than that of labour earnings. One potential advantages of using annual earnings is that it may also capture changes in work effort resulting from adjustment to less demanding and lower paid jobs with no corresponding reduction in the number of working hours, which is an important aspect of labour supply.[10]

4.4. Descriptive statistics

Table 3 presents the LFP rate, average number of weekly working hours (conditional on working) and average annual labour earnings for the sample, with columns representing age and rows representing the year of observation. We observe a notable time trend in all three variables: for workers of the same age, higher LFP, more weekly working hours of work and higher labour earnings are generally evident in more recent years. Additionally, there is a distinct aging trend: in any given year, older workers tend to have lower LFP, fewer weekly working hours and lower labour earnings than younger workers. However, the time trend is insufficient to offset the aging effect: for the same birth cohort, a significant reduction in labour supply is still observed over time.

Table 4 shows the descriptive statistics for the whole sample, which we also split into person-age observations with access to the new old-age pension (ages 62–66 after the reform) and person-age observations with no access to the new old-age pension (ages 60–61 both before and after the reform, and ages 62–66 before the reform). As expected, the LFP rate, the average number of weekly working hours and annual labour earnings (which do not include old-age pension income) are lower for those with access to the old-age pension. However, in terms of covariates, the sample is balanced across those with and without access to the new old-age pension.

5. Empirical approach

5.1. The identification strategy

We study the impact of the 2011 pension reform, following Sun and Abraham (2021), employing an event-study identification strategy

[7] By ensuring that the individuals included were not employed in the public sector or by a private sector firm with AFP participation at age 59, we make sure that they do not qualify for the AFP in terms of the pre-reform requirements. A small subset of private sector workers had a lower eligibility age; however, we cannot identify these individuals in the data.

[8] For the birth cohorts we consider, 94% of individuals experienced their highest earnings-years before the age of 62, see Appendix A1.1.

[9] Note however that the data on the contracted number of weekly working hours exhibit substantial bunching at the full-time (37.5 h) level, see Appendix A1.2 for the distribution of contracted weekly working hours.

[10] While the hourly wage rate is the most accurate measure of effort per unit of work, we do not have data on this for our sample. Due to the difference between the contracted weekly working hours and actual weekly working hours, we are not able to derive the *ex post* hourly wage by using the number of contracted weekly working hours and annual earnings.

Table 3
Labour force participation rate, weekly working hours and annual earnings, by year and age.
Source: Authors' own calculations using data from Statistics Norway.

	60	61	62	63	64	65	66
2009	0.974	0.929	0.891	0.803	0.705	0.642	0.538
	36.2	36.1	35.9	35.6	34.9	34.6	34.3
	63,685	60,265	55,482	48,626	40,546	35,807	29,035
2010	0.964	0.929	0.885	0.797	0.729	0.626	0.550
	36.2	36.0	35.8	35.4	35.0	34.6	34.2
	65,108	59,778	55,444	48,979	43,278	34,572	29,636
2011	0.973	0.982	0.894	0.796	0.720	0.648	0.538
	36.3	36.0	35.7	35.3	35.0	34.4	33.9
	68,035	63,821	57,304	49,601	44,432	37,232	28,825
2012	0.972	0.936	0.895	0.813	0.738	0.667	0.561
	36.4	36.4	36.0	35.4	35.0	34.5	34.1
	69,769	66,852	61,050	52,689	46,327	40,121	32,061
2013	0.974	0.939	0.905	0.819	0.754	0.680	0.581
	36.4	36.4	36.2	35.4	34.8	34.3	33.9
	70,979	67,128	63,263	55,282	48,631	40,848	33,966
2014		0.942	0.907	0.824	0.764	0.691	0.589
		36.4	36.1	35.9	34.9	34.4	33.6
		68,330	63,177	56,079	50,093	42,325	34,369

Note: Each cell shows the labour force participation rate in percent (first row), average number of contracted weekly working hours conditional on working (second row), and the average annual labour 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. We zero earnings to those not in the labour force.

Table 4
Descriptive statistics.
Source: Authors' own calculations using data from Statistics Norway.

	Ages 60–61		Ages 62–66	
	Before	After	Before	After
Labour force participation (%)	94.9	95.2	72.4	74.0
Average number of weekly working hours	36.2	36.3	35.1	35.1
Average annual labour earnings (EUR)	62,219	67,894	42,735	46,986
Covariates, average values				
Years of completed education	12.9	13.0	12.7	12.9
Annual earnings, ages 30–59 (EUR)	53,663	55,959	51,240	53,310
Net liquid wealth at age 59 (EUR)	5,471	3,230	5,268	3,836
Number of observations	25,170	45,382	59,011	125,100

Note: Descriptive statistics for the age groups 60–61 and 62–66, before and after the reform. Net liquid wealth is defined as the sum of all assets less property and debt. We assign zero labour earnings to those not in the labour force, while the number of weekly working hours is conditional on working.

incorporating a difference-in-difference design. The building blocks for our estimates of the reform effects are cohort- and age-specific average effects, defined similarly to the cohort-specific average treatment effects on the treated in Sun and Abraham (2021). We aggregate these cohort- and age-specific effects using weights representing the shares of birth cohorts affected by the reform at a given age to obtain the average treatment effects on the treated for a given age. Similarly, we can construct reform effects for a specific year or birth cohort and, eventually, the averaged reform effects over all birth cohorts from ages 62–66.

