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Pension Reform and Labor Supply – Flexibility vs. Prescription

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Abstract

We exploit a comprehensive restructuring of the early retirement system in Norway in 2011 to examine labor supply responses to pension reform strategies that rely on changes in work incentives (flexibility) or access ages (prescription), respectively. We find that increasing the returns to work is a powerful policy: The removal of an earnings test, implying a doubling of the average net take-home wage, led to an increase in average labor supply by 7 hours per week (30%) at age 63 and by 8 hours (46%) at age 64. The responses primarily came at the extensive margin.

Keywords: early retirement, labor supply, pension reform, program evaluation

Classification: D6, H3, J1, J2

1. Introduction

Demographic imperatives, especially rapid increases in mature age life expectancy, are leading many countries to encourage workers to work longer. Increased mature labor force participation may be induced by increasing the pension access age, or by allowing work decisions and pension access to be decoupled and chosen by individuals in response to a flexible, actuarially adjusted pension with annuitization at some legislated anchoring age. In this paper we estimate the labor supply responses in a reform package of this latter type.

Increasing the pension access age prescribes longer labor force attachment and a reduced life span of pension payouts. More than a dozen countries have either undertaken such a reform, or have announced plans to do so (OECD, 2012). This will almost certainly result in later retirement (Gruber and Wise, 1999; 2004), and in the absence of deferral adjustment, reduced future government expenditures. However, this is achieved in an inflexible way that imposes a uniform and often binding minimum retirement age on people who differ in terms of preferences, health, and job opportunities. Increasing the access age may also generate spillovers to disability insurance programs.

Allowing individual workers the flexibility to decide independently both the retirement age and the age at which to claim benefit payments has been undertaken in far fewer countries: the US in 1983 and 2000, Canada and Sweden in the 1970s, the UK in 1989, Japan in 1985 and 2002, and Norway in 2011. In an idealized reform of this type, benefits are actuarially adjusted by take-up age relative to an anchoring age, and future increases in longevity accounted for by annuitizing the pension wealth at the anchoring age. Increased longevity will then automatically result in reduced annual benefits. Earnings tests and related work disincentives are removed. Workers can then decide work patterns and pension drawdown independently, according to individual circumstance and preference.

In this paper we use the comprehensive 2011 Norwegian pension reform, which was primarily based on the latter reform strategy, to examine the labor supply responses to alternative reform paths. As we explain below, the reform had widely different implications for different groups of workers, depending on pre-determined factors such as sector of employment and accumulated pension entitlements. Some workers were subject to changes in access age only

(in both directions), some were subject to (large) changes in work incentives, and some were more or less unaffected. The reform therefore presents a surprisingly complete quasi-natural experimental set-up for our investigations. No other country has simultaneously varied access age in both directions, introduced significant age-graduated benefits, and abolished earnings tests.

Our analysis is based on two complementary empirical strategies, both based on comprehensive administrative registers with panel data on employment and earnings for the first birth cohorts who were potentially affected by the reform and the last cohorts who were not affected. The first approach is descriptive, comparing labor supply patterns before and after reform implementation for groups who were affected by it in different ways and directions. The second approach exploits the fact that individuals whose work incentives were affected by the reform were affected to varying degrees depending on their (predetermined) earnings levels and past earnings histories. We compute the work incentives (actual and hypothetical) faced under the pre- and post- reform regimes, and examine the relationship between labor supply and the two sets of work incentives, for both affected and unaffected cohorts. A causal relationship would predict that the partial positive correlation between labor supply and pre-reform and post-reform calculated labor supply incentives, respectively, would shift toward post-reform calculated incentives after the reform, whereas a purely spurious relationship would leave the correlation patterns unchanged.

Our paper relates to an existing literature which has indicated that earnings tests reduce labor supply both when the tests are "real" in the sense that benefits are not deferred (Baker and Benjamin, 1999; Brinch et al., 2015; Hernæs and Jia, 2013) and when benefits are merely deferred (Friedberg, 2000; Song and Manchester, 2007; Haider and Loughran, 2008; Engelhardt and Kumar, 2009; Disney and Smith, 2002). If labor supply and deferral choices are linked, a labor supply impact of earnings tests with actuarially fair deferral may be caused by workers perceiving the earnings test as a tax, possibly because the deferral schemes are complicated and poorly understood (Haider and Loughran, 2008), or because actuarial fairness does not apply to persons with high expected mortality (Engelhardt and Kumar, 2009). There is also a related literature indicating that reforms which solely reduce access to – or the generosity of – early retirement programs may embody the unintended side-effect of increasing the pressure on alternative subsidized escape routes from the labor market, such as disability

insurance programs (Duggan et al., 2007; Staubli and Zweimuller, 2011; Bratberg et al., 2004; Vestad, 2013; Røed and Haugen, 2002).

