



# The impact of pension reform on employment, retirement, and disability insurance claims

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## Abstract

We evaluate the 2011 comprehensive reform of Norwegian early retirement institutions using a parsimonious random utility choice model. Conditional on employment at age 60, we estimate a three-state conditional logit model to explain the realized labor market state at age 63 among the alternatives of employment, retirement, and disability program participation. The reform radically changed work incentives and/or pension access age for some (but not all) workers, such that the influence of economic incentives can be identified based on reform-generated variation only. We find that improved work incentives caused employment rates to rise considerably at the expense of early retirement and exit through disability insurance. Improved liquidity through a lower age to access own pension funds on actuarially neutral terms caused a small increase in employment and a large drop in disability program participation. Properly designed pension reforms thus need to take the interplay between old-age pension and disability insurance programs into account.

**Keywords** Pension reform · Disability insurance · Program substitution

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## 1 Introduction

Virtually, all industrialized countries are in the process of planning or implementing pension reform to enhance fiscal sustainability. These reforms typically target postponing retirement by raising the statutory retirement age, reducing the level of public pensions, or eliminating work disincentives such as earnings tests. These strategies all encourage workers to delay retirement and thus directly improve the balance between public pension expenditures and tax revenues. Some, however, have potential side effects that result in increased demand for alternative social insurance transfers. In particular, given that health impairments increase with age, disability insurance (DI) may substitute for a thriftier old-age pension system, undermining some of the fiscal improvements achieved through pension reform. For older persons, the distinction between being worn out and being disabled may be blurred, and many workers may be on the margins of DI eligibility. A properly designed and modernized pension system requires taking potential benefit substitution into account, such that the overall social insurance system is considered economically and socially sustainable.

In this article, we examine the substitution between continued work, old-age pensions, and disability insurance by evaluating a comprehensive Norwegian pension reform implemented in 2011. This reform radically changed work incentives and pension-access regulations for cohorts born after 1948, leaving earlier cohorts unaffected. The reform impacted groups of workers in different ways, depending on their pre-reform circumstances (Hernæs et al. 2016; Andersen et al. 2021). We identify groups of workers who gained and lost early access to their pension wealth and groups whose work incentives were and were not affected.

Our analysis is conditional on individuals being employed at age 60, and the outcome of interest is the chosen labor market state at age 63 among the alternatives of employment, early retirement, and DI program participation. We examine the reform-initiated changes in economic behavior through the lens of a parsimonious random utility three-state choice model to explain the realized state at age 63 by two key variables: (i) the financial reward associated with each state, as captured by the discounted predicted net income stream over the remaining lifetime, and (ii) a dummy variable indicating access to their own pension wealth already at age 62—5 years prior to the statutory retirement age.

As we detail below, the causal effects of these variables are identified based on reform-generated variation only, conditional on an otherwise stable economic environment experienced by those in the 1947–1949 birth cohorts. We find that the realized state at age 63 is heavily influenced by economic incentives. For example, point estimates suggest that a 10% increase in the expected remaining lifetime income associated with staying employed for five more years raises the probability of employment at age 63 by approximately 7–8 percentage points (12–13%) and reduces the probabilities of early retirement and disability program participation by 5–6 and 1–2 percentage points, respectively. Earlier access to own pension wealth (on actuarially neutral terms) has a small positive effect on employment and a large negative effect on the probability of DI-program

participation. We find that access to own pension funds reduces entry into DI programs after age 60 by as much as 20–25%.

Hence, policymakers who consider delaying the retirement of workers by raising the age at which potential retirees can access their own pension savings may have reason to think twice, at least if other (subsidized) exit routes from the labor market exist. There appears to be a “squeezed sausage effect”: If one exit route is closed, the demand for alternative routes increases. Access to their own pension wealth, on the other hand, makes it easier for some mature workers to continue in employment with reduced intensity because it can be combined with a partial pension. It is also a type of pension reform that may circumvent some of the tradeoffs related to equity and efficiency that otherwise plague pension reform processes and make them politically unmanageable (see Börsch-Supan et al. 2023).

Our study is not the first to report an interaction between DI programs and old-age pensions. The present analysis is inspired by our findings, reported in Hernæs et al. (2016), that the 2011 reform seemed to affect the transition into DI. Although in this earlier study we focused on the overall labor-supply responses to pension reform, we also provided reduced-form evidence of increased propensity to claim DI for a small group of workers who lost access to a generous early retirement. By contrast, Euwals et al. (2011) compare eligible and non-eligible workers for an attractive early retirement program in the Netherlands and find no substitution into disability programs.

The potential for program substitution is also documented elsewhere. Duggan et al. (2007) find that an increase in the age for full retirement in the US, which reduced the potential annual pension at lower ages, increased the DI program enrollment. In a later study, however, Coe and Haverstick (2010) find that the same reduction in the attractiveness of an old-age pension relative to the disability benefit increased the number of DI applications but not the actual number of recipients. Using data from Germany, Hanel (2012) reports a positive relationship between the inflow to DI and the size of the implicit tax on future earnings, indicating that better work incentives may reduce enrollment in DI programs. Borghans et al. (2014) exploit a cohort discontinuity in a Dutch disability insurance reform to estimate the effects of decreased DI generosity on the behavior of existing recipients. Their results imply that each euro cut in the DI benefit is offset by a 0.3 euro increase in social assistance and a 0.6 euro increase in earnings. The latter is interpreted as evidence of substantial remaining earnings capacity among DI claimants. In another study using Norwegian data, Kostøl and Mogstad (2014) show that a significant portion of DI recipients possess the ability to work, which can be effectively activated through financial incentives for employment. Specifically, they demonstrate that offering work incentives to DI recipients could boost their disposable income while cutting down program expenses. More generally, using US Department of Veterans Affairs Disability Compensation program, Autor and Duggan (2007) provide preliminary evidence suggesting that income effects are important in determining reduced labor supply among Vietnam War veterans.

We contribute to this literature by exploiting an exceptionally clean quasi-experiment with large reform-generated differences in incentives for adjacent birth cohorts. In relation to our earlier study (Hernæs et al. 2016), this work extends the

analysis to identify the circumstances under which pension reform entails substitution effects. In particular, we provide an economic interpretation of the identified responses using a simple choice model to more easily disentangle the roles of work incentives and liquidity constraints (access to own pension wealth).

Our paper is also related to a larger literature that studies how changes in one particular social insurance program affect the use of others. An early and influential contribution to this literature is Autor and Duggan (2003) who showed that expansion of the DI program in the US after the mid-1980s contributed to a significant reduction in the measured US unemployment rate. Consistent with this, Karlström et al. (2008) present evidence that stricter eligibility criteria for DI benefits in Sweden increased take-up of UI and sickness benefits. And whereas Lammers et al. (2013) show that the introduction of job search requirements for older UI claimants in the Netherlands increased the transition to sickness and DI benefits, Inderbitzin and Staubli (2016) show that extended UI benefits for older laid off workers in Austria led to a reduction in DI benefit claims.

In our case, precise identification of the reform effects requires assumptions about other local time trends (specifically, their absence). Although we cannot rule out unobserved trends that interfere with the reform's impacts, the extent of the shifts in observed behavior from the last unaffected to the first affected birth cohort renders the typical threats against identification almost negligible. Our interpretation of the reform effects—that is, the specific roles of incentives and pension access—is more open to discussion, relying on additional (simplifying) assumptions, to which we return below.

There is an important asymmetry between the states of employment and retirement, on the one hand, and DI program participation, on the other. Whereas the former two can quite safely be assumed to belong to the workers' choice sets, a DI benefit is only an option for persons with reduced work capacity due to ill health, and this must be verified by a physician and approved by the social security administration. Our data do not facilitate a precise identification of DI eligibility. We argue, however, that DI eligibility is a vague concept at the ages under consideration here, with ample scope for individual judgment and influence. In the cohorts examined, almost 40% of the population entered the DI program before they reached the statutory retirement age of 67. As a proxy for potential eligibility, we use data describing the workers' observed sick leave between the ages of 55 and 59.