This approach offers a significant advantage over a standard two-way fixed-effect difference-in-difference setup by providing increased robustness against potential bias resulting from variation in the timing of treatment, as in our case. Recent research, including by Goodman-Bacon (2021) and Sun and Abraham (2021), highlights the limitations of employing these two-way fixed-effect models in such situations.[11]

[11] See Appendix A2 for a comparison of the results from the two-way fixed-effect difference-in-difference model and the method discussed here.

However, it should be stressed that two underlying assumptions must still be fulfilled for the estimates to have a causal interpretation. The first, the so-called "parallel trends in baseline outcomes" assumption, essentially resembles the common trend assumption in the standard difference-in-difference model. In our setting, it implies that, in the absence of the reform, the labour market outcomes over different ages across different birth cohorts would exhibit parallel trends.

The second assumption that must be satisfied to ensure identification is the "no anticipation" assumption, which requires that individuals, on average, do not change their behaviour before the reform actually begins to impact them. Specifically, this means that the potential effect of the reform on the labour supply for those who are not yet affected by the reform, namely those in the age group 60–61, should be zero. Yet, forward-looking workers might adjust their labour market behaviour in anticipation of the reform. For instance, before the reform, a 60-year-old worker would need to wait until age 67 to retire and claim old-age pension benefits; post-reform, this waiting period was reduced to two years. Consequently, the incentive to invest in human capital or health to sustain work capacity until retirement may differ between these groups. If those aged 60–61 had less incentive to prepare for a longer working life post-reform, they may already adjust their labour supply before reaching age 62 — which would indicate that even this age group was influenced by the reform. Additionally, workers who might have applied for disability benefits at age 61 before the reform may now choose to continue working until the new earliest retirement age, potentially increasing their LFP rate after age 62 if continuing in the workforce implies sustained labour supply.

In addition to examining the reform effect on labour supply at the extensive margin (labour market participation) and the intensive margin (average hours of work conditional on participation) – as typically found in the literature (see, for example, Börsch-Supan et al., 2018) – we also aim to assess its impact on the combination of these two margins. Specifically, we analyse the unconditional distribution of weekly working hours and labour earnings, as this approach captures the reform effect across the entire outcome distribution and provides insights into its heterogeneous impacts.

6. Empirical results

The descriptive empirical evidence shown in Table 3 suggests that there are effects of the 2011 Norwegian pension reform on the labour

supply, both on the extensive and intensive margin. We rigorously evaluate these effects through an event-study analysis focusing on the number of weekly working hours and labour earnings using ordinary least squares (OLS). We then examine the impacts on the distribution of the number of weekly working hours and labour earnings using the CCDF method.

6.1. Average effects

We estimate the following linear event-study equation to derive the average effect on the labour supply:

$$y_{i,a,t} = \alpha + X_i\beta + \sum_{s=60}^{66} \gamma_s DA_s + \sum_{l=2009}^{2014} \lambda_l DT_l + \sum_{s=60}^{66}\sum_{l=2009}^{2014} \eta_{s,l} DA_s DT_l + \varepsilon_{i,a,t}, \quad (1)$$

where $y_{i,a,t}$ is either an indicator variable for the extensive margin (defined as non-zero contracted weekly working hours), the intensive margin (a continuous variable with the contracted number of weekly working hours, conditional on labour force participation), or a continuous variable with labour earnings (set to zero for individuals not in the labour force) of individual i at age a in year t. The vector X_i 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, DA_s are dummy variables for age (with age 61 as the reference age) and DT_l are dummy variables for year (with 2009 as the reference year); $\varepsilon_{i,a,t}$ is the error term.[12]

In this specification, the coefficients $\eta_{s,l}$ recover the cohort- and age-specific reform effects for birth cohort c ($c = l - s$) at age s, assuming the two conditions discussed in Section 5.1 are met. By including the pre-reform (placebo) treatment effects ($\eta_{61,2010}$, $\eta_{62,2010}$, and so on), we can check whether, in the absence of the reform, there are parallel trends across cohorts. Although insignificant estimates for these placebo treatment effects do not represent a guarantee that the common trend assumption is satisfied, it would be reassuring to see no substantial difference between the age cohorts prior to the reform.

To find the age-specific effects for ages 60–66 (η_a) from the estimation of Eq. (1), we sum the year-age-interaction coefficient across the years 2011–2014 (the period after the reform), with each coefficient weighted by the relative size of the corresponding birth cohort (ω_c). This is essentially the "interaction-weighted" estimator proposed by Sun and Abraham (2021). Similarly, we can derive the year-specific effects for the years 2009–2014 (η_t) from the estimation of Eq. (1), based on the estimates from those cohorts that are affected in the given year. Finally, the reform's aggregate treatment effect (η) is derived as the weighted sum of the age- and year-specific effects:

$$\begin{aligned} \eta_a &= \sum_{l=2011}^{2014} \omega_c \eta_{s=a,l} \quad \text{for } a = 60, \ldots, 66 \\ \eta_t &= \sum_{s=62}^{66} \omega_c \eta_{s,l=t} \quad \text{for } t = 2009, \ldots, 2014 \\ \eta &= \sum_{s=62}^{66}\sum_{l=2011}^{2014} \omega_c \eta_{s,l} \end{aligned} \quad (2)$$

[12] Note that some individuals have no access to the old-age pension in one year and have access the following year. This does not invalidate any point estimates, but it needs to be taken into account when we compute standard errors. We report block-bootstrapped standard errors, with individuals as the unit for bootstrapping. We perform 200 bootstrap replications (see, e.g., Efron and Tibshirani (1993) for the rule of thumb regarding the necessary number of replications).