Our findings show that the flexibility approach has the potential to raise labor supply considerably. We find that the repeal of the early retirement earnings test in the private sector (leaving the access age of 62 years unchanged) increased average work-hours substantially, with aggregate labor supply elasticities of 0.31 and 0.45 at ages 63 and 64, respectively. Most of the labor supply response arose at the extensive margin, and the probability of staying on in the labor market with roughly the same work-hours and earnings as at age 60 rose by approximately 17 percentage points at both ages 63 and 64, from initial levels of 41 and 30 per cent, respectively. Although eliminating the earnings test may seem fiscally costly, we show that the behavioral response to the reform under consideration was such that the government budget actually benefited from it, as tax revenues rose more than pension outlays. We also find that *given actuarially fair work incentives,* the access age is of minor importance for labor supply behavior. Workers who as a result of the reform faced a lower access age with an actuarially fair early retirement pension (i.e., improved liquidity only) responded by reducing labor supply only slightly at the intensive margin, while maintaining the level of employment unchanged.

2. Institutional setting

Before the 2011 reform, the earliest access age for the public pension (hereafter referred to by its acronym FTP) in Norway was 67 years. But all public sector workers and more than 60% of private sector workers had access to a supplementary early retirement system (hereafter referred to by its acronym AFP), in essence offering a full pension from age 62. Both these pensions were subject to a full earnings test, implying that continued employment after retirement resulted in reduced lifetime pension entitlements. With a full pension, the earnings test became effective from the first dollar earned, such that labor earnings constituting a certain percentage of the pre-retirement earnings level resulted in same percentage cut in the annual pension.[1] There was no deferral option by delayed take-up, in effect implying very

[1] To avoid adjustments in cases of "negligible" labor earnings, there was a so-called "tolerance amount" of approximately $2,000 per year that could be earned without adjustment of benefits. All the monetary amounts reported in this paper are inflated to 2013-values using the Norwegian official pension benefit inflator, which in the period covered by this paper roughly corresponds to the wage growth, and then converted to USD ($) with exchange rate of mid-2013, $1=NOK 6.04.

high implicit tax rates on continued work. Hence, the AFP system embodied a strong disincentive to work after the age of 62, particularly for persons with relatively low wages.

The Norwegian 2011 pension reform changed both these systems radically, but the AFP was reformed only in the private sector. The reform implied large and immediate changes in the work incentives for many elderly workers. In this paper, we focus on two system parameters of paramount importance for labor supply: i) the earliest access age (EAA) and ii) the returns to continued work as determined by earnings tests and the degree of actuarial fairness in deferred pension entitlements.[2]

Adjustments to the FTP. The reform reduced the earliest access age to FTP from 67 to 62 years, thus giving all Norwegian workers access to a pension at the same age. Further, this early retirement option is based on an actuarially fair recalculation of annual benefits.[3] Hence, it embodies no work disincentives at all.

The new system is designed such that the decisions regarding the timing of pension claims and the timing of employment are decoupled; i.e., one is largely free to combine labor and pension income at will, as long as annual pension claims do not exceed the annuitized value of total pension wealth (lump sum withdrawal is thus not possible).[4] A partial pension can be taken in steps of 20, 40, 50, 60, 80 and 100% of the full annual pension. The percentage can be altered annually and a full pension can be taken out at any time.

The actuarial adjustment implies that the annual pension becomes lower with early withdrawal. A precondition for early take-up is that the actuarially adjusted pension entitlement ensures a pension level at age 67 at least as high as the minimum pension, which is effective from that age. A number of workers have such low entitlements that they are prevented from drawing a (full) pension at 62 and thus have to delay claiming, either until age 67 or until

[2] The reform implied a number of fundamental changes in the Norwegian public pension system which are *not* part of the evaluation in this paper; the most important being i) a transition from a system where pension point accumulation was based on the 20 years with highest earnings to a system where all years count equally much, and ii) the introduction of automatic longevity-adjusted annuities, implying that future increases in longevity will result in lower annual pension entitlements. These reforms will be implemented gradually, however, such that those who were close to retirement age at the time of the reform were completely unaffected by them.

[3] Deferral calculations are based on *average* life-expectancy within birth-cohorts. This implies that individuals with shorter (longer) life-expectancy than the average may find the deferral scheme disadvantageous (advantageous) for them, and thus choose to draw on their pensions as early (late) as possible, regardless of labor supply behavior.

[4] Given the progressivity of the Norwegian tax system, it may still be economically advantageous for some workers to postpone claiming the pension until they have reduced their annual labor earnings.

their adjusted entitlements provide a pension that at age 67 equals the legislated minimum, which is defined at age 67.

Adjustments to the AFP in the private sector. Concurrently with the public pension reform, the AFP was also radically changed into an actuarially fair system for all private sector workers. The earnings test was completely removed, and the AFP-pension was redesigned to become a life-long top-up annuity that could be taken only in combination with the FTP. As a result, work incentives improved dramatically for the workers covered by this pension system. Based on the detailed data used in this paper, we have computed that the average hourly net take-home wage (after tax and earnings test deductions) doubled, from $15 to $30 (implicit total tax rates declined from approximately 70 to 40 percent). But the removal of the earnings test also implied that the direct pension costs increased considerably. Our data indicate that although the average total life time benefit in the new AFP was approximately 28% lower than in the old one for persons who claimed the old AFP fully, and thus exited the labor market at age 62, the overall pension costs increased by around 32%, as the system now gave valuable pension entitlements to all covered workers. Hence, with unchanged labor supply behavior, the reform of AFP would clearly have added to – rather than alleviated –fiscal costs.

AFP in the public sector. In contrast to the private sector, the public sector AFP has not been reformed. It has preserved the pre-reform earnings test, and is still limited to the age range 62-67. Hence, workers in this sector of the economy continue to face strong labor supply disincentives. Moreover, the earnings-tested public sector AFP cannot be combined with early withdrawal of FTP, so that the liquidity option in the new FTP is open only by giving up the AFP option.

Consequences for different worker groups. Table 1 provides an overview of the main consequences of the reform for different worker groups, distinguished by i) their access to AFP and ii) their FTP entitlements at the earliest access age (EAA). Some of the groups identified in this table, particularly those who were subjected to changes in work incentives and/or access age (groups 2, 3, and 5) will play an important role in our empirical analysis. An important point to note is that group-assignment at the time of the reform was based on predetermined factors. Eligibility to AFP was determined by the employer's membership in the major Norwegian employer associations in combination with the worker's tenure and total work experi-

ence.[5] Entitlement to early take-up of FTP was determined by the worker's complete history of past earnings (the level of earnings in the "best" 20 years).

Table 1. The Norwegian 2011 pension reform – overview of main consequences for six different worker groups. By AFP affiliation and FTP entitlements at age 62 (percent of workers in parentheses).

	Entitled to full public pension (FTP) at age 62 after the reform	Not entitled to full public pension (FTP) at age 62 after the reform
AFP public sector	**Group 1** (28%) *No changes in either access age or work incentives.* New opportunity to start drawing on a full FTP from age 62 (with actuarial recalculation of benefits), conditional on giving up AFP entitlements.	**Group 4** (12%) *No changes in either access age or work incentives.* Depending on exact pension entitlements, a new opportunity to start drawing on a reduced (full) FTP at some time between age 62 and 66 (with actuarial recalculation of benefits) conditional on giving up AFP entitlements.
AFP private sector	**Group 2** (23%) *No change in the access age, but large improvements in work incentives.* Flexibility: Continuation of the opportunity to draw a full AFP/FTP from age 62. Complete removal of the old confiscatory earnings test (actuarial recalculation of benefits).	**Group 5** (3%) *Increases in the access age (reduced liquidity) and large improvements in work incentives.* Prescription: No longer possible to claim a full pension from age 62. Depending on exact pension entitlements, a new opportunity to start drawing on a reduced (full) FTP and AFP at some time between age 62 and 66 (with actuarial recalculation of benefits).
No AFP-entitlement	**Group 3** (23%) *Reductions in the access age (improved liquidity), but no changes in work incentives.* Flexibility: New opportunity to draw a full FTP from age 62. No earnings test adjustment (actuarial recalculation of benefits).	**Group 6** (11%) *No changes in either access age or work incentives.* Depending on exact pension entitlements, a new opportunity to draw on a reduced (full) FTP at some time between age 62 and 66 (with actuarial recalculation of benefits).

Note: The percentage distribution is based on the analysis sample described in the next section.

Announcements, Communication and Anticipation. Since the AFP system has been developed over several years through a tripartite agreement between the major associations of employers and employees and the state, the new reform package was also subject to negotiations between these parties. The negotiations took place in 2008 (the private sector) and 2009 (the

[5] The most important criteria for eligibility to the old AFP were the following: (i) current employment in a firm belonging to one of the major employer associations in the private sector (private sector AFP) or in the public sector (public sector AFP); (ii) at least 3 years' tenure with the present employer; (iii) at least 10 years of work experience since the age of 50; and (iv) an average of the 10 highest yearly incomes after 1966 exceeding an amount corresponding to approximately one-third of average full-time earnings. In the new private sector AFP, the second (tenure) criterion has been modified to require employment in at least seven of the last nine years in a firm offering private sector AFP.

public sector). From around May 2009, we can assume that all the main elements of the new early retirement system were known to the workers; i.e., around two years before the reform's implementation. This includes the "new" concept of actuarially fair deferral, which was forcefully communicated by policy makers and unions, as well as by the media. By this stage it was typically no longer possible for the workers to switch between the different AFP systems by changing employer, as access to AFP entitlements in both the private and most of the public sector requires several years of sector-specific tenure.

Transitional rules. Persons in the private sector in the 1948 cohort, who reached the age of 62 in 2010, the last year before the implementation of the reform in 2011, had the possibility of choosing between the "old" earnings tested AFP (which then had to be taken out before January 1, 2011) or waiting until January 2011 to become eligible for the non-earnings-tested "new" AFP: life-long, but with a lower annual amount. Similar, