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ABSTRACT

The Impact of Pension Reform on Employment, Retirement and Disability Insurance Claims*

We evaluate a comprehensive reform of Norwegian early retirement institutions in 2011 through the lens of a parsimonious random utility choice model. The reform radically changed work incentives and/or pension access-age for some (but not all) workers. We find that improved work incentives caused employment to rise considerably, at the expense of both early retirement and exits through disability insurance. Lower access-age to own pension funds 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.

JEL Classification: H55, J22
Keywords: pension reform, disability insurance, program substitution

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1. Introduction

In this paper we study the choices of workers at age 63 across the states of employment, early retirement and disability insurance (DI) program participation, conditional on employment at age 60. The analysis exploits, and evaluates the impacts of, a comprehensive Norwegian pension reform implemented in 2011, which radically changed work incentives and pension access regulations for cohorts born after 1948, while leaving earlier birth cohorts unaffected. The reform impacted different groups of workers in different ways, depending on their pre-reform circumstances (Hernæs et al., 2016; Andersen et al., 2021). In particular, we identify groups who gained and lost early access to their pension wealth, and groups whose work incentives were and were not affected.

We examine the reform-initiated changes in economic behavior through the lens of a parsimonious random utility three-state choice model, which seeks 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 own pension wealth already at age 63. As we explain in more detail below, the causal effects of these variables are identified from the reform-generated variation only, conditional on an otherwise stable economic environment experienced by 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 percent increase in the expected remaining lifetime income associated with staying employed for five more years on average 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. When early access to own pension wealth is denied, the probability of participating in the DI program increases significantly, by 3-4 percentage points (25-26%). When it is provided, the DI probability decreases by 2.1 percentage points (21-22%). Hence, policy makers considering delaying retirement by raising access-ages to the potential retirees' own pension savings may have reasons to think twice – at least if there are other (subsidized) exit routes from the labor market. There appears to be a "squeezed sausage effect": If one exit route is closed, the demand for alternative routes increases. Access to 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 partial pension outtake.

Our paper is not the first to report an interaction between disability insurance programs and old age pension. Duggan et al. (2007) found that an increase in the full retirement age, which reduced the potential annual pension at lower ages, increased the DI program enrolment in the US. In a later study, Coe and Haverstick (2010) found that the same reduction in attractiveness of 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) reported 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 disability. Euwals et al. (2011) found no substitution into disability in the Netherlands, by comparing those eligible for an attractive early retirement program to those who are not. The latter analysis is based on data for the health

sector, and since earlier studies find such a substitution, they conclude that institutional changes have stopped the substitution.

We contribute to this literature not only by exploiting an exceptionally clean quasi-experiment with unusually large reform-generated differences in incentives for adjacent birth cohorts, but also by providing an economic interpretation within the context of a simple choice model. Precise identification of the overall reform effects requires assumptions about (the absence of) other local time trends. Yet, although we cannot rule out unobserved trends that interfere with impacts of the reform, the mere sizes of the shifts in observed behavior from the last unaffected to the first affected birth cohort render the typical threats against identification almost negligible. Our interpretation of the reform effects – in terms of the specific roles of incentives and pension access – is more open to discussion, however, as it relies 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 states can quite safely be assumed to belong to the workers' choice sets, a DI benefit is an option for persons with reduced work capacity due to sickness only, 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 examined in this paper, with ample scope for individual judgement and influence. In the cohorts examined in this paper, almost 40% of the population entered the disability insurance program before they reached the statutory retirement age of 67. As a proxy for potential eligibility, we will use data describing the workers' observed sickness absence over the age 55-59 period.

In a recent study, also based on Norwegian register data, Autor et al. (2019) look at the impact of getting a disability insurance benefit, utilizing the variation in the leniency of appellate judges in treating reapplications. Whereas their study examines how allowing a marginal applicant into the disability insurance program affects the household's consumption, labor supply, and income from other social programs, we look at how changes in economic incentives and access to own pension wealth impact the choice of seeking and getting a disability benefit. Hence, both studies shed light on the issue of social program substitution, yet from different angles.

We begin in the next section with a brief outline of Norway's pension and disability insurance institutions. Section 3 describes the data set and its organization for this study. Section 4 explains our empirical approach and Section 5 reports the results. Section 6 concludes.

2. Institutional Setting

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 pension 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 total income among 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 (hereafter referred to by its acronym AFP). Before the reform in 2011, AFP offered a full pension from age 62, 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. There was no deferral option by delayed take-up, and the pension was based on projected earnings up to age 67, in effect implying very high implicit tax rates on continued work. Hence, the AFP system embodied a strong disincentive to work after the age of 62 for its members, particularly for persons with relatively low wages.

The NIS and the private sector AFP were reformed in 2011 in a number of ways.[1] The NIS access-age was reduced to align with AFP access, but in both post-reform systems, annual benefits are adjusted on actuarially neutral terms. The NIS benefits are adjusted to generate a constant estimated present value of the expected benefit stream, anchored to expected work until age 67. The AFP was transformed to a supplementary pension independent of continued labor earnings. Hence, 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 taxes and earnings test reductions) was approximately doubled (Hernæs et al., 2016).

The reform also implied changes in early access to pension savings. Before the reform, only those covered by AFP had access to any form of pension income before age 67. After the reform, all persons with NIS and AFP entitlements ensuring a pension at least as high as the guaranteed minimum pension from age 67, have pension access from age 62. As, well, in the new system, entitlement to AFP is dependent on entitlement to early takeout of the NIS pension. As a result, workers with a contribution history too thin to ensure an annual pension above the minimum level, lost their entitlement to AFP and could also not start drawing on their own pension savings. On the other hand, workers with sufficient contribution history gained access to own pension wealth on actuarially neutral terms from age 62, regardless of AFP entitlement.

Disability insurance (DI)

Disability benefits can be granted if health problems imply that the ability to provide own income is reduced, typically by at least 50%. This loss must be certified by a physician and then approved by the social security administration. It is granted in the age span 18-67. At age 67, the old age pension takes over. For disability benefits granted between age 62 and 67, there is also a condition of an annual income of at least 1 Basic amount (BA) (a metric used extensively in redistributive policies in Norway, worth about 1/6 of average full-time earnings, approximately € 10,000) in the course of the three preceding years.

The full annual benefit is 66 % of the person's previous labor earnings up to an earnings threshold approximately corresponding to average fulltime earnings. There is also a minimum level, and, as a result, the annual benefit before tax is almost always in the range of 38-66 % of average fulltime earnings.

[1] 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.

Disability insurance can be temporary or permanent, but for the age group considered in this paper, the difference between the two is more a matter of timing than of content. A permanent disability benefit is typically preceded by a temporary one, and at high age it is rarely the case that temporary disability is not followed by a permanent disability (or by a transfer to old-age pension at age 67),

Disability can be full or partial, based on the assessed, remaining earnings capacity. The disability benefit can be combined with earnings, but subject to income testing. With a full permanent disability pension and earnings above a level of about 7 % of average full time earnings (0.4 BA), the disability is reduced by the fraction that current earnings constitute of the previous earnings. With a partial disability pension, also earnings up to a fraction of previous earnings are exempt.

3. Data and Empirical Overview

Data sources

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

The register data on individuals cover the whole population of Norway. The most important information is annual earnings and other income, taxes paid, and benefits received. At age 60, we identify for all employees the identity of the employer and the worker's tenure. Together with complete data on pension-generating earnings back to 1967, this information facilitates accurate computation of pension entitlements, both from NIS and AFP, and under pre-reform as well as post-reforms 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 during age 55-59.

Sample

In our analysis, we use the pre-reform birth cohorts 1945-1947 and the post-reform birth cohorts 1949-1951. The 1948 cohort is a transitional cohort, with some (but not full) potential reform influence, and the cohorts born from 1952 were affected by a later reform of the disability insurance program in 2015; see Alne (2018). Using the cohorts born between 1945 and 1951 has the, from our perspective, important advantage that they all faced a similar economic environment around age 63 (our outcome year), with unemployment rates among people above 55 years (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 approximately 1/6 of average full time earnings) and who were not receiving DI benefits at that point.[2]

Classification at age 60

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

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
	1945-47	1949-51	1945-47	1949-51	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	3,564	2,631	6,195	Radically improved work incentives. Lost early access.
D	No	No	7,513	6,806	14,319	No change in either work incentives or access.
All			54,254	54,515	109,039	

Outcome at age 63

To capture the impact of the reform, we look at the observed state at age 63, which is the first full calendar year after age 62 when the new rules set in after the reform. At this age, income can come from three different sources; i.e., from work, from old-age-pension, and/or from disability insurance. The latter is typically granted after a lengthy sick leave, and can be temporary or permanent. Here, we do not distinguish between temporary and permanent

[2] We us as threshold the basic unit, G, in the Norwegian social security system. This is updated annually to index pensions and calculate entitlements. One G is quite close to 1/6 of average full time earnings.

disability insurance, however, as the probability of returning to employment from temporary disability after age 63 is almost negligible.[3]

Table 2 shows how members of the different birth cohorts combined different types of income at age 63, and how we use this information to aggregate age-63-outcomes into three states. It is notable that the fraction in employment increased sharply after the reform, from 64.1% (58.0+6.1) for the 1947 cohort to 73.7% for the 1949 cohort. Most evidently, the combination of employment and pension income became much more common (from 6.1% to 35.4%), as the new NIS (public old age pension) became accessible from age 62. Before the reform, this was mainly possible via AFP only, and then with a confiscatory earnings test.

Table 2. Classification by work, disability and pension at age 63. Working in private sector at age 60. Percentages

		Employment (%)		Retirement (%)		Disability insurance (%)	
	Number of observations	Labor earnings only	Labor earnings and pension	Only pension income	No income	Only DI	DI and work
1945	18202	57.7	6.2	17.6	9.2	6.4	3.0
1946	20482	58.5	7.4	16.5	8.5	6.0	3.1
1947	19624	58.0	6.1	17.7	9.0	6.3	2.8
1949	19569	38.3	35.4	12.2	5.4	5.9	2.7
1950	19263	36.1	39.7	12.0	4.7	5.0	2.4
1951	19168	34.5	40.9	13.0	4.6	4.7	2.3
All	116308	47.2	22.6	14.8	6.9	5.7	2.7

Note: Persons who at age 60 had at least one BA (approximately 10,000 Euro) annual earnings and who were not receiving disability benefit above 0.2 BA. The classification thresholds in the table is annual amounts of at least 1 BA. The disability states include a few who also receive an old age pension during the year.

Figure 1 gives a more detailed picture of age-63-outcomes for the four different groups for the pre-reform (1945-47) and the post reform cohorts (1949-51). We note in particular the large employment increases in group A and group C, in group A matched by a corresponding decline in retirement and in group C matched by an even larger drop in retirement accompanied by an increase in disability insurance claims. In group B, we note that there were only small (if any) reform-generated changes in employment, but perhaps a shift from DI program participation to retirement by means of own pension funds.

[3] Among those who received some form of disability insurance benefits at age 63, 95 % did that also at age 64.

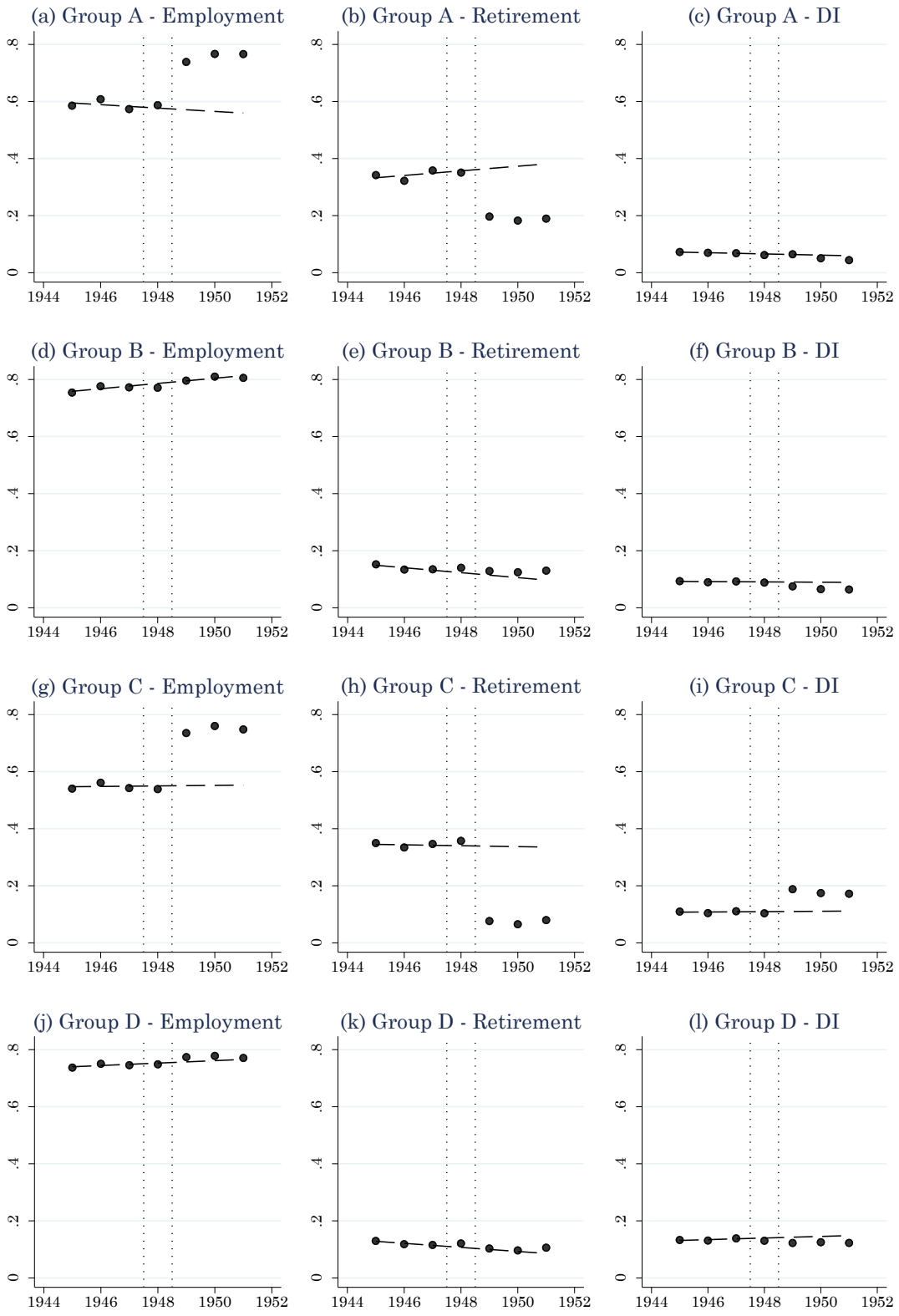

Figure 1. Activity at age 63 by group classification at age 60, for cohorts 1945-1951.
Note: The dotted lines are linear extrapolations (OLS) of the 1944-46 observations.

4. Empirical Approach

The central idea of the paper is that there is a number of persons who at mature age are in a "grey zone" between work, retirement and disability. They may or may not be eligible for DI benefits, but can influence their eligibility status through own behavior. For example, some workers may be able to continue working if they reduce the workload by cutting hours and earnings, yet they may qualify for a full DI. In that sense, they are in a situation where a decision around working is available. Their actual choice may depend on work incentives, on whether or not they can start drawing on their old-age pension, and on the costs associated with a DI application as well as its expected outcome. Different pension regimes may therefore give different choices.

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) an option to start drawing their pension wealth from an earlier age.

Model

The main purpose of our empirical analysis is to examine 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 the remaining lifetime, and ii) the access to own pension wealth from age 63. As the former characteristic varies by outcome, whereas the latter varies by individual only, we base our analysis on a three-state conditional logit model (McFadden, 1974). We use the extent of sick leave over a preceding five-year period as a proxy for potential DI eligibility.

The methodological basis for our choice model is a set of utility functions, which describe the evaluation of person i of state j as a function of state-specific and individual characteristics in addition to a random "taste shifter":

$$U_{ij} = \mathbf{X_{ij}}\boldsymbol{\beta} + \mathbf{Z_i}\boldsymbol{\alpha_j} + \varepsilon_{ij} = U^*_{ij} + \varepsilon_{ij} \; for \; j = 1,2,3, \tag{1}$$

where $j = 1$ indicate work, $j = 2$ indicate retirement without DI, $j = 3$ indicate DI, $\mathbf{X_{ij}}$ indicates variables varying by state (economic incentives), and $\mathbf{Z_i}$ indicates variables varying by individuals only (personal 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 modelled as:

$$\Pr(Y_i = j) = \frac{\exp\left(U^*_{ij}\right)}{\sum_{j=1}^{3}\exp\left(U^*_{ij}\right)} \; j = 1,2,3 \; . \tag{2}$$

This specification implies that the relative choice probabilities for any two alternatives are modelled 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 for persons without any health problems. Formally, we can think of that as implying very low

utility in the DI state, which will then be captured by variables revealing low past sickness absence. We may also interpret the results in a more reduced form fashion, since the actually realized labor market state results from a combination of preferences and opportunities.

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 both pre- and post-reform pension rules under the following simplifying assumptions, based on the choice of labor market state at age 63:[4]

i) The employment alternative implies work for five years, at ages 63-67, and then full retirement.
ii) The disability insurance alternative implies DI benefits for five 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.[5]
v) Expected lifetime is 82 years, and the (real) discount rate is 2 percent.

Economic incentives are of course not randomly assigned, as persons with different expected state-specific income streams are likely to differ systematically along other dimensions as well, such as health status, motivation, job characteristics, and valuation of leisure. In order to facilitate estimation of the causal relationship between incentives and choice of status at age 63, we thus seek to isolate the random assignment-like variation generated by the reform. We do this by including in the X-vector not only the actually predicted state-specific income streams, but also the corresponding hypothetical income streams that would have applied under pre-reform 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:

$$\begin{aligned} &X_{ij} = \log(NPI_{ij}^{Old}), \log(NPI_{ij}^{New}), \log(NPI_{ij}^{Actual}), \quad \text{with} \\ &\log(NPI_{ij}^{Actual}) = (1 - REFORM) \times \log(NPI_{ij}^{Old}) + REFORM \times \log(NPI_{ij}^{New}), \end{aligned} \tag{3}$$

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 rules, respectively, and REFORM is an indicator variable equal to 1 for the 1949-51 birth cohorts

[4] 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 age 60 earnings. Pre-reform cohorts are taxed by 2010 tax rules, whereas post-reform cohorts are taxed by 2011 rules. All incomes are inflated to 2014-value, using the adjustment factor embedded in the Norwegian pension system. They are translated to Euro value by using the average exchange rate in 2014; i.e., € 1=NOK 8.35.

[5] Those who did not qualify from age 62 for the post-reform pension, were assumed to claim as soon as they qualified, and no later than age 67, when all could claim,

(and zero for the 1945-47 cohorts). It is then the coefficient associated with NPI_{ij}^{Actual} that captures the causal effect.

Figure 2 shows how the average expected income streams vary across the four groups in our data and how they were affected by the pension 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 was chosen was on average equal to approximately € 30,000 per year. Had these cohorts been exposed to post-reform pension rules, the work-alternative would have been much more profitable, such that the expected remaining lifetime net income would have been 13% higher on average, i.e., approximately € 34,000 per year. For the post-reform cohorts, the pattern is exactly the same, with the important difference that for the 1945-47 cohorts, the pre-reform incentives were the actual incentives, whereas for the 1949-51 cohorts, the post-reform incentives were the actual incentives. Looking at the overall patterns described in Figure 2, we note that for groups A and C, employment became much more economically attractive as a result of the reform, whereas the retirement alternative became less attractive. For the members of group B, all alternatives became more generously rewarded, whereas for group D, there were only minor changes.

In our choice model, it is the relative economic rewards that matter; i.e., the *differences* between the log(NPI) derived from the alternative states. In Figure 3, we show how these differences changed as a result of the reform. We use the employment alternative as the reference state, such that the graphs show the differences between log(NPI) in retirement and employment and the differences between log(NPI) in DI and employment. To illustrate how the reform changed incentives at individual levels, we use a 15% random sample of the data to form a cross-plot between pre-reform (horizontal axis) and post-reform (vertical axis) incentives. Starting in the upper left panel of Figure 3, we note that for members of group A, the income stream in retirement relative to that in employment (log(NPI in retirement)-log(NPI in employment)) shifted down almost in in parallel for everyone by approximately 0.2 log-points. Moving to the upper right panel instead, we see a similar pattern for DI, although with somewhat larger individual variation. For members of group C, we see a pattern similar to group A, whereas for the members of groups B and D, the reform-initiated changes are much smaller.

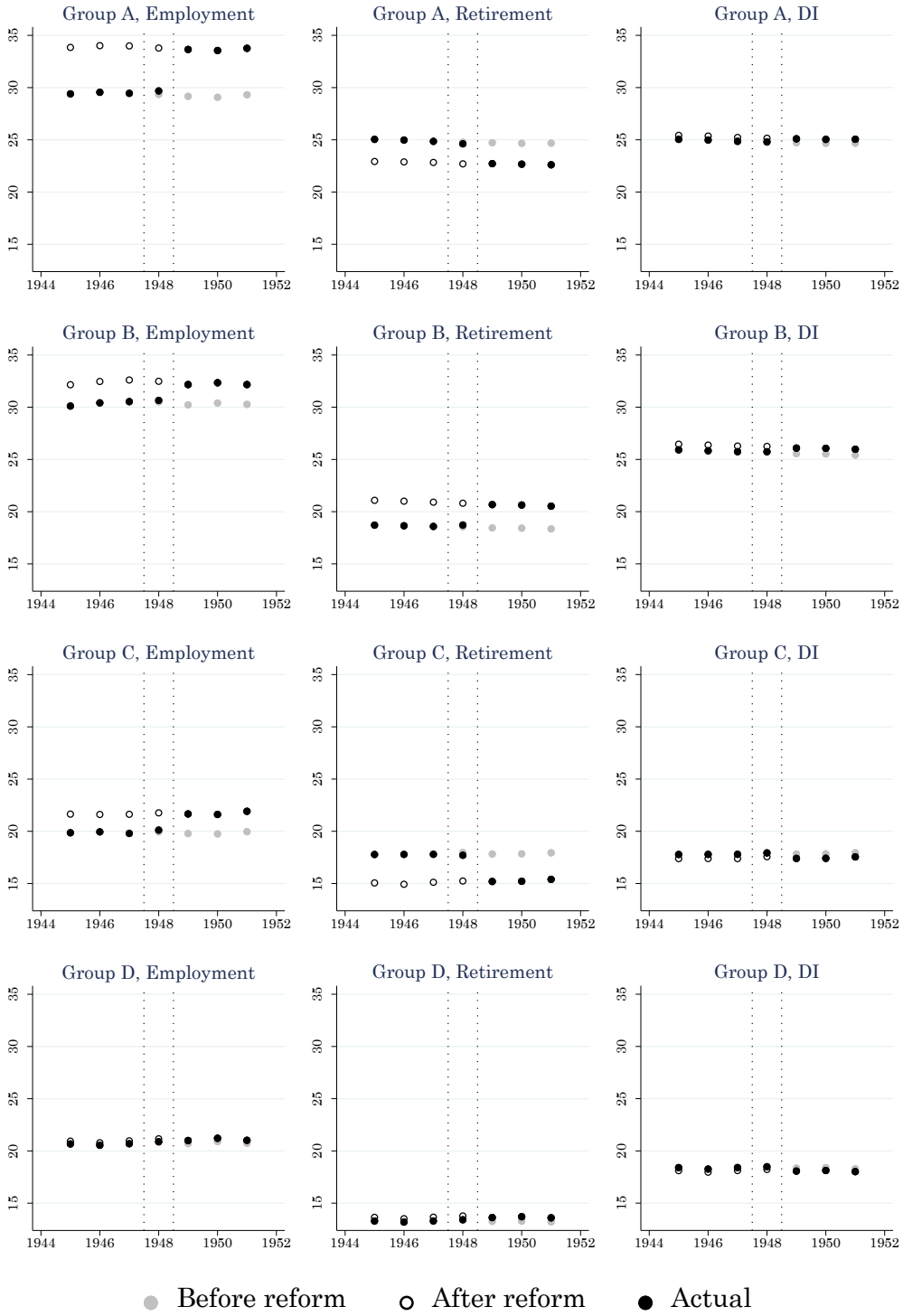

Figure 2. Predicted annualized lifetime net incomes (age 63-82) by state at age 63. By group and cohort, 1945-1951
Note: Annualized lifetime incomes are measured in 1000 Euros, inflated to 2014 value and converted to Euro with the exchange rate applying in 2014 (€ 1=NOK 8.35) and divided by 21 to approximate a average annual amount.

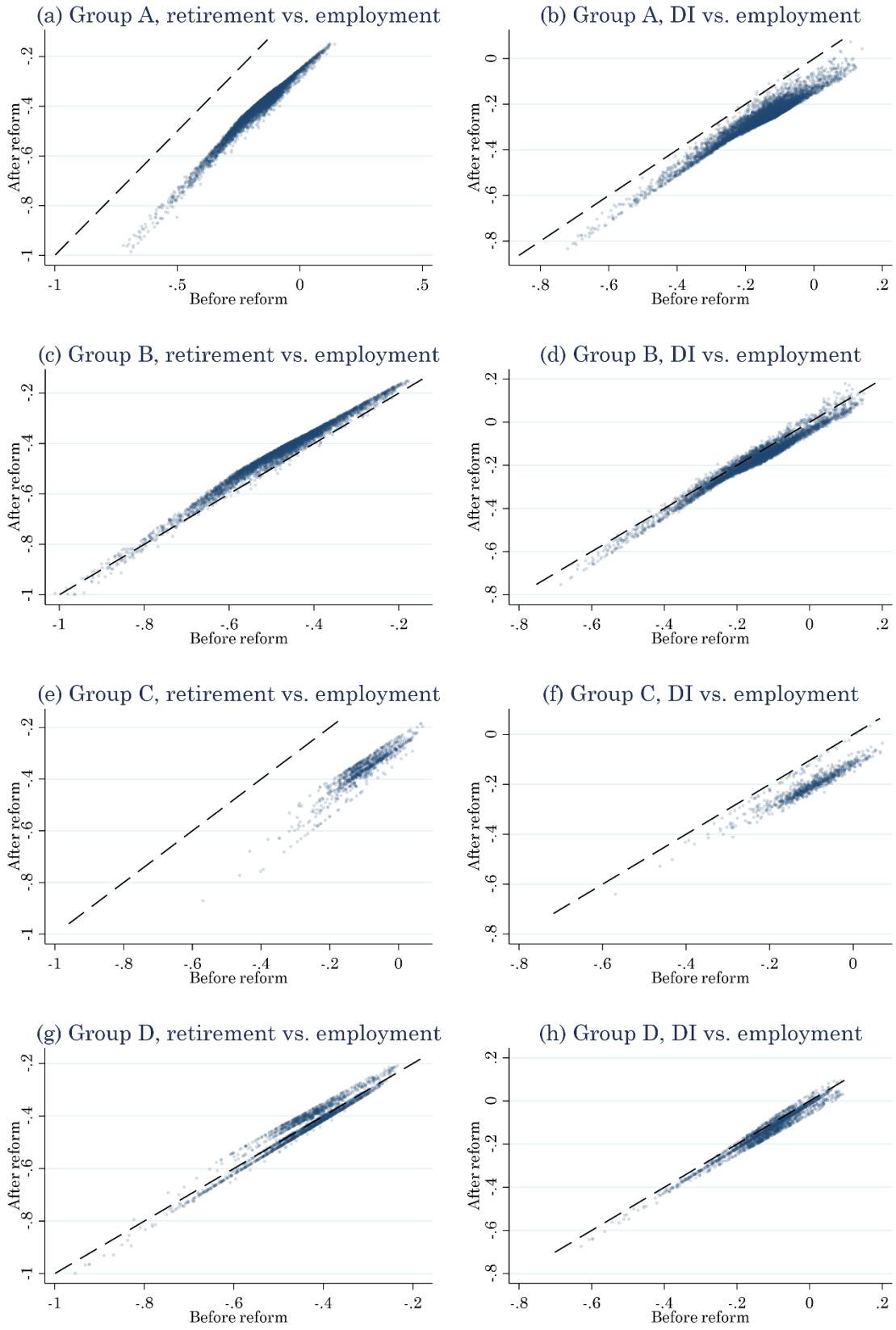

Figure 3. Economic incentives. Differences in log(net predicted present values of the income streams) between retirement and employment (to the left) and DI and employment (to the right) before and after the reform.
Note: The 45-degree-lines correspond to no reform-generated incentive change. The cross-plots are based on a 15% random sample.

To the extent that our model allows for group –and cohort fixed effects (or interactions between the two), Figure 3 illustrates a potential identification problem, namely that the reform-initiated changes in incentives exhibit little within-group variation. This is particularly evident for the retirement-versus-employment choice facing members of group A. If we include flexible cohort-fixed effects in a model estimated for this group, it will clearly be difficult to disentangle the role of economic incentives from unspecified cohort effects.

As we explain in more detail in the next section, our solution to this dilemma is to estimate alternative models, separated by differences in the way we control for groups and cohorts. Our preferred specification is going to be a model including 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 reform) are negligible over this short time-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 observed developments over the last pre-reform and first post-reform cohorts shown in Figure 1 largely confirm this assumption. The changes in labor market state at age 63 were small both pre and post reform, and almost invisible relative to the jumps that occurred between the 1947 and 1949 cohorts. There is, however, some indications of a post-reform trend toward lower DI program participation. While this may represent at violation of the no-trend-assumption, it may also represent a lagged reform effect. As it may take some time to be admitted into disability insurance, it seems probable that the reform effects may have become stronger over time.

Identification of access effects

Another important covariate reflects access to own pension wealth at age 63. In contrast to disability insurance entitlements, we identify precisely who has this option, before and after the reform. This is separable from economic incentives, since it reflects the option of claiming a pension earlier, but subject to actuarial adjustment. Without liquidity constraints, this should not matter for perfectly rational and foresighted agents. However, liquidity constraints may indeed be important, and it is extremely uncommon to borrow against future pension. The variation in access in our data comes 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 gain access at age 63 if they were born in 1945-47, but had access if they were born in 1949-51.
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 1945-47, but not if they were born in 1949-51.

Consequently, we specify pension access as a dummy variable which is 1 before and 0 after the reform for those who lost access (group C) and 0 before and 1 after the reform for those gained access (group B). For group A it is always 1 and for group D it is always 0.

Identification of a single "access-effect" hinges on the assumptions that members of group B and group C respond in a similar fashion and that policies introducing and abolishing access have symmetric effects. These assumptions can clearly be questioned.

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, master or above), average earnings age 30-59, and size of bank deposits. Table 3 provides an overview of the group-specific sample composition of these variables, as well as incentives and outcomes (shown graphically above), for the pre- and post-reform cohorts.

Table 3. Descriptive statistics

	Group A		Group B		Group C		Group D	
	1945-47	1949-51	1945-47	1949-51	1945-47	1949-51	1945-47	1949-51
Women (%)	19.2	22	15.7	18.3	91	87.7	84.2	80.9
Marital status (%)								
Married/partner	74.9	71.5	75	72.4	74.2	74.2	72.9	71.9
Divorced	18.1	20	19.1	20.7	22.8	21.6	22.6	21.9
Never married	7	8.6	6	6.9	3	4.2	4.6	6.2
Education (%)								
Compulsory only	19.3	16.2	14.7	12.6	35	32.3	23.8	21.1
High school	62.4	65.1	57.6	55.9	60.7	62	62.3	60.7
Bachelor	13.4	14	18.6	21.3	3.5	4.2	11.7	13.9
Master or above	5	4.7	9.1	10.2	0.8	1.5	2.2	4.3
Av. gross earnings age 30-59 (1000 €)	72	72.4	78.5	79.8	29.1	28.5	30.3	29.8
Bank deposits at age 60 (1000 €)	46.2	46.6	62.6	58.3	29.2	28.4	30.6	33.2
Fraction with bank deposits above 32700 Euro	35.8	34.9	38.3	37.4	29	26.6	26.1	27.5
Years with paid sick leave age 55-59 (%)								
None	85.4	85.8	85.2	84.6	64.4	62.9	75.9	76.9
1-2	8.7	8.5	9.8	10.3	19.2	20.8	15.5	15
All 5	5.9	5.7	5	5.1	16.5	16.3	8.6	8
Predicted annualized remaining lifetime income by state at age 63 (1000 €)								
Employment	30.1	33.5	30.7	32	20.3	21.6	20.8	21.1
Retirement	24.7	21.9	18.6	20.1	17.7	15.2	13.2	13.3
DI program	24.1	23.8	24.9	24.6	17.7	17.7	18.2	18.2
Outcome age 63 (%)								
Employment	58.9	75.7	76.8	80.4	54.9	74.8	74.5	77.4
Retirement	34.1	19	14	12.8	34.3	7.4	12.1	10.2
DI program	7	5.3	9.2	6.8	10.8	17.9	13.4	12.4
Number of observations	25,247	26,385	18,200	18,693	3,564	2,631	7,513	6,806

5. Estimation Results

We start out estimating our choice model based on group A only. This is convenient because the members of this group were subjected to changes in economic incentives only, and not to changes in pension access; hence they provide the cleanest basis for identification of incentive effects. We estimate four different models, distinguished by how they include incentives and/or

cohort-fixed effects. The main results are presented in Table 4, with a selection of estimated cohort effects shown in Figure 4. At the bottom of Table 4, we also provide two metrics describing how well the models fit the data. "Prediction accuracy" compares the predicted and the 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 model for Group A (M2 and M3) and when cohort-by-group dummies are included in the model for all (M6 and M7). 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 4).

M1 is the most parsimonious model; it includes the incentive variables (pre-reform, post-reform, and actual state-specific predicted income streams), but no cohort-fixed effects. M2 represents the opposite approach; it contains no incentive variables, but separate dummy variables for each cohort. Looking at the resultant log-likelihood values and "Model fit" metrics, it is clear that the more parsimonious model (M1) actually explains the data better than the cohort-fixed-effects model (M2). Model M3 combines M1 and M2 and includes both incentive variables and cohort-fixed effects. This model improves the fit to the data, measured both by the log-likelihood and "Model fit".

While M3 has a better fit, it renders the estimated incentive effect small and statistically insignificant, implying that the cohort dummies are left to pick up the large shifts in outcomes between the 1947 and 1949 cohorts. Since we know that the reform did not change anything but the economic incentives for this group, we suspect that the reason why the large behavioral changes are not absorbed by the incentive variable is what we saw in Figure 3; i.e., that the incentive changes in group A were almost perfectly parallel throughout the incentives distribution. In Model M4, we take the consequence of this view and forces the cohort effects to be the same for the 1947 and 1949 cohorts, in essence assuming away any spurious trends across these birth cohorts. Somewhat simplified, we could say that whereas M2 may be 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), M4 offers a more useful economic interpretation of these effects provided that the added stability assumption for the 1947-1949 cohorts is valid.

Columns M5-M8 in Table 4 present corresponding results for the complete dataset; i.e., with all four groups included. Again, we start out with a parsimonious model (M5), including incentives and access variables, but no cohort or group fixed effects. The estimated incentive effect is then remarkably similar to the effect estimated for group A only. Access to own pension wealth is estimated to have a considerable negative influence on transitions to disability insurance, but a very small (and presumably wrongly signed) effect on early retirement. Again, the parsimonious model performs better in terms of log-likelihood value than a model with a full set of group-specific cohort fixed effects (M6).

Yet, the combination of the two models (M7) renders the incentive effect small and statistically insignificant (note that in M6 and M7, any access effects will be absorbed by the groups-specific cohort effects). 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 then indicate an incentive effect of roughly the same size as in group A. Access to own pension wealth appears to be unimportant for the choice between work and retirement once we

have controlled for group effects, but very important for the propensity to claim disability insurance benefits.

To see how the models that assume away any general trend effects between the 1947 and 1949 cohorts (M4 and M8) interpret the overall time patterns, Figure 4 compares the estimated cohort effects from the models with a full set of cohort-fixed effects (M2) or cohort-by-group fixed effects (M6) with the models restricting the cohort effects to be equal for the 1947 and 1949 cohorts (M4 and M8). It is clear that by forcing the time trend to be the same for the last pre-reform and the first post-reform cohorts, the observed changes in retirement behavior and disability insurance claims are interpreted as almost entirely caused by the pension reform. The estimated within-period trends on each side of the reform are similar across the different models, however. It is also notable that for group A (the two upper top panels in Figure 4), the separate group-A-model (M4) and the joint model (M8) yield similar trend estimates. This is not something that could 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 4 suggest that the reform's influence on the choice between work and retirement at age 63 can be quite nicely accounted for by its impact on the discounted income streams associated with these two alternative states. However, in order to account for its influence on disability insurance claims, 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 disability insurance program significantly, even when access is offered on actuarially neutral terms.

Table 4. Estimation results (standard errors in parentheses)

	Group A only (M1-M4)				All groups (M5-M8)			
	M1	M2	M3	M4	M5	M6	M7	M8
Predicted state income (NPI actual)	3.610 (0.088)		0.830 (0.965)	3.625 (0.148)	3.730 (0.083)		0.630 (0.664)	3.314 (0.119)
<u>Retirement</u> Access to own pension wealth					-0.049 (0.026)			0.013 (0.035)
<u>DI program</u> Access to own pension wealth					-0.258 (0.026)			-0.272 (0.037)
Cohort-fixed effects		Yes	Yes					
Cohort-fixed effects with 1947=1949				Yes				Yes
Group-by-cohort fixed effects						Yes	Yes	
Group-fixed effects								Yes
Log likelihood	-38573.2	-38900.8	-38525.4	-38531.0	-77330.4	-77732.7	-77085.2	-77238.9
Prediction accuracy	99.2 %	100 %	100 %	99.8 %	98.9 %	100 %	100 %	99.1 %
Model fit	0.055	0.044	0.056	0.056	0.060	0.053	0.062	0.061
# Observations	51,632	51,632	51,632	51,632	109,039	109,039	109,039	109,039

Note: All models include the variables listed in Table 3 related to gender, marital status, education, past earnings, sickness absence and the fraction with bank deposits above 32,700 Euro, as individual covariates. All models except M2 and M6 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 actually chosen state 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 naïve model) and unity (perfect predictions for everyone).

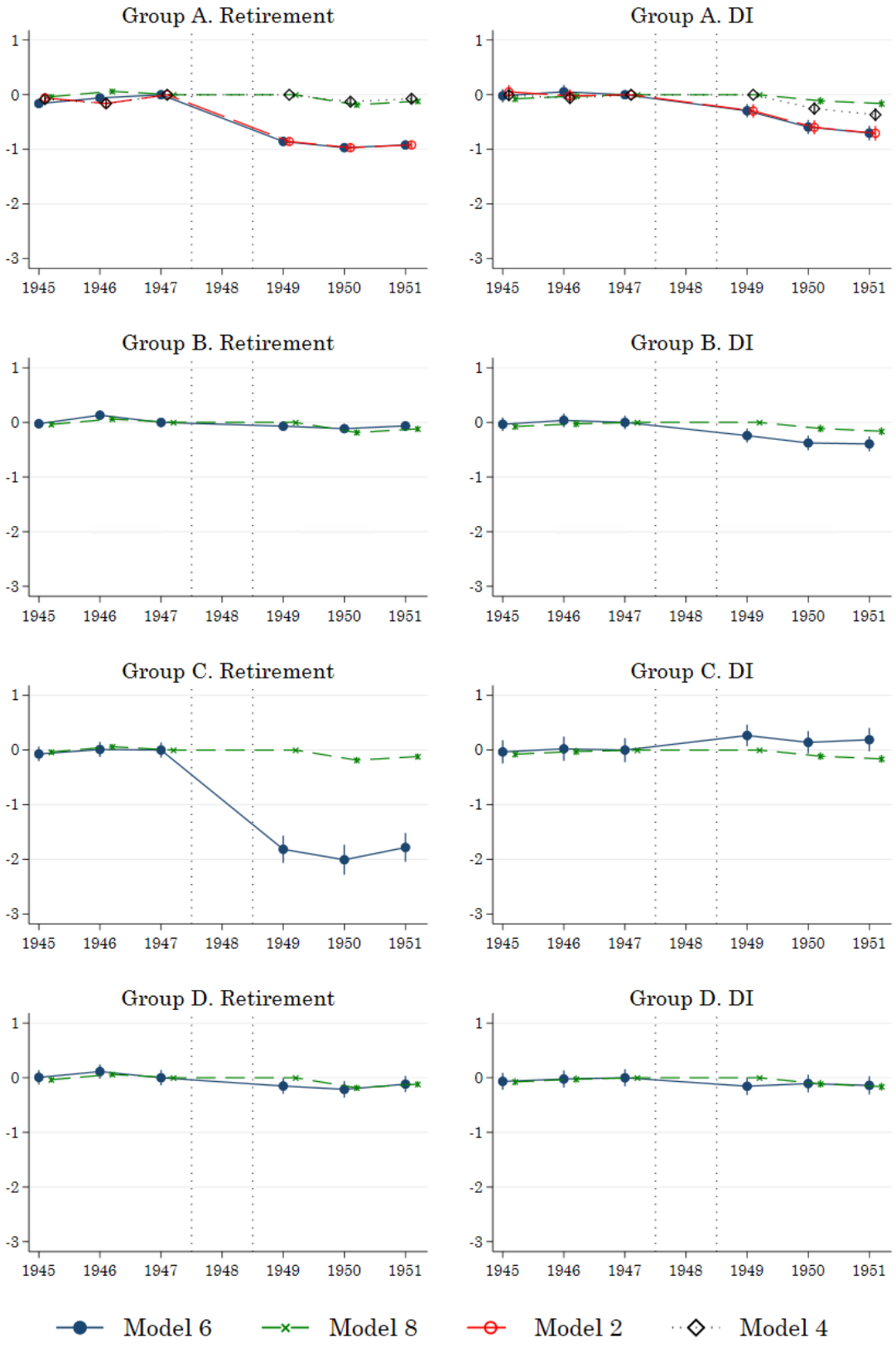

Figure 4. Estimated cohort effects (underlying time trends). By group and choice of model
Note: The cohort-fixed effects are normalized on the 1945-cohort. See Table 4 for a description of the models.

To offer more intuitive interpretations of the estimated incentive and access effects, Table 5 presents selected average marginal effects based on M4 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 (M4), we find that a 10% increase in employment income (ceteris paribus) is predicted to raise employment at age 63 from 57.3% to 65.2%; i.e. by 7.9 percentage points (13.8%). This increase is mirrored by a 6.5 percentage points (18.3%) decline in early retirement and a 1.3 points (also 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. Note that the parallel relative decline in retirement and disability program participation 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 – i.e., 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 here, and thus provide a sound basis for interpretation of the reform effects.

The relatively large incentive effects identified in our analysis also imply that changes in DI benefits will have a considerable impact on DI program participation. For example, for group A (model M4), we predict that a 10% rise in disability insurance income, ceteris paribus, will raise program participation at age 63 by 2.6 percentage points (36.7%).

Table 5. Average estimated marginal effects – 1947 cohort

	Group A – M4			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 M4/M8	0.573	0.355	0.071	0.670	0.237	0.093
+ 10% employment	0.652	0.290	0.058	0.732	0.193	0.075
income	(+13.8%)	(-18.3%)	(-18.3%)	(+9.3%)	(-18.6%)	(-19.3%)
+ 10% retirement	0.503	0.435	0.062	0.621	0.293	0.086
income	(-12.2%)	(+22.5%)	(-12.7%)	(-7.3%)	(+23.6%)	(-7.5%)
+ 10% disability	0.558	0.345	0.097	0.649	0.230	0.121
income	(-2.6%)	(-2.8%)	(+36.7%)	(-3.1%)	(-3.0%)	(+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				0.791	0.131	0.077
to pension wealth				(+2.1%)	(+3.1%)	(-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				0.548	0.276	0.176
to pension wealth				(-3.7%)	(-5.1%)	(+25.7%)

Based on Model M8, we can also assess the impacts of providing access at actuarially neutral terms to individuals' own pension wealth at a lower age. The results shown in Table 5 indicate that such access actually increases employment. For group B – who gained such access through

the pension reform – our estimates indicate a 1.5 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 much fewer entries into the DI program. For group B, we estimate that the gain of early access reduced DI program entry by 2.1 percentage point (21.4%), whereas for group C, the loss of early access increased entry by as much as 3.6 percentage points (25.7%).

Based on model M8, it is possible to decompose the overall change in behavior from the Norwegian pension reform, into factors related to changed incentives, changed access rules and changed composition of covariates.[6] The changes in incentives are then the actual changes resulting from the changes in pension rules, and, to a smaller degree, changes in tax rules. The main effects stem from the repeal of the earnings test. The results are shown in Table 6.

Table 6. A decomposition of behavioral changes from the pre-reform to the post-reform cohorts

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

Note: The decomposition is based on model 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. Line 4 is a simulation with both access and incentives set to post-reform values. Lines 5 and 6 show simulation 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.

The first three lines in Table 6 show the observed changes in behavior. Employment increased by 10.7 percentage points, mirrored by a decline in Retirement by 9.2 pp. and a decline in DI by 1.6 pp. The following four lines show the results of simulations with model M8. In the pre-reform sample (line four), the combined effect of the incentives and access is an increase in employment of 8.1 pp. and declines in Retirement and DI of 6.9 and 1.3 pp. 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 stem from incentives, in contrast to DI, where about one third is from access.

In comparison to the observed change from the pre-reform to the post-reform samples, we need to take into account also the changes in composition with respect to incentives and covariates. In line 7 we compare simulations on the pre-reform and post-reform samples, with post-reform incentives and access. The difference is 1.7 pp. higher employment in the post-reform sample and lower Retirement (-1.2 pp.) and DI (-0.4 pp.). The groups (A-D) are affected differently by the reform, and the relative group sizes change over time. In particular, groups C and D, are getting smaller, as more people qualify for early pension access over time. This accounts for part of the decline in disability which was simulated by access and incentives. The increase in

[6] Note 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 model M8.

employment not simulated by access and incentives is due to other changes in the composition, in particular in groups A and B.

Adding up the impact of incentives and access and the change in composition, we simulate 92 % (1.7 plus 8.1 pp, of 10.7 pp.) of the increase in Employment and 90 % (1.2 plus 6.9 pp. of 9.2 pp.) of the decline in Retirement. We overshoot the decline in DI by 6 % (0.4 plus1.3 pp. versus 1.6 pp).

6. Concluding remarks

It has previously been shown that a pension reform in Norway in 2011 led to a large increase in employment at ages above 62 years. In the present paper, we have examined 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 that there is a considerable interplay between these two exit routes with important implications for pension reform design. In particular, we emphasize three lessons: The first is that the elimination of earnings tests in the pension system not only seems to increase employment considerably, but also to reduce entry into the disability insurance 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. The second is that opening up access to own pension funds at 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 the workload and thus continue with some employment into higher ages. A final lesson is that early access to own pension funds leads to a considerable reduction in transitions into disability insurance. In essence, what early access seems to do is that it enables some workers to deal with their own health challenges themselves rather than to apply for disability benefits. It facilitates "self-insurance". Of course, this option will only be available for workers with sufficient pension savings, yet 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) disability insurance programs, it is probable that higher pension ages or more restrictive access to own pension savings leads to a "squeezed sausage effect" whereby more people are pushed into disability insurance.

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