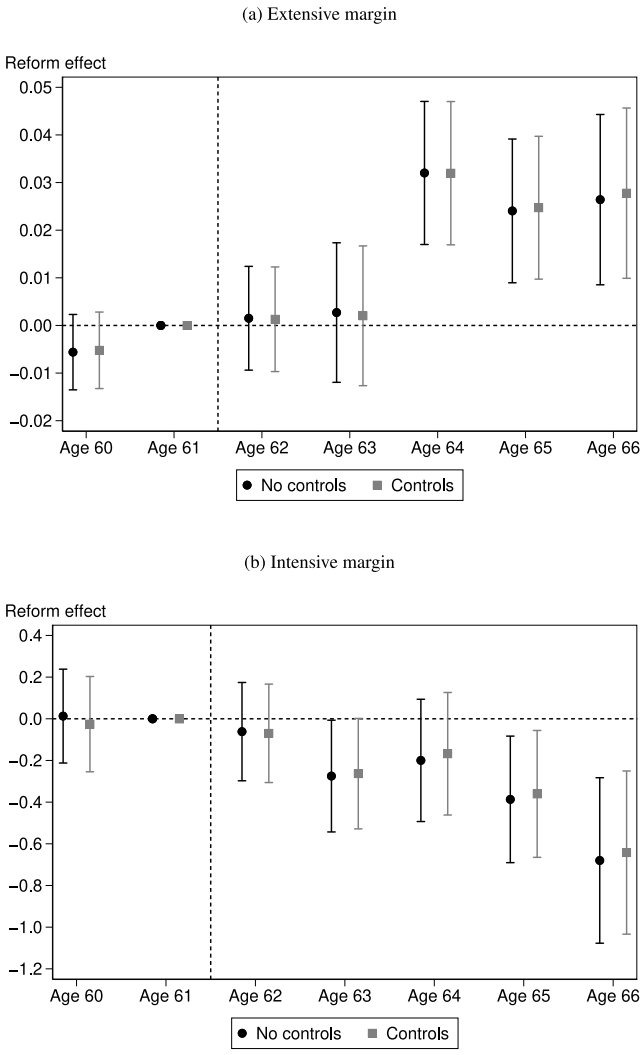


Fig. 1. Effect on extensive and intensive margin, by age.
Note: OLS estimation results of the age-specific effects on extensive margin and intensive margin (defined as the number of weekly working hours conditional on working) from Equation (1). Black lines indicate estimation results with no control variables, grey lines indicate estimation results with control variables. 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. The capped lines show the 95% confidence intervals, based on standard errors clustered at the individual level with 200 non-parametric bootstrap replications. Point estimates and the associated standard errors are deferred to Appendix A3.1.
Source: Authors' own calculations using data from Statistics Norway.

6.1.1. Effect on weekly working hours

In terms of the effect on the number of weekly working hours, the average effect on the extensive margin across all birth cohorts and ages is an increase of 1.3 percentage points (with a standard error of 0.4 percentage points), both with and without predetermined covariates. In terms of the intensive margin of labour supply there is a small decrease of 0.23 weekly working hours (with a standard error of 0.08), both without and with predetermined covariates, respectively. These estimated effects on the intensive margin correspond to a decrease of 0.7% of pre-reform weekly working hours at age 59.

Fig. 1 presents the age-specific average effects ($\hat{\eta}_a$) on the extensive and intensive margin of labour supply. For the extensive margin, the effect is positive for ages 64–66, both with and without the predetermined covariates. For the intensive margin of labour supply, the

effect is negative for ages 62–66 and increasingly so by age, but with relatively large standard errors. Note also that there are no significant effects for individuals aged 60 for both the extensive and intensive margin, as they do not have access to the old-age pension either before or after the reform.[13]

6.1.2. Effect on labour earnings

In terms of labour earnings, the average effect across all birth cohorts and ages is a reduction of EUR 1,016 (with a standard error of 567) and a reduction of EUR 763 (with a standard error of 478), without and with predetermined covariates, respectively. These estimated effects, which nests both the extensive and intensive margin, correspond to a reduction of 2.0% and 1.5% of pre-reform earnings at age 59, respectively.[14]

Fig. 2 presents the age-specific average effects ($\hat{\eta}_a$) on labour earnings. When not including the pre-determined covariates, there is only a statistically negative effect at ages 65–66. Adding the pre-determined covariates shifts the estimate of the age-specific effects, with only the effects at age 63 and 66 now being statistically significant and negative. Note also here there are no significant effects for individuals aged 60, as they do not have access to the old-age pension at that age either before or after the reform.[15,16]

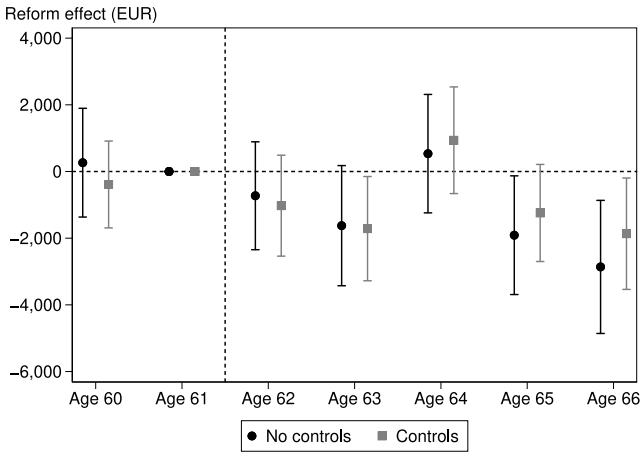


Fig. 2. Effect on labour earnings, by age.
Note: OLS estimation results of the age-specific effects on labour earnings from Eq. (1). Black lines indicate estimation results with no control variables, grey lines indicate estimation results with control variables. Control variables are pre-determined and 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. The capped lines show the 95% confidence intervals, based on standard errors clustered at the individual level with 200 non-parametric bootstrap replications. Point estimates and the associated standard errors are deferred to Appendix A4.1.
Source: Authors' own calculations using data from Statistics Norway.

6.2. Distributional effects

Because workers may respond differently to the flexibility reform, we expect an uneven effect across the distribution of weekly working hours and labour earnings. It is thus helpful to analyse the changes induced by the reform across the entire outcome distribution, whether in terms of weekly working hours or earnings. We achieve this using an estimator based on the complementary cumulative distribution function (CCDF), which is defined as 1 minus the cumulative distribution function, such that $CCDF(y) \equiv 1 - F(y)$. This approach, including the construction of distribution functions and the regression analysis of shifts and covariate impacts, is detailed in Hernæs and Jia (2013) and Brinch et al. (2017). When analysing shifts and effects that may vary across the distribution, this method serves as an alternative to quantile regression (Lingxin and Naiman, 2007). Intuitively, while quantile analysis focuses on modelling the horizontal shifts in the cumulative function, the CCDF method targets the vertical shifts. Both approaches are suitable for analysing distributional shifts, but the CCDF method is less numerically cumbersome.

Following Hernæs and Jia (2013) and Brinch et al. (2017), we extend Equation (1) to study the reform effects on the weekly working hours distribution and the labour earnings distribution. For the weekly working hours, this is achieved by conducting a series of linear regressions on the probability of working more above a series of steps, each of length 1 h, up to 50 h. For labour earnings, we use linear regressions on the probability of earning more than a series of steps, each of length EUR 5,000, up to EUR 150,000. Specifically, for each level of weekly working hours and labour earnings $y_{i,a,t}$ for individual i at age $a = 60, \ldots, 66$ and year t, we model the CCDF as follows:

$$\mathbf{1}\left(y_{i,a,t} > y^k\right) = \alpha^k + X_i\beta^k + \sum_{s=60}^{66} \gamma_s^k DA_s + \sum_{l=2009}^{2014} \lambda_l^k DT_l + \sum_{s=60}^{66}\sum_{l=2009}^{2014} \eta_{s,l}^k DA_s DT_l + \varepsilon_{i,a,t}^k \tag{3}$$

The variables are defined as in Eq. (1), and the outcome variable on the left-hand side equals one if $y_{i,a,t} > y^k$ and zero otherwise. The identifying assumptions are those discussed above for the OLS regression. However, in our setting the parallel trend assumption must hold at *each* point on the support, not just at the mean of the dependent variable: in the absence of the reform, the change in the population shares at each of the hours thresholds across ages would have been the same across cohorts. Similar to the analysis in the previous section, although we cannot directly test this assumption, we can check whether the estimated treatment effects differ from zero for the pre-treated age-cohort combinations.

For the weekly working hours, we estimate the specification for 51 specific cases, letting y^k vary from 1 h to 50 h by increments of 1 h such that $y^k \in \{0, 1, 2, \ldots, 49, 50\}$. Based on 51 separate estimations, one for each of the levels of hours described above, we can simulate the counterfactual CCDF of the weekly working hours distributions. In particular, the coefficient $\eta_{s,l}^k$ shows the marginal effects equal to the difference in the post- and pre-reform probabilities of weekly working hours higher than a given level y^k for those who are age s in year t: $P\left(y_{i,a,t} > y^k | DA_s DT_l = 1, X_i\right) - P\left(y_{i,a,t} > y^k | DA_s DT_l = 0, X_i\right)$, which is the age and cohort-specific reform effect on the CCDF.

For the labour earnings, we estimate the specification for 31 specific cases, letting y^k vary from EUR 0 to EUR 150,000 by increments of EUR 5,000 such that $y^k \in \{0, 5000, 10000, \ldots, 145000, 150000\}$. The average, age- and year-specific average effects (η, η_a and η_t) across birth cohorts can be derived in the same way as in the OLS case using the weights from Eq. (2).

[13] The placebo reform effect estimate for the year-specific effect for 2010, the year prior to the reform, is also statistically insignificant for both the intensive and extensive margin of labour supply, see Appendix A3.1.

[14] These effects are somewhat lower than the 3.3% decrease that Brinch et al. (2021) find using their specification with covariates, as reported in Column (5) of Table 5 in Brinch et al. (2021).

[15] The year-specific effects for labour earnings in 2010, the year prior to the reform, is also statistically insignificant, see Appendix A4.1.

[16] Appendix A3.2 and Appendix A4.2 shows the birth cohort-specific effects on weekly working hours and labour earnings by age for all birth cohorts in our analysis. The same dynamic patterns as shown in Figs. 1 and 2 are also evident for the other cohorts, although there are some indications of heterogeneity in these age-specific reform effects across birth cohorts. The critique of Goodman-Bacon (2021) and Sun and Abraham (2021) may therefore apply in our application.

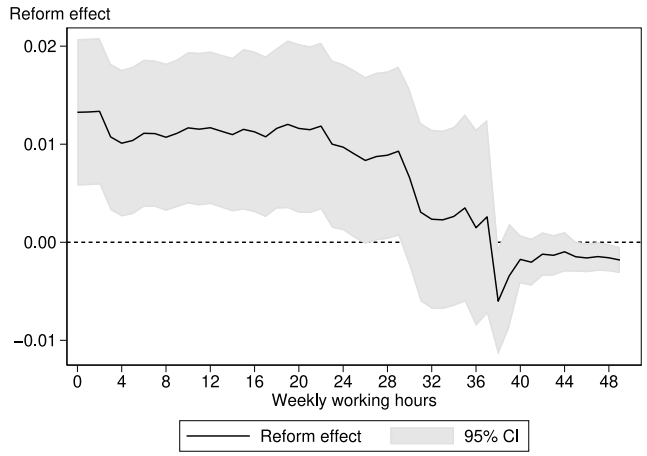


Fig. 3. Effect on weekly working hours distribution.
Note: Simulation results from estimation of Eq. (3). Control variables are pre-determined and include linear controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. The grey-shaded area shows the 95% confidence intervals (based on 200 non-parametric bootstrap replications for each estimation, clustered at the individual level).
Source: Authors' own calculations using data from Statistics Norway.

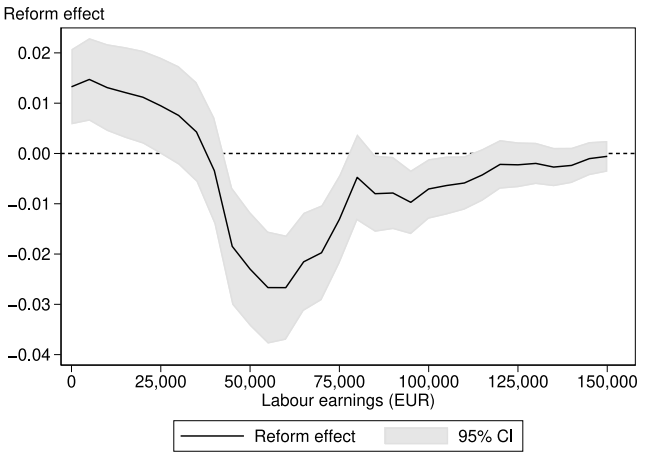


Fig. 4. Effect on labour earnings distribution.
Note: Simulation results from estimation of Eq. (3). Control variables are pre-determined and include linear controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. The grey-shaded area shows the 95% confidence intervals (based on 200 non-parametric bootstrap replications for each estimation, clustered at the individual level).
Source: Authors' own calculations using data from Statistics Norway.

Table 5
Probability of being in selected weekly working hours intervals.
Source: Authors' own calculations using data from Statistics Norway.

Weekly working hours interval	Probability	Effect	
	(before reform)	Estimate	Standard error
Non-participation (0 h)	0.205	−0.013	0.004
Short part-time (1–18 h)	0.033	0.002	0.006
Long part-time (18–35 h)	0.078	0.008	0.006
Full-time (35–40 h)	0.633	0.005	0.005
Overtime (40–49 h)	0.051	−0.002	0.001

Note: Probability of being in the different weekly working hours intervals and the effect on the probability of being in the different intervals. The effect is generated using the marginal effects from Fig. 3, with pooled standard errors in the last column. The procedure is described in Appendix A5 and Appendix A7.1.

6.2.1. Effect on weekly working hours

Fig. 3 shows the average reform effect across all ages and years of the reform on the weekly working hours distribution. The point estimates in Fig. 3 start above zero, indicating that the reform led to an increase in the LFP rate. Note that, by definition, the CCDF estimate at zero weekly working hours corresponds to the estimate on the extensive margin, i.e. 1.3 percentage points (with a standard error of 0.4 percentage points).

The estimates remain non-negative throughout the distribution, implying that the post-reform weekly hours of work distribution stochastically dominates the pre-reform distribution of weekly working hours, resulting in higher overall weekly hours of work after the reform. The estimated effect on the total labour supply (the extensive and intensive margin combined) is 0.33 weekly working hours (with a standard error of 0.18), pointing to a positive but near-zero effect on total labour supply.

This pattern becomes even clearer when examining the corresponding shifts in the probability of being within various intervals of the weekly working hours distribution (Table 5). While Table 5 shows a reduction in the proportion of non-participating individuals, the increases in the proportions of individuals across other weekly working hours intervals are not uniform. There are relatively larger increases in short and long part-time categories, consistent with the findings on the intensive margin from the OLS analysis.

6.2.2. Effect on labour earnings

The data on weekly hours of work shows significant bunching at full-time (37.5 h), with approximately 64% of workers observed working full-time before the reform. This limits the usefulness of distributional analysis based on weekly working hours. In contrast, labour earnings exhibit more favourable properties for such analysis. Fig. 4 shows the average effect over all ages and years of the reform across the earnings distribution. The figure shows the reform effects on the probability of having earnings above the level of the horizontal axis. Using the CCDF method, we find that the effect on the extensive margin is 1.3 percentage points, identical to the result obtained using the OLS method. The effect on total labour supply (combining the extensive and intensive margins) is also very close to the OLS estimate, indicating a decrease of EUR 670 with a standard error of EUR 596. This suggests a reduction in total labour supply, which contrasts with the results on total labour supply derived from weekly working hours. However, the effect on labour earnings is statistically insignificant.

Although the results provide no strong evidence of reform effects on average labour earnings, the CCDF estimates reveal distinct shifts in the shape of the earnings distribution. While the reform has drawn people into the labour force, the drop in the share not working is accompanied by an increase of similar magnitude over the interval (0,40000). The net result is that the share of individuals with earnings less than EUR 40,000 is the same before and after the reform. In the meantime, the reform shifts the rest of the earnings distribution to the left: there is an increase in the share of individuals with earnings between EUR 40,000 and EUR 60,000 and a reduction in the share earning between EUR 60,000 and EUR 85,000. In sum, the reform seems to have increased the relative share of workers aged 62–66 in the lower part of the earnings distribution (below EUR 60,000) and decreased the relative share of workers aged 62–66 in the upper part of the earnings distribution (above EUR 60,000 and up to EUR 85,000). The reform thus has a positive effect on the LFP rate and a negative effect on labour earnings, shifting the mass from the upper to the lower part of the earnings distribution. The corresponding shifts in the probability of being within various intervals of the labour earnings distribution are shown in Table 6.

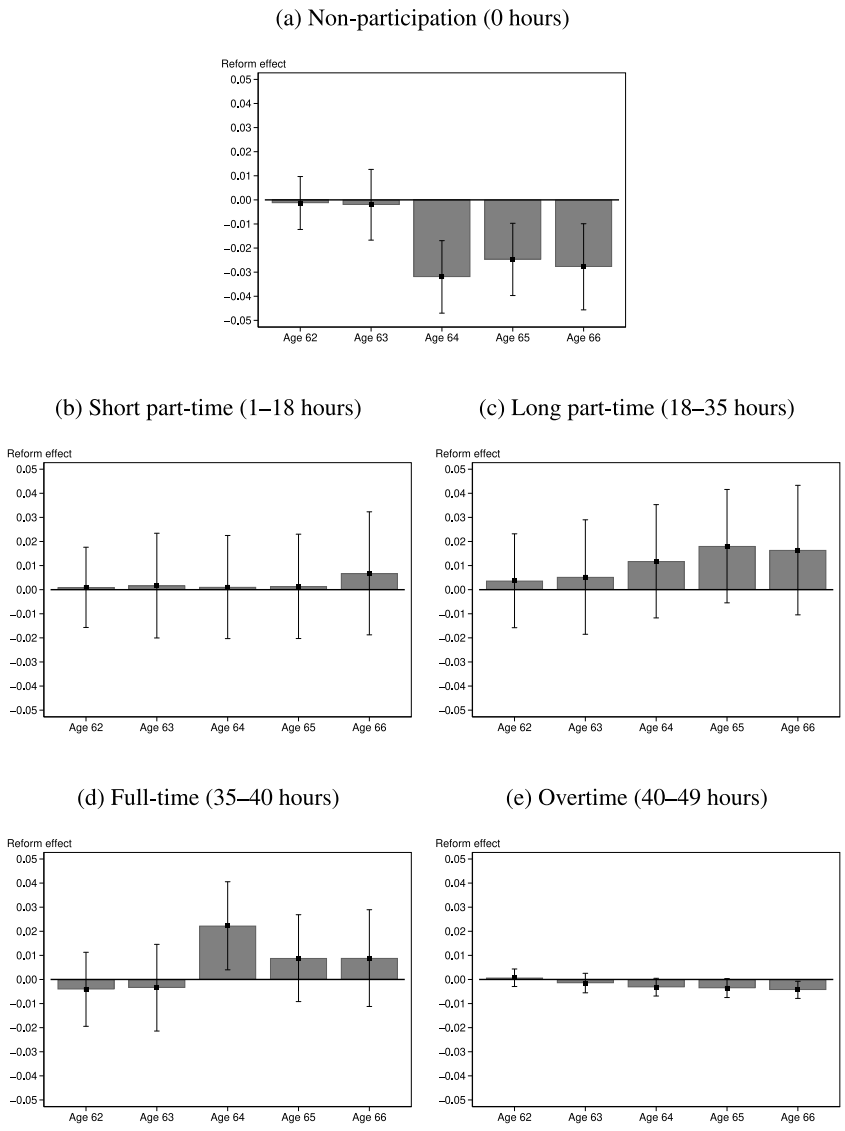


Fig. 5. Change in probability of being in selected weekly working hours intervals, by age.
Note: The effect on the probability of being in the different weekly working hours intervals, by age. The effects are generated using the marginal effects from Eq. (3), with the capped lines showing the 95% confidence intervals. The procedure for deriving effects and the associated standard errors is described in detail in Appendix A5 and Appendix A7.1.
Source: Authors' own calculations using data from Statistics Norway.

Table 6
Probability of being in selected labour earnings intervals.
Source: Authors' own calculations using data from Statistics Norway.

Labour earnings interval	Probability	Effect	
	(before reform)	Estimate	Standard error
Non-participation (EUR 0)	0.205	−0.013	0.004
EUR 0 – EUR 40,000	0.170	0.017	0.007
EUR 40,000 – EUR 60,000	0.244	0.023	0.008
EUR 60,000 – EUR 85,000	0.220	−0.019	0.007
> EUR 85,000	0.161	−0.008	0.004

Note: Probability of being in the different labour earnings intervals and the effect on the probability of being in the different intervals. The effect is generated using the marginal effects from Fig. 4, with pooled standard errors in the last column. The procedure is described in Appendix A5 and Appendix A8.1.

6.2.3. Age-specific effects

Our hypothesis is that a flexible retirement scheme allows older workers to reduce their work intensity according to their preferences, potentially increasing job satisfaction by better aligning their personal working capacity with their workload. Higher job satisfaction, in turn, could make it easier to continue working for longer periods. In other words, flexible claiming options may facilitate gradual retirement with a beneficial effect on the LFP rate.

This hypothesis implies specific patterns in the estimated reform effects. If the hypothesis holds, we would expect a negative effect on the intensive margin and a positive effect on the extensive margin. However, the extensive-margin effect is unlikely to appear before the intensive-margin effect. In other words, the effects on LFP are expected to vary by age and over time. We anticipate minimal or no impact on LFP at the new eligibility age, with increasing effects observed as individuals age. Similarly, we expect no immediate impact on LFP in the reform year, with measurable effects emerging as the reform becomes fully implemented.

To determine whether this is the case, we examine the age-specific effects of the reform. We begin by analysing the responses in weekly working hours. Consistent with our hypothesis, the increase in LFP appears to be somewhat delayed, becoming evident only after age 64.

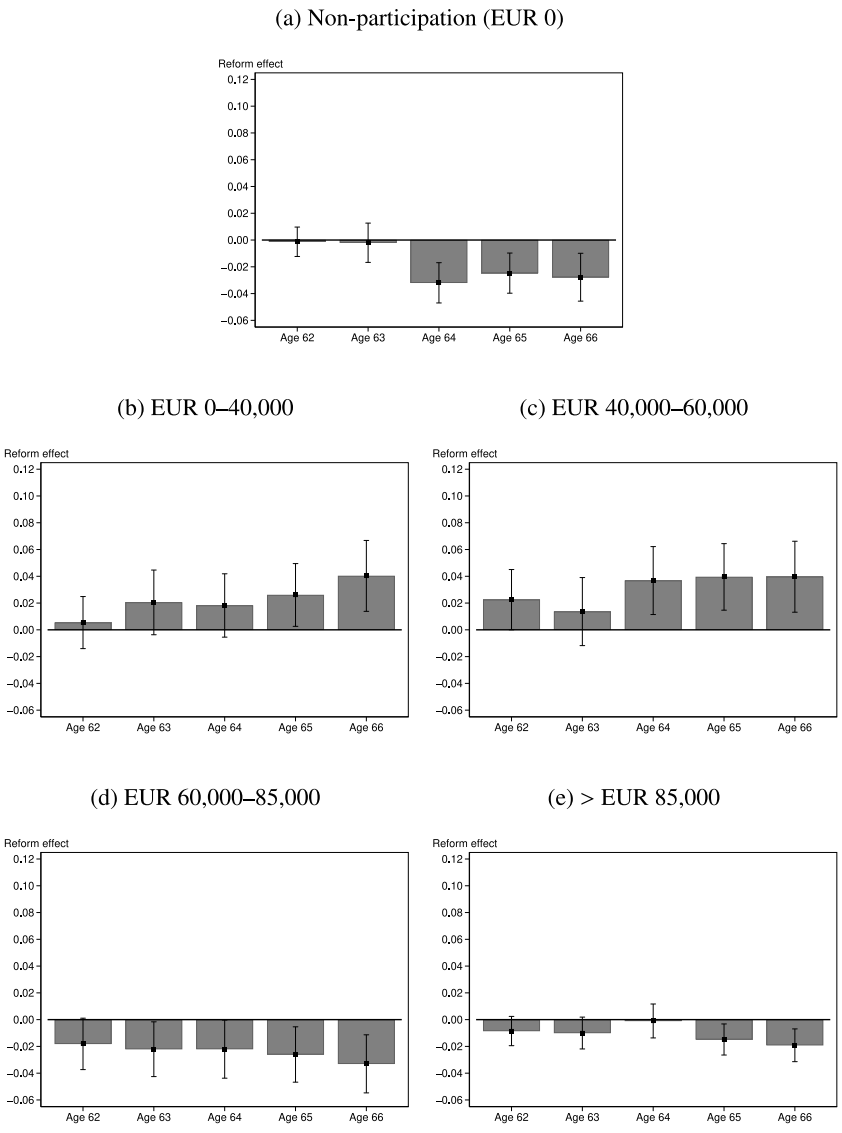


Fig. 6. Change in probability of being in selected earnings intervals, by age.
Note: The effect on the probability of being in the different labour earnings intervals, by age. The effects are generated using the marginal effects from Eq. (3), with the capped lines showing the 95% confidence intervals. The procedure for deriving effects and the associated standard errors is described in detail in Appendix A5.
Source: Authors' own calculations using data from Statistics Norway.

This can be seen in Fig. 5, which illustrates the estimated changes in the probabilities of being in different weekly working hours intervals by age.

The results indicate that the LFP rate is approximately 2–3 percentage points higher for individuals aged 64 to 66, with these changes being statistically significant. However, we generally do not observe any statistically significant effects on the intensive margin for any of the ages across the weekly working hours interval combinations.[17]

The results for labour earnings are illustrated in Fig. 6. The pattern for LFP is very similar to what we observed based on weekly hours of work: negligible effects for ages 62 and 63, and positive effects for those aged 64 through 66. However, the results on the intensive margin differ. Using labour earnings, we find clear indications of responses on the intensive margin across the entire age range. Specifically, the reform induced a reduction in labour earnings: compared with the pre-reform labour earnings distribution, there is a leftward shift in the post-reform labour earnings distribution, with fewer workers earning above EUR 60,000 and more workers earning in the EUR 40,000 to 60,000 interval.[18]

These observations of age dynamics – where changes on the intensive margin occur before changes on the extensive margin – align with our hypothesis, although they do not serve as direct tests. We believe the discrepancy between the estimated responses on the intensive margin using weekly working hours and those using labour earnings can be explained by the differences discussed earlier in Section 4.3. Namely, the data on the number of weekly working hours is noisier with substantial bunching, while labour earnings on its part also capture changes in work effort.

[17] The year-specific and age-specific effects on the distribution of weekly working hours using the CCDF approach are shown in Appendix A7.2.

[18] The year-specific and age-specific effects on the distribution of labour earnings using the CCDF approach are shown in Appendix A8.2.

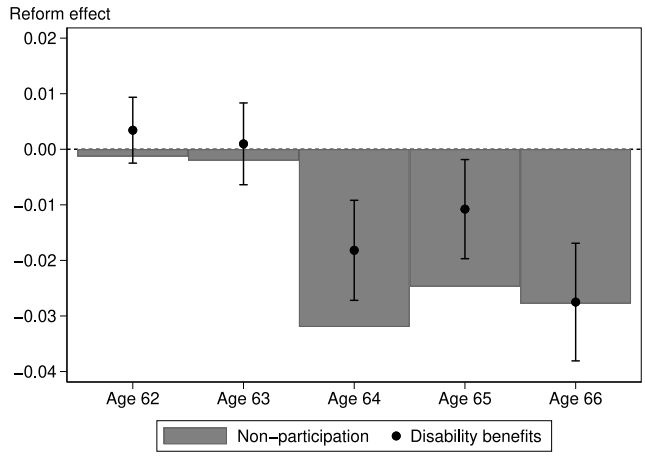


Fig. 7. Effect on disability benefit claiming, by age.
Note: OLS estimation results of the age-specific effect on likelihood of disability benefit claiming. Disability benefit claiming is defined as claiming any disability benefits. Control variables are pre-determined and include linear controls for education length, education length squared, log average annual pre-tax earnings from age 30 to age 59, and net liquid wealth at age 59. Point estimates and the associated standard errors are deferred to Appendix A6.
Source: Authors' own calculations using data from Statistics Norway.

7. Effect on disability benefit claiming

For both the weekly working hours and the labour earnings measures of the extensive margin labour supply we find a positive effect of the reform of 1.3 percentage points. The average effect is mostly driven by the effects at ages 64–66, an age range where there has historically been a high incidence of disability benefit claiming in Norway as a sort of health-contingent early retirement scheme (Hernæs et al., 2024). We estimate the effect of the reform on the likelihood of disability benefit claiming, and the age-specific results are shown in Fig. 7. As is evident from the figure, a large fraction of the positive effect on the extensive margin of labour supply at ages 64–66 is driven by a decrease in the incidence of disability benefit claiming at these ages (especially at age 66). To the extent that the gradual retirement process facilitates a longer working life by reducing the work burden, the effects on disability benefit claiming as shown in Fig. 7 aligns well with this hypothesis.

8. Conclusion

We investigate the impact of the 2011 Norwegian pension reform, which reduced the pension access age from 67 to 62 for some workers and made actuarial adjustments of periodic pension payments to keep the expected present value of benefits constant, regardless of the claiming age. We identify a group exposed only to the new flexible pension option, without any changes in the present value of the pension. This allowed us a rare opportunity to identify the impact of flexible pension access ages apart from changed work incentives.

Using an event-study setup that nests a difference-in-difference design, we find that, following the introduction of flexibility in the age of pension access, older workers increase their labour force participation by an average of 1.3 percentage points over the age range 62–66. The results on the number of weekly working hours indicate a reduction in the intensive margin of labour supply, which is further supported by using the labour earnings measure which indicates a decrease in the intensive margin of labour supply prior to the extensive margin response. In other words, older people reduce their workload and remain in the labour market for longer.

While we focus here on the supply side of the labour market, there is good reason to believe that there are demand-side restrictions in terms of accommodating 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 labour supply over several years over abrupt full retirement (Kantarci and van 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 by Takala and Väänänen (2016) suggest that older workers opt for gradual retirement to (i) devote more time to hobbies and family, and because (ii) they had been working full-time for too long. There seems to be a latent desire for phased retirement. Our results indicate that a reduced access age, holding the overall value of the pension constant, can facilitate such a transition to full retirement, implying a positive welfare effect. The dynamic effect, with increasing LFP over age, reinforces such an argument. This presents a case for regulatory policies aimed at making jobs more suitable for older and part-time workers.

CRediT authorship contribution statement

Erik Hernæs: Writing – original draft, Writing – review & editing, Data curation, Project administration, Visualization, Conceptualization, Investigation, Resources, Validation, Funding acquisition, Formal analysis, Software, Supervision, Methodology. **Zhiyang Jia:** Writing – review & editing, Supervision, Software, Formal analysis, Conceptualization, Writing – original draft, Project administration, Funding acquisition, Investigation, Visualization, Validation, Methodology, Resources, Data curation. **John Piggott:** Software, Project administration, Writing – review & editing, Methodology, Data curation, Writing – original draft, Validation, Formal analysis, Visualization, Supervision, Investigation, Resources, Funding acquisition, Conceptualization. **Trond Christian Vigtel:** Investigation, Software, Data curation, Validation, Visualization, Resources, Methodology, Conceptualization, Formal analysis, Project administration, Funding acquisition, Writing – original draft, Writing – review & editing, Supervision.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.labeco.2025.102771.

Data availability

The data that has been used is confidential.

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