In a recent study, also based on Norwegian register data, Autor et al. (2019) examine the impact of receiving a DI benefit, utilizing the variation in the leniency of appellate judges in treating reapplications. A finding of relevance for our paper is that a DI award causes a considerable decline in payments from non-DI transfer programs. Autor et al. (2019) did not specifically focus on substitution between DI and old-age pension. Rather, they examine alternative tax-financed transfers, such as social assistance, housing benefits, and vocational rehabilitation. The net impact of a DI allowance on total transfers received is estimated to be about 20 to 40% smaller than its direct impact.

Whereas Autor et al. (2019) examine how allowing a marginal applicant into the DI program affects household consumption, labor supply, and income from other social programs, we explore how changes in economic incentives and access to own

pension wealth impact the choice of seeking and subsequently receiving a disability benefit. Hence, both shed light on the issue of social program substitution, albeit from different angles.

We begin in Sect. 2 with a brief outline of Norway's pension and DI institutions. In Sect. 3, we describe the dataset and how we have adapted this for our study. In Sect. 4, we explain our empirical approach and report the results in Sect. 5. In Sect. 6, we conclude.

## 2 Institutional setting

### 2.1 Old-age pensions

The backbone of Norway's pension system is a mandatory, public pay-as-you-go defined benefit plan, the National Insurance Scheme (NIS) old-age pension. This provides basic pension coverage from the age of 67 until death for all individuals, subject only to residency requirements. In 2016, the NIS provided two-thirds of the total income for persons aged 75.

In addition to the NIS, all public sector workers and roughly half of private sector workers have access to a supplementary early retirement system, referred to by its acronym, AFP (*ÅvtaleFestet pensjon*). Before the 2011 reform, AFP offered a full NIS pension that could be accessed from age 62. The amount was based on projected earnings up to the standard retirement age of 67 and calculated *as if* the retiree had continued working until 67. Projected annual earnings were set to either the average of the last three earnings years before AFP withdrawal or the average of the best half of all earnings years since age 17, whichever is highest.

Notably, the pension was subject to a proportional earnings test, implying that continued employment after age 62 resulted in reduced lifetime pension entitlements. With a full pension, the earnings test became effective from the first euro earned, such that labor earnings constituting a certain percentage of the pre-retirement earnings level resulted in the same percentage cut in the annual pension. For example, if earnings after the age of 62 constituted 50% of previous earnings, the AFP pension was reduced by 50%. There was no deferral of earning-tested pension entitlements; hence, the benefit cut as a result of the test was never repaid. The AFP system thus essentially imposed very high rates of implicit taxation on continued work. Given the redistributive nature of the pre-reform NIS, with lower pension accrual for earnings above a threshold close to mean earnings, the implicit tax on continued work was particularly high for those with relatively low wages.<sup>1</sup>

<sup>1</sup> For the cohorts covered by our analysis, pension points were accumulated in full for earnings up to 6 basic amounts (BA), then reduced to a third of annual earnings for earnings between 6 and 12 BA, and to zero for earnings above 12 BA. The basic amount is an entity used in the Norwegian pension system which is adjusted each year roughly in accordance with annual wage growth. It is currently equal to about 10,000 euros. The actual influence of earnings obtained after age 60 on the subsequent pension level could vary considerably across individuals due to the so-called 20-year and 40-year rules, the former stating that only the individual's 20 best earning year would enter into the pension formula, and the latter that 40 years of social security coverage, mainly defined as residence after age 16, would be required to obtain a full pension. The marginal effect of yet another earnings year at high age thus

The NIS and the private sector AFP reforms in 2011 had several elements.<sup>2</sup> The NIS access age was lowered to align with the age of AFP access; however, in both post-reform systems, annual benefits are adjusted on actuarially neutral terms. NIS benefits are adjusted to generate a constant estimated present value of the expected benefit stream, anchored to the standard retirement age of 67. The AFP was transformed to become a supplementary pension independent of continued labor earnings. For the affected workers, the reform implied a complete decoupling of the work/retirement decision from the pension-management decision. As a result, work incentives were drastically improved; for the average worker, the hourly take-home wage (after tax and earning-test reductions) was approximately doubled (Hernæs et al. 2016).

The reform also had implications for early access to pension savings. Before the reform, only those covered by AFP had access to any form of pension income before the age of 67. Following the reform, anyone entitled to an NIS or AFP pension at least as high as that guaranteed at age 67 was entitled to start drawing at 62. In the new system, AFP entitlements also depend on the right to early withdrawal of the NIS pension. As a result, workers with a contribution history that was too low to ensure an annual pension above the minimum level lost their entitlement to AFP and could not start drawing on their own pension savings. By contrast, workers with a sufficient contribution history had access to their own pension wealth on actuarially neutral terms from age 62, regardless of their AFP entitlement.

## 2.2 Disability insurance

Disability benefits are available if a person's health problems mean that their ability to earn an income has been reduced by at least 50%. This loss must be certified by a physician and then approved by the social security administration. It is granted for those aged between 18 and 67; at 67, a pension is paid instead. For new DI entrants aged between 62 and 67, there is also a condition that they have an annual income in the three preceding years of at least 1 BA (the "basic amount" metric is used extensively in redistributive policies in Norway and is worth about 1/6 of average full-time earnings, approximately €10,000).

For the cohorts included in our sample, the DI benefit is calculated as the NIS old-age pension based on the earnings projections described for AFP above.<sup>3</sup> The full annual DI benefit would typically meet a net-of-tax replacement ratio close to 66% of the person's previous labor earnings up to an earnings threshold approximately corresponding to average full-time earnings. There is also a minimum

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Footnote 1 (continued)

depended on the whole earnings history and would most often be unimportant, since most persons would meet the 40-year residence condition and annual earnings would most often not add to the 20 best years.

<sup>2</sup> The public sector was mostly unaffected; in this paper, we exclude public sector employees from the analysis. Hernæs et al. (2016) provide a more thorough description of the pension reform.

<sup>3</sup> The DI program was reformed in 2015, such that the benefits are now calculated directly as 66% of the earnings level during the best three of the last 5 years prior to disablement (up to a threshold). On average, the reform did not change the net-of-tax replacement ratio in the DI program.

level, and, as a result, the annual benefit before tax is almost always in the range of 38–66% of previous earnings.

DI can be temporary or permanent, but for the age groups considered here, the difference between them is more a matter of timing than content. A temporary disability benefit is typically followed by a permanent benefit, especially at later ages, or by a transfer to old-age pension at age 67.

Disability can be full or partial, based on the assessed remaining earnings capacity, and can be combined with earnings, but it is subject to an earnings test. For the cohorts covered in our analysis, the earnings test applied to earnings exceeding 1 BA. For earnings above that level, the disability benefit was reduced by a fraction equal to the excess earnings as a share of previous earnings. Earnings above the BA threshold also triggered a reassessment of the degree of disability.<sup>4</sup>

Disability benefits cannot be combined with AFP. Prior to the 2011 reform, the overall benefit levels were roughly the same in the AFP and DI programs. After the reform, DI benefits normally provide a somewhat higher income than AFP for those who do not work at all but at the cost of a strict earnings test.

### 3 Data and empirical overview

#### 3.1 Data sources

The main empirical basis for our analysis is large sets of administrative register data leased from Statistics Norway and linked by unique encrypted personal identification numbers. The register data on individuals are supplemented by enterprise data, which includes a listing of all private firms offering AFP.

The individual register data cover Norway's whole population. For our purposes, the most important information is annual earnings and other income, taxes paid, and benefits received. We identify the employer and tenure for all employees at age 60. This complete data on pension-generating earnings back to 1967 also facilitates accurate computation of pension entitlements from NIS and AFP and under pre- and post-reform rules. Other individual characteristics observed at age 60 include gender, marital status, educational attainment, and liquid financial wealth (bank deposits). In addition, we observe total sick leave taken between the ages of 55 and 59.

#### 3.2 Sample

In our analysis, we use pre-reform (1945–1947) and post-reform (1949–1951) birth cohorts. The 1948 cohort is a transitional cohort, experiencing some (but not all) reform impacts, and the cohorts born from 1952 onward were impacted by the 2015 reform of the DI program (see Alne 2018). In our case, using the cohorts born between 1945 and 1951 has the important advantage that these cohorts faced similar

<sup>4</sup> In 2015, the earnings threshold was reduced to 0.4 BA and the reassessment requirement dropped. A more generous threshold was preserved for persons who had received a disability benefit before 2015.

economic environments at around age 63 (our outcome year), with unemployment rates among people over 55 (as measured by the Labor Force Sample Survey) varying between 1% (in 2007 and 2008) and 1.4% (in 2014). Persons born either before or after these cohorts faced somewhat higher unemployment at this age (1.7% in both 2005 and 2015).

We further restrict the sample to persons employed in the private sector at age 60 (defined as having earnings exceeding an amount corresponding to 1 BA (approximately 1/6 of average full-time earnings) and who were not receiving DI benefits at the time. Given these restrictions, our sample comprises the complete population as captured in administrative registers.

### 3.3 Classification at age 60

The impact of the reform on private sector workers differed depending on their pension entitlements. Therefore, in our comparison of pre- and post-reform cohorts, we cross-classify all cohorts by the criteria for claiming a pre-reform AFP pension and the criteria for claiming the post-reform NIS pension, with access to both systems from age 62. The four groups, shown in Table 1 below, correspond to the classification in Hernæs et al. (2016), with the exception of public sector employees, who are excluded here. Group A was entitled to AFP benefits before and after the reform, but the reform removed the earnings test and thus radically improved work incentives for workers belonging to the post-reform cohorts. Group B had no AFP entitlement either before or after the reform, but the post-reform cohorts in this group had access to actuarially adjusted NIS benefits from age 62. Group C enjoyed AFP entitlements before the reform but lost these as a result of the reform because contributions were too thin to meet minimum pension requirements. Finally, Group D did not have AFP access before or after the reform, and following the reform, it did not meet the criteria for access to the NIS pension at age 62.

### 3.4 Outcome at age 63

We capture the impact of the reform by examining the observed state in the year each person reached the age of 63, which is the first full calendar year after (potentially) having become entitled to AFP. At this age, income can come from three different sources: work, old-age pension, and/or DI. The latter is typically granted after a lengthy sick leave and can be temporary or permanent. We do not distinguish between temporary and permanent DI, however, since the probability of returning to employment from temporary disability after age 63 is very low.<sup>5</sup>

Table 2 shows how members of the different birth cohorts combined income types at age 63 and our aggregation of age-63 outcomes into three states. Notably, the fraction of each cohort in employment increased sharply after the reform, from 65.7% (59.2+6.5) for the 1947 cohort to 75.8% (38.6+37.2) for the 1949 cohort. It is especially clear that

<sup>5</sup> Among those who received some form of DI benefits at age 63, 95% also did so at age 64.

**Table 1** Classification at age 60 of pension entitlements from age 62

Group	Access to pension wealth at age 62		Number of observations			Reform effect
	1945–1947	1949–1951	1945–1947	1949–1951	Total	
A	Yes, with earnings test (AFP)	Yes, without earnings test (AFP and NIS)	25,247	26,385	51,632	Radically improved work incentives No change in early access
B	No	Yes, without earnings test (NIS)	18,200	18,693	36,893	No (or small) change in work incentives Obtained early access
C	Yes, with earnings test (AFP)	No	3564	2631	6195	Radically improved work incentives Lost early access
D	No	No	7513	6806	14,319	No change in either work incentives or access
All			54,254	54,515	109,039	

AFP (AvtaleFestet pensjon) is the supplementary pension system covering roughly half of all private sector workers. NIS (National Insurance Scheme) is the public pay-as-you-go defined old-age pension plan covering all residents of Norway

**Table 2** Classification by work, disability, and pension at age 63, working in private sector at age 60 (by %)

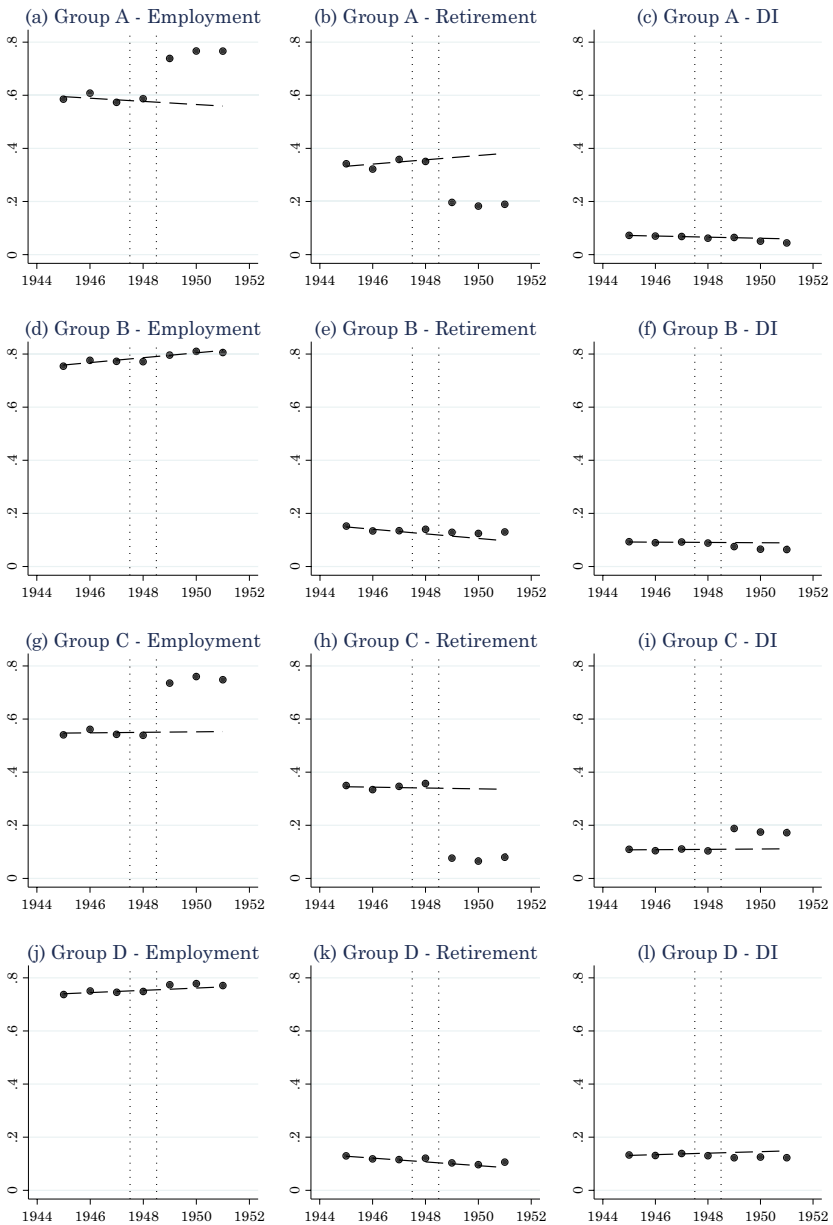
	Number of observations	Employment (%)		Retirement (%)		Disability insurance (%)	
		Labor earnings only	Labor earnings and pension	Only pension income	No income	Only DI	DI and work
1945	16,940	58.9	6.6	18.4	7.1	6.0	3.1
1946	19,180	59.7	7.9	17.2	6.5	5.5	3.2
1947	18,404	59.2	6.5	18.5	7.1	6.0	2.8
1949	18,399	38.6	37.2	12.3	3.7	5.4	2.8
1950	18,128	36.4	41.4	12.2	2.8	4.7	2.5
1951	17,988	34.6	42.9	13.3	2.6	4.3	2.3
All	109,039	47.9	23.7	15.3	5.0	5.3	2.8

This table includes persons who at age 60 had at least one BA (“basic amount,” approximately 10,000 euro) annual earnings and who were not receiving disability benefits above 0.2 BA. The classification threshold is an annual amount of at least 1 BA. Those in disability states include some who also receive an old-age pension during the year

having a combination of employment and pension income became much more common (from 6.5 to 37.2%) as the new NIS (public old-age pension) became accessible at 62. Before the reform, this was, for the most part, only possible via AFP and then only with a confiscatory earnings test.

The three outcome states are defined as follows: Persons receiving a disability pension are defined as DI claimants, regardless of the amount of the disability pension and regardless of any other pension or earnings. If a person does not receive a disability pension and labor earnings exceed 1 BA, he/she is defined as employed. If a person does not receive a disability pension and labor earnings do not exceed 1 BA, he/she is classified as retired. Hence, the state of retirement is not directly tied to pension claims (which are now frequently received in combination with employment earnings) but to the absence of employment and DI.

Figure 1 offers a more detailed picture of age-63 outcomes for the four groups for the pre-reform (1945–1947) and post-reform (1949–1951) cohorts. We note, in particular, the large employment increases in Group A and Group C; in Group A, these are matched by a corresponding decline in retirement, and in Group C, by an even larger drop in retirement accompanied by an increase in DI claims. In Group B, we note that there are only small (if any) reform-generated changes in employment and perhaps a shift from participation in the DI program to retirement by means of their own pension funds.



**Fig. 1** Activity at age 63 by group classification at age 60, for cohorts 1945–1951. Note: The dashed lines are linear extrapolations (OLS) of the 1945–1947 observations

## 4 Empirical approach

The central idea of this study is that there are those who, at a mature age, are in a “gray zone” between work, retirement, and disability. They may or may not be eligible for DI benefits, but they can influence their eligibility status through their own behavior. For example, individuals may be able to continue working if they reduce their workload by cutting hours and earnings, yet they may qualify for full DI. Their actual choice may depend on incentives to work, whether or not they can start drawing on their old-age pension, the costs associated with a DI application, and its expected outcome.

As a result of the pension reform in 2011, some groups experienced a higher economic return to work because they no longer lost a potential supplementary pension. Other groups gained (or lost) the option to start drawing their pension wealth from an earlier age.

### 4.1 Model

The main purpose of our empirical analysis is to identify the extent to which the observed patterns of employment, retirement, and DI-program participation at age 63 can be explained by two key variables (in addition to a vector of individual characteristics): (i) the economic consequence associated with each state, as captured by the expected net present value of the income stream over remaining lifetime, and (ii) access to own pension wealth from age 63. The first varies by outcome, whereas the latter varies by individual only. Therefore, we base our analysis on a three-state conditional logit model (McFadden 1974), employing the amount of sick leave taken over the preceding 5 years as a proxy for potential DI eligibility.

The methodological basis for our choice model is a set of utility functions that describe person  $i$ 's evaluation of state  $j$  as a function of state-specific and individual characteristics and a random “taste shifter”:

$$U_{ij} = X_{ij}\beta + Z_i\alpha_j + \varepsilon_{ij} = U_{ij}^* + \varepsilon_{ij} \text{ for } j = 1, 2, 3, \quad (1)$$

where  $j=1$  denotes the state of work,  $j=2$  denotes retirement without DI,  $j=3$  denotes DI,  $X_{ij}$  indicates individual  $i$ 's economic incentives by state, and  $Z_i$  denotes individual characteristics. The content of these covariate vectors is described in more detail below.

With standard assumptions on the error term (type 1 extreme value distribution), the outcome probabilities are modeled as follows:

$$\Pr(Y_i = j) = \frac{\exp(U_{ij}^*)}{\sum_{j=1}^3 \exp(U_{ij}^*)} \quad j = 1, 2, 3. \quad (2)$$

This specification implies that the relative choice probabilities for any two alternatives are modeled as independent of the attractiveness of the third alternative. The

model also relies on the assumption that each person is free to choose the utility-maximizing state. None of these assumptions is likely to be fully satisfied. In particular, the DI alternative may be closed to those without health problems. Formally, we can think of this as implying very low utility in the DI state, which will then be captured by variables revealing few absences for illness in their past. We could also interpret the results in a more reduced form setting since the realized labor-market state results from a combination of preferences and opportunities.

## 4.2 Identification of incentive effects

As indicated above, we specify economic incentives parsimoniously by assuming that they can be appropriately represented by a single state-specific variable, namely, the discounted value of the expected remaining lifetime net income. This income is calculated for pre- and post-reform pension and tax rules under the following simplifying assumptions, based on the choice of labor market state at age 63<sup>6</sup>:

- (i) The employment alternative implies continuing work for 5 years, at ages 63–67, and then full retirement.
- (ii) The disability insurance alternative implies DI benefits for 5 years, at ages 63–67, and then full retirement.
- (iii) The retirement alternative implies full retirement from age 63 (no return to employment).
- (iv) In all cases, any accessible pension is claimed when it is available.<sup>7</sup>
- (v) The expected lifetime is 82 years for everyone, approximately corresponding to the average life expectancy reported by Statistics Norway for persons aged 60 and belonging to the studied cohorts.
- (vi) The (real) discount rate is 2%.<sup>8</sup>

Economic incentives are, of course, not randomly assigned; persons whose expected state-specific income streams differ are also likely to differ systematically along other dimensions, such as health status, motivation, job characteristics, and valuation of leisure. To facilitate estimation of the causal relationship between incentives and choice of status at age 63, we isolate the random assignment-like variation generated by the reform by including in the X-vector not only the actually predicted state-specific income streams but also the corresponding hypothetical

<sup>6</sup> The expected net income streams are calculated based on a complete account of earnings histories up to and including age 60, assuming that the employment alternative represents a continuation of earnings at age 60. Pre-reform cohorts are taxed according to 2010 tax rules, whereas post-reform cohorts are taxed according to 2011 rules. All incomes are inflated to 2014 values, using the adjustment factor embedded in the Norwegian pension system. They are translated to euro value by using the average exchange rate in 2014; that is, €1 = NOK 8.35.

<sup>7</sup> Those who did not qualify for the post-reform pension at 62 were assumed to claim as soon as they qualified, and by no later than age 67, when all could claim.

<sup>8</sup> The specific choice of discount rate is not critical for our results, at least not as long as it lies within the [0,4] interval. We elaborate on this below.

income streams that would have applied under pre- and post-reform rules, respectively. The basic idea is that whereas the spurious (non-causal) correlations between these hypothetical income streams and choice behavior should be the same before and after the reform, the causal correlation should shift toward the income stream actually applying. We thus specify the X-vector as follows:

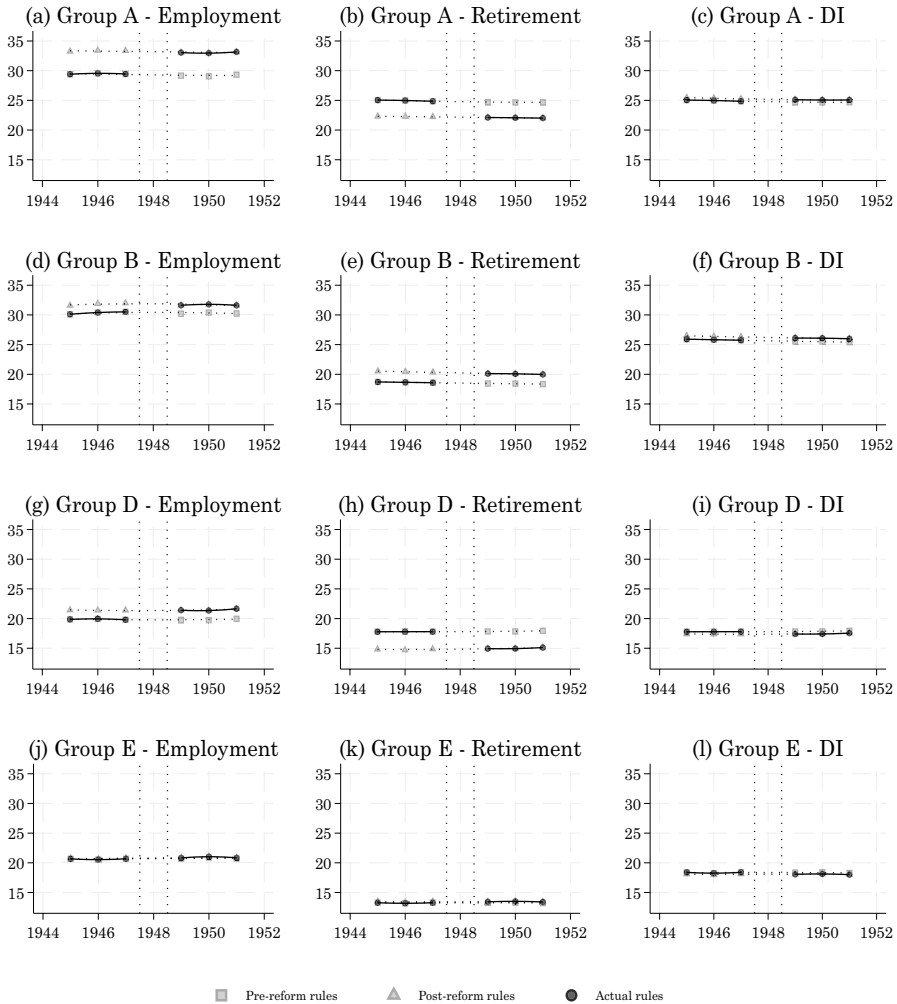
$$X_{ij} = \log(\text{NPI}_{ij}^{\text{Old}}), \log(\text{NPI}_{ij}^{\text{New}}), \log(\text{NPI}_{ij}^{\text{Actual}}), \text{ with} \\ \log(\text{NPI}_{ij}^{\text{Actual}}) = (1 - \text{REFORM}) \times \log(\text{NPI}_{ij}^{\text{Old}}) + \text{REFORM} \times \log(\text{NPI}_{ij}^{\text{New}}), \quad (3)$$

where the NPI variables are the expected state-specific net present values of the income streams calculated according to old (pre-reform) and new (post-reform) pension and tax rules, respectively, and REFORM is an indicator variable equal to one for the 1949–1951 birth cohorts (and zero for the 1945–1947 cohorts). It is then the coefficient associated with  $\text{NPI}_{ij}^{\text{Actual}}$  that captures the causal incentive effect.

Figure 2 shows how the average expected income streams vary across the four groups in our data and how these were affected by the reform. For example, starting in the upper left panel, we note that for the pre-reform cohorts belonging to Group A, the expected net remaining lifetime income if the work alternative is chosen is, on average, equal to approximately €30,000 per year. Had these cohorts been exposed to post-reform pension and tax rules, the work alternative would have been much more profitable; the expected remaining lifetime net income would have been 13% higher on average, that is, approximately €34,000 per year. For the post-reform cohorts, the pattern is exactly the same, with the important difference that whereas the pre-reform incentives were the actual incentives for the 1945–1947 cohorts, the post-reform incentives were the actual incentives for the 1949–1951 cohorts. As the overall patterns described in Fig. 2 show, for Groups A and C, employment became much more economically attractive after the reform, and the retirement alternative became less attractive. For the members of Group B, all alternatives were more generously rewarded after the reform, whereas for Group D, there were only minor changes, mainly tax induced.

In our choice model, it is the relative economic rewards that matter; that is, the *differences* between the  $\log(\text{NPI})$  derived from the alternative states. While there are substantial changes in average incentives due to the pension reform, there is little within-group variation in these changes. To the extent that our model allows for group- and cohort-fixed effects (or interactions between the two), this creates a potential identification problem, which we illustrate in Figure A1 in the Online Appendix; we show how the relative economic rewards changed for a 15% random sample of the data. It is clear that economic incentives shifted almost in parallel for all members of the respective groups, particularly within Groups A and B, making it difficult to disentangle incentives from group-by-cohort effects.

As we explain in more detail in the next section, our solution is to estimate alternative models, separated by differences in the way we control for groups and cohorts. Our preferred specification is a model that includes a full set of group and cohort-fixed effects, with the important exception that we use a joint fixed effect for the last pre-reform (1947) and the first post-reform (1949) cohorts. Provided that any other time trends in individual outcomes (i.e., trends unrelated to the pension



**Fig. 2** Predicted annualized lifetime net incomes (age 63–82) by state at age 63 and by group and cohort, 1945–1951. Note: The figure shows predicted annualized lifetime net incomes calculated for all cohorts according to both pre-reform and post-reform pension regimes. The shaded triangles and squares show the counterfactuals (after-reform-incentives for the pre-reform cohorts and before-reform-incentives for the post reform cohorts), whereas the black dots show the predictions actually applying. Annualized lifetime incomes are measured in €1000, inflated to 2014 value, and converted to euros using the exchange rate in 2014 (€1 = NOK 8.35) and divided by 21 to approximate an average annual amount

reform) are negligible over this short period, the estimated effect of  $NPI_{ij}^{Actual}$  will trace out the causal influences of economic incentives on the actual realization of labor market states at age 63. The developments observed over the last pre-reform and first post-reform cohorts shown in Fig. 1 largely confirm this assumption. The changes in the labor-market state at age 63 were small before and after the reform and almost invisible relative to the jumps that occurred between the 1947 and 1949

cohorts. There is, however, some indication of a post-reform trend toward lower levels of participation in the DI program. While this may represent a violation of the no-trend assumption, it may also represent a lagged reform effect. Since it may take some time to be admitted into DI, it is probable that the reform effects have become stronger over time.

### 4.3 Identification of access effects

Another important covariate reflects a person having access to their own pension wealth at age 63. In contrast to DI entitlements, we are able to precisely identify who has this option before and after the reform. This is separable from economic incentives since it reflects the option of claiming a pension early but, after the reform, is subject to actuarial adjustment. Without liquidity constraints, this should not matter for perfectly rational agents with foresight. Liquidity constraints may be important, however, and it is extremely uncommon to borrow against future pension payments. The variation in access reflected in our data emerges from two sources:

- (i) Workers belonging to Group B gained access to early retirement as a result of the reform; hence, workers belonging to this group did not have access at age 63 if they were born in the years 1945–1947 but had access if they were born from 1949 to 1951.
- (ii) Workers belonging to Group C lost access to early retirement as a result of the reform; hence, workers belonging to this group had access at age 63 if they were born in the period 1945–1947 but not if they were born between 1949 and 1951.

Consequently, we specify pension access as a dummy variable, which takes the value of one before and zero after the reform for those who lost access (Group C) and zero before and one after the reform for those who gained access (Group B). For Group A, it is always one, and for Group D, it is always zero.

Identification of a single “access effect” hinges on the assumptions that members of Group B and Group C respond similarly and that policies introducing and abolishing access have symmetric effects. This will then essentially be identified by the members of Group B, as they were subject to changes in access, with only minor changes in economic incentives (see Appendix Figure A1, Panels C and D), whereas the members of Group C were subject to changes in access and incentives. These assumptions can be questioned given the differences in the composition of Groups B and C, particularly in terms of gender.

### 4.4 Additional control variables

In the regressions, we include the following individual covariates observed at age 60: gender, marital status (married or partnered, divorced, never married), educational attainment (compulsory school only, high school, bachelor’s degree, master’s degree or above), average earnings between ages 30 and 59, and size of bank

deposits at age 60. As explained above, we also include past sick leave, specified as the number of years between ages 55 and 59 with paid sick leave (none, 1–2, 3–4, all five). Table A1 in the Online Appendix presents descriptive statistics for all groups and for both pre- and post-reform cohorts. The most important point to note is the large difference in the gender composition of Groups A and B (with 15–20% women) and Groups C and D (with more than 80% women). This is related to the fact that most women in these generations had long periods outside the labor market and often worked part-time, making them ineligible for the early retirement options defining Groups A and B.

## 5 Estimation results

We start by estimating our model for Group A alone. This is convenient because the members of the group were subjected to changes in economic incentives but not to changes in pension access; hence, they provide the cleanest basis for the identification of incentive effects. We estimate three different models, distinguished by how they include incentives and/or cohort-fixed effects. The main results are presented in Table 3, with a selection of estimated cohort effects shown in Fig. 3. At the bottom of Table 3, we also provide two metrics describing the models' fit to the data. "Prediction accuracy" compares the predicted and observed frequencies across outcome states and groups before and after the reform. This metric is by construction equal to 100% when cohort dummies are included in the models for Groups A and B (M1, M4, and M5) and when cohort-by-group dummies are included in the model for all (M6).<sup>9</sup> In addition to this aggregate measure, "Model fit" compares *individual* predicted probabilities from the model to predictions using only sample averages (see note to Table 3).

Starting with Group A, M2 includes the incentive variables (pre-reform, post-reform, and actual state-specific predicted income streams) but no cohort-fixed effects. M1 represents the opposite approach; it contains no incentive variables but separate dummy variables for each cohort. Looking at the resultant log-likelihood values and metrics for model fit, the incentive model (M2) clearly explains the data better than the cohort-fixed-effects model (M1). M3 combines incentive and cohort dummy variables, but with the important restriction that cohort effects are the same for the 1947 and 1949 cohorts.<sup>10</sup> In essence, we are assuming away any spurious trends across these close cohorts. We could say that, whereas M1 is the most suitable model for obtaining rough estimates of the overall reform effects in Group A (by comparing cohort effects for the 1949 and 1947 cohorts), M3 offers a more useful

<sup>9</sup> Although the cohort effects are restricted to be the same for the 1947 and 1949 cohorts in M5, the inclusion of access effects implies full flexibility in terms of cohort effects also for this model, such that prediction accuracy is 100% by construction.

<sup>10</sup> Without this restriction, it is difficult to disentangle incentives and cohort effects, as the incentive changes in Group A are almost perfectly parallel throughout the incentive distribution (see Appendix Figure A1).

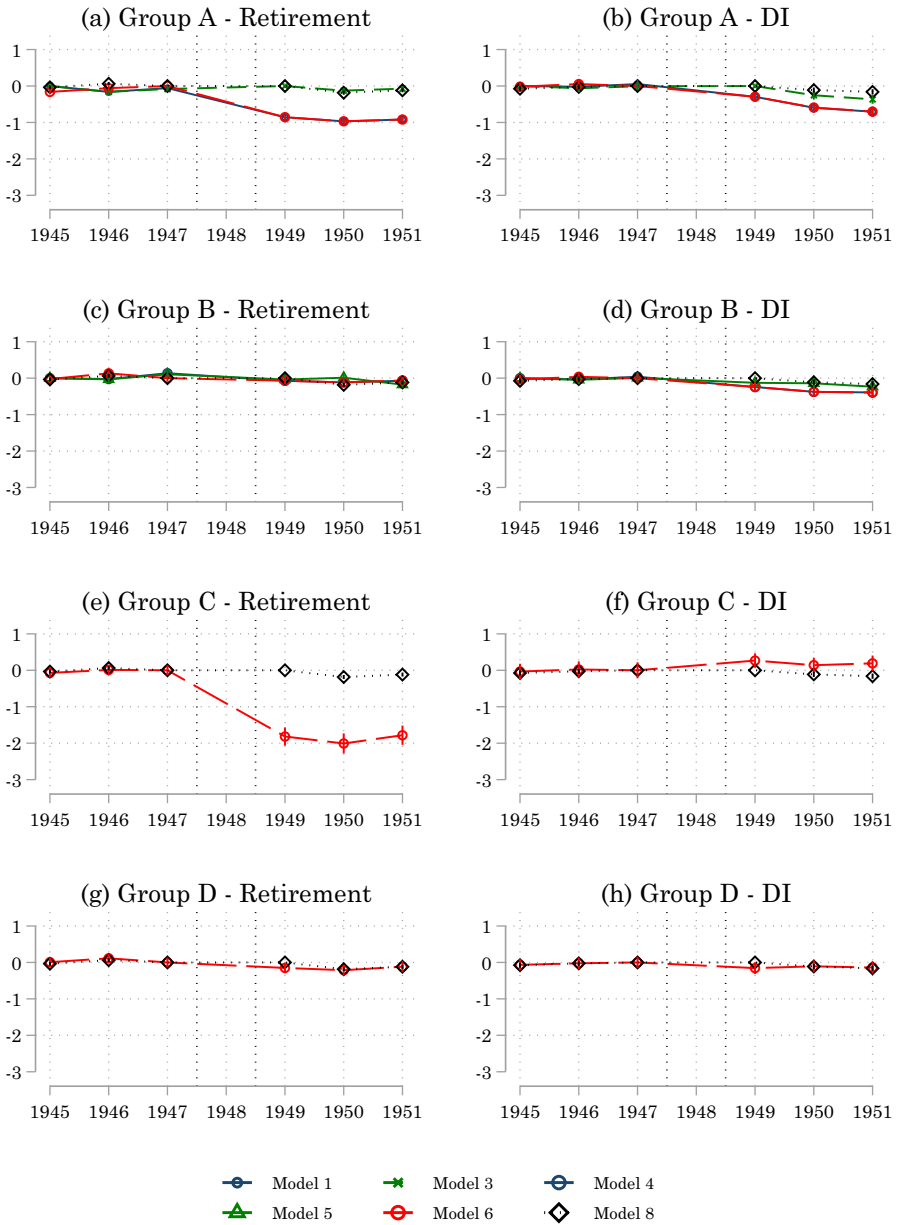
Table 3 Estimation results

	Group A only (M1–M3)			Group B only (M4–M5)			All groups (M6–M8)		
	M1	M2	M3	M4	M5	M6	M7	M8	
Predicted state income (NPI actual)	–	3.610 (0.088)	3.625 (0.148)	–	1.539 (1.366)	–	3.750 (0.083)	3.314 (0.119)	
Retirement	–	–	–	–	–0.148 (0.079)	–	–0.049 (0.026)	0.013 (0.035)	
Access to own pension wealth	–	–	–	–	–	–	–	–	
DI program	–	–	–	–	–0.198 (0.077)	–	–0.258 (0.026)	–0.272 (0.037)	
Access to own pension wealth	–	–	–	–	–	–	–	–	
Cohort-fixed effects	Yes	No	No	Yes	No	No	No	No	
Cohort-fixed effects with 1947–1949	No	No	Yes	No	Yes	No	No	Yes	
Group-by-cohort-fixed effects	–	–	–	–	–	Yes	No	No	
Group-fixed effects	–	–	–	–	–	No	No	Yes	
Log likelihood	–38,900.8	–38,573.2	–38,531.0	–23,542.3	–23,368.6	–77,732.7	–77,330.4	–77,238.9	

Table 3 (continued)

	Group A only (M1–M3)			Group B only (M4–M5)			All groups (M6–M8)		
	M1	M2	M3	M4	M5	M6	M7	M8	
Prediction accuracy	100%	99.2%	99.8%	100%	100%	100%	98.9%	99.1%	
Model fit	0.044	0.055	0.056	0.022	0.029	0.053	0.060	0.061	
# Observations	51,632	51,632	51,632	36,893	36,893	109,039	109,039	109,039	

All models include the variables listed in Table A1 related to gender, marital status, education, past earnings, sickness absence, and bank deposits as individual covariates. M2, M3, M7, and M8 also include pre-reform and post-reform calculated NPI as state-specific covariates. “Prediction accuracy” is one minus the sum of absolute values of the differences between the actual outcome frequencies for each group before and after the reform and the frequencies predicted by the respective models, divided by 2. “Model fit” is a measure aggregated from individual choices, calculated for each model by adding up the estimated probabilities corresponding to the state actually chosen for each individual. This is then compared to a corresponding measure based on setting the individual probabilities equal to the sample fractions across the three states. For each individual  $i$ , let  $p_i$  denote the predicted probability for the chosen alternative using a model. Let  $P_{i0}$  denote the corresponding predicted probability from a “null” model containing a constant term only (this corresponds to the sample mean of each alternative). Finally, let  $P$  and  $P_0$  denote the sample mean of  $p_i$  and  $P_{i0}$ . Model fit is then  $M = (P - P_0) / (1 - P_0)$  and can be interpreted as the fraction of the difference between the “null model” and the “perfect” prediction model that can be explained by the model in question. It varies between zero (no improvement relative to the naïve model) and unity (perfect predictions for everyone). Standard errors are in parentheses.



**Fig. 3** Estimated cohort effects (underlying time trends), by group and choice of model. Note: The cohort-fixed effects are normalized on the 1945 cohort. See Table 3 for a description of the models

economic interpretation of these effects provided that the added stability assumption for the 1947–1949 cohorts is valid.

Whereas Group A was subject to substantial change in incentives but no change in access, Group B was subject to changes in access but almost no change in incentives (since access was provided on actuarially neutral terms). Group B is, therefore, particularly useful for identifying access effects; see Columns M4 and M5 in Table 3 and Fig. 3. Our findings suggest that early access to own pension wealth significantly reduces entry into the DI program. For retirement without DI, the point estimate indicates a negative effect, but this is only marginally statistically significant. Although a negative effect on the propensity to retire appears counterintuitive, it may be rationalized on the ground that access to one's own pension funds facilitates a more flexible combination of work and pension and thus reduces the demand for alternative exit through, for example, unemployment insurance or severance packages. Our main takeaway, however, is that we can rule out sizable positive effects on retirement propensity of early access to own pension funds. Column M5 also reports the role of economic incentives (state-specific predicted income). Despite the absence of pension-reform-generated variation in incentives for Group B, it is feasible to estimate incentive effects for this group due to minor changes in the tax system. The foundation for identification is weak, however, as reflected in the large standard error; hence, the lower estimated impact of predicted income should be interpreted with some care.

Columns M6–M8 in Table 3 present the estimated incentives and access effects for the complete dataset, that is, with all four groups included. We begin by comparing a model that includes incentives and access variables but no cohort or group-fixed effects (M7) with a model that includes a full set of group-specific cohort-fixed effects (M6) but no incentives or access variables. For the incentive-access model (M7), the estimated incentive effect is remarkably similar to the effect estimated for Group A only, whereas the access effects are similar to those estimated for Group B.

To interpret the reform effects in terms of financial incentives and access to own pension wealth, we combine separate group- and cohort-fixed effects with the added assumption that the cohort effects are the same for the 1947 and 1949 cohorts (M8). The results indicate incentive and access effects of roughly the same size as those estimated for Groups A and B separately. Access to own pension wealth appears to be unimportant in the choice between work and retirement once we have controlled for group-fixed effects, but it is very important for the propensity to claim DI benefits.

To see how the time patterns are interpreted in models that assume away any general trend effects between the 1947 and 1949 cohorts (M3, M5, and M8), in Fig. 3, we compare the estimated cohort effects from these models with those based on a full set of cohort-fixed effects (M1, M4) or cohort-by-group-fixed effects (M6). It is clear that by forcing the cohort effects to be the same for the last pre-reform and the first post-reform cohorts, the observed changes in retirement behavior and DI claims are interpreted as caused almost entirely by the pension reform. The estimated within-period trends on each side of the reform, however, are similar across the different models. It is also notable that, for Group A (the upper two panels in Fig. 3), the separate Group A model (M3) and the joint model (M8) yield similar

trend estimates. This is not something that can be taken for granted, as M8 restricts the underlying time trend to be the same for all groups.

Viewed as a whole, the results in Table 3 suggest that the impact of the reform on the choice between work and retirement at age 63 can be neatly accounted for by its impact on the discounted income streams associated with these alternative states. To account for its influence on disability insurance claims, however, it is important to take its impact on access to own pension wealth into account. Early access to pension savings reduces the pressure on the DI program significantly, even when access is offered on actuarially neutral terms.

Given the considerable heterogeneity in the financial situations of workers approaching retirement age, it seems probable that the influence of work incentives and access to their own pension wealth vary across workers. In the Online Appendix, Table A2, we present estimates of the key parameters in M8 separately by the level of financial savings at age 60 (bank deposits), the size of lifetime earnings (age 30–59), and marital status at age 60. Essentially, the estimated influence of financial incentives is remarkably stable across groups, whereas the estimated influence of access to own pension wealth varies considerably, depending on lifetime earnings. While access to own pension wealth raises the propensity to retire (without DI) for low earners, it decreases the propensity for high earners. The latter somewhat counterintuitive result may reflect the attractiveness of combining reduced work and pension claims.<sup>11</sup> It is also worth noting that own financial savings seem to reduce the influence of access to own pension wealth on disability insurance claims. In particular, we find no significant effect at all for workers with very high bank deposits (above approximately 100,000 euros). This is as expected, as these individuals are not likely to be constrained by liquidity.

To assess the robustness of our findings with respect to the choice of discount rate, we have also estimated the models with discount rates set to zero or four instead of two. The estimated impact of financial incentives change moderately, such that less myopia implies larger effects. For example, using M8 and assuming a discount rate equal to zero, the estimated NPI coefficient increases from 3.3 to 3.9. If we instead assume a discount rate equal to four, it declines to 2.9.

In Table 4, we offer more intuitive interpretations of the estimated incentive and access effects reported in Table 3. Table 4 presents selected average marginal effects based on M3 and M8 calculated for the last pre-reform birth cohort (1947). For the incentive variables, we show how the predicted outcome distribution is affected by a 10% increase in the net present value of each of the three state-specific income streams, respectively. Based on the results estimated for Group A (M3), we find that a 10% increase in employment income (*ceteris paribus*) will raise employment at age 63 from 57.3 to 65.2%, that is, by 7.9 percentage points (13.8%). This increase is mirrored by a 6.5 percentage point (18.3%) decline in early retirement and a 1.3

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<sup>11</sup> The difference may also reflect a composition effect resulting from the fact that high earners are overrepresented in Group B (who gained access to an actuarially neutral option of pension claiming) whereas low earners are overrepresented in Group C (who lost access to an option of claiming a pension with a strict earnings test) (see Online Appendix Table A1).

**Table 4** Average estimated marginal effects—1947 cohort

	Group A—M3			All groups—M8		
	Employment	Retirement	DI	Employment	Retirement	DI
Observed 1947 cohort	0.573	0.358	0.068	0.661	0.251	0.088
Predicted by M3/M8	0.573	0.355	0.071	0.670	0.237	0.093
+ 10% employment income	0.652 [+0.079] (+13.8%)	0.290 [-0.065] (-18.3%)	0.058 [-0.013] (-18.3%)	0.732 [+0.062] (+9.3%)	0.193 [-0.044] (-18.6%)	0.075 [-0.018] (-19.3%)
+ 10% retirement income	0.503 [-0.070] (-12.2%)	0.435 [+0.080] (+22.5%)	0.062 [-0.009] (-12.7%)	0.621 [-0.049] (-7.3%)	0.293 [+0.056] (+23.6%)	0.086 [-0.007] (-7.5%)
+ 10% disability income	0.558 [-0.015] (-2.6%)	0.345 [-0.010] (-2.8%)	0.097 [+0.026] (+36.7%)	0.649 [-0.021] (-3.1%)	0.230 [-0.007] (-3.0%)	0.121 [+0.028] (+30.1%)
Observed 1947 cohort Group B	-	-	-	0.773	0.135	0.092
Predicted by M8	-	-	-	0.775	0.127	0.098
+ Gain of early access to pension wealth	-	-	-	0.791 [+0.016] (+2.1%)	0.131 [+0.004] (+3.1%)	0.077 [-0.021] (-21.4%)
Observed 1947 cohort Group C	-	-	-	0.543	0.347	0.111
Predicted by M8	-	-	-	0.569	0.291	0.140
+ Loss of early access to pension wealth	-	-	-	0.548 [-0.021] (-3.7%)	0.276 [-0.015] (-5.1%)	0.176 [+0.036] (+25.7%)

Numbers in brackets show estimated marginal effects measured in percentage points; numbers in parentheses show the effects measured in percent

percentage point (18.3%) decline in disability program participation. Despite large differences in group composition and circumstances, similar results are obtained for all groups when the simulations are based on M8. As indicated above, the estimated responses to changes in economic incentives would have been somewhat stronger (weaker) had we assumed a lower (higher) discount rate. For example, based on M8, we find that the predicted 9.3% increase in employment following from a 10% increase in the net present value of expected employment income would have been estimated to be 10.6% with a discount rate of zero and 8.0% with a discount rate of four.

Note that the parallel relative decline in retirement and participation in the disability program follows directly from the properties of the statistical model. By construction, the multinomial logit model implies that the relative individual-choice probabilities of early retirement and DI are unaffected by a change in the reward of employment. Hence, the assumption of free choice—that is, that the utility-maximizing state actually belongs to each agent's choice set—is essential. Although this may not always be satisfied for the DI alternative, it may serve as a reasonable approximation for the elderly population under study and thus provide a sound basis for interpretation of the reform effects.

The relatively large incentive effects we identify also imply that changes in DI benefits will have a considerable impact on participation in the DI program. For example, for Group A (M3), we predict that a 10% rise in DI income will, *ceteris paribus*, increase program participation at age 63 by 2.6 percentage points (36.7%).

Based on M8, we can also assess the quantitative impacts of providing individuals access at actuarially neutral terms to their own pension wealth at a lower age. The results in Table 4 indicate that such access actually increases employment. For Group B—who gained such access through the pension reform—our estimates indicate a 1.6 percentage point (2.1%) increase in employment. For Group C—who lost access—we estimate a 2.1 percentage point (3.7%) decline. For both groups, we find that access to own pension wealth leads to far fewer entries into the DI program. For Group B, we estimate that the gain of early access reduced entry into the DI program by 2.1 percentage points (21.4%), whereas for Group C, the loss of early access increased entry by as much as 3.6 percentage points (25.7%).

We assess the robustness of the results in Table 4 with respect to model specification by reporting corresponding estimates based on separate ordinary least squares (OLS) equations for each of the three outcomes (see Online Appendix, Table A3). Since OLS does not allow covariates to vary with alternatives, the estimates are not directly comparable. In the OLS models, we include two relative-incentive variables in each equation, that is  $[\log(\text{NPI}_2) - \log(\text{NPI}_1)]$  and  $[\log(\text{NPI}_3) - \log(\text{NPI}_1)]$ , in actual and hypothetical (pre- and post-reform) values. This specification ensures that a given percentage increase in one of the NPIs has approximately the same effect as the same percentage decrease in the other two, as in the conditional logit model. The resulting OLS version of Table 4 is provided in the Online Appendix as Table A3.

Overall, the OLS results are qualitatively similar to those based on the conditional logit model, although point predictions differ somewhat. In contrast to the conditional logit model, OLS does not impose the assumption that the relative choice probabilities for any two alternatives are independent of the attractiveness of

the third alternative. This opens space for differential retirement and DI responses to, say, an increase in work incentives. For example, whereas the conditional logit model predicts that a 10% increase in employment income reduces retirement and DI participation by 18–19%, the OLS model predicts a larger effect on retirement (27–40%) and a smaller effect on DI participation (11–21%).

Based on M8, it is possible to decompose the overall change in behavior resulting from the Norwegian pension reform into factors related to changed incentives, changed access rules, and changed covariate composition. The results are shown in Table 5.

The first three lines in Table 5 show the observed changes in behavior. Employment increased by 10.7 percentage points, mirrored by a decline in retirement by 9.2 percentage points and a decline in DI of 1.6 percentage points. The following four lines show the results of simulations with M8. In the pre-reform sample (line 4), the combined effect of the incentives and access is an increase in employment of 8.1 percentage points and declines in retirement and DI of 6.9 and 1.3 percentage points, respectively. Partial simulations (lines 5 and 6) almost add up in a simple way to the combined effect and allow the interpretation that most of the impact on employment and retirement is a result of incentives, in contrast to DI, where about one-third is from changes in access.

Compared to the change observed from the pre- to the post-reform samples, we also need to take into account the changes in composition with respect to incentives and covariates. In line 7, we compare simulations on the pre- and post-reform samples with post-reform incentives and access. The difference is 1.7 percentage points higher employment in the post-reform sample and lower retirement (–1.2 pp.) and DI (–0.4 pp.). Groups A to D are differently impacted by the reform, and the relative group sizes change over time. In particular, Groups C and D are shrinking as more people qualify for early pension access over time. This accounts for some of the increase in employment and the decline in income from DI benefits.

**Table 5** A decomposition of behavioral changes from the pre-reform to the post-reform cohorts

	Employment	Retirement	DI
1. Observed pre-reform cohorts	0.668	0.244	0.089
2. Observed post-reform cohorts	0.775	0.152	0.073
3. Overall change (post–pre)	0.107	–0.092	–0.016
4. Simulated change on the pre-reform sample	0.081	–0.069	–0.013
5. ...caused by new incentives	0.073	–0.070	–0.009
6. ...caused by new access rules	0.004	0.001	–0.005
7. Change in composition from pre- to post-reform sample	0.017	–0.012	–0.004

The decomposition is based on M8. This implies that we have ruled out any time trend between the 1947 and 1949 cohorts, as the absence of such a trend is an identifying assumption behind M8. The simulated change is based on the pre-reform sample. The result in line 4 is based on a simulation with access and incentives set to post-reform values. Lines 5 and 6 show results with access and incentives in turns set to post-reform values. Line 7 shows a simulation on the post-reform sample with post-reform values for access and incentives minus a simulation of the pre-reform sample with post-reform values of access and incentives

## 6 Concluding remarks

It has previously been shown that the 2011 pension reform in Norway led to a large increase in employment for those over 62. This study examines the impacts of this reform through the lens of a parsimonious random utility model, with a particular focus on possible substitution between early retirement and disability program participation. Our findings suggest a considerable interplay between these two exit routes with important implications for the design of pension reform.

We emphasize three lessons. First, the elimination of earnings tests in the pension system not only seems to substantially increase employment but also to reduce entry into the DI program. A possible explanation is that it makes the combination of labor earnings and pension income more attractive, thereby facilitating continued employment at reduced hours or intensity. Second, opening access to own pension funds on actuarially neutral terms at an earlier age also seems to have a positive effect on employment. At first sight, this appears counterintuitive, but again, the explanation may be that early access to pension funds makes it easier to cut down on one's workload and thus continue with some employment into higher ages. Finally, early access to own pension funds leads to a considerable reduction in transitions to DI, particularly for workers with little liquid savings outside the pension system. In essence, what early access seems to do is that it enables some workers to deal with their own health issues themselves rather than applying for disability benefits; it facilitates "self-insurance." While this option will only be available for workers with sufficient pension savings, it illustrates an important aspect of pension reform that may not have received adequate attention: When one exit route is blocked, the demand for others may increase. In the presence of (reasonably generous) DI programs, it is probable that higher pension ages or more restrictive access to own pension savings pushes more people into DI.

Our findings suggest that it may be advisable to give workers access to their own pension wealth on actuarially neutral terms somewhat earlier than the statutory or standard retirement age, provided that the funds are sufficiently large to ensure self-support in the absence of continued employment. This facilitates a smoother and more individually adapted transition from work to retirement and reduces the demand for DI. Our results suggest that it may also increase labor supply.

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**Data availability** The data used in this paper are not publicly available, but may be accessed through Statistics Norway with appropriate approvals.

## Declarations

**Conflict of interest** The authors declare no competing interests.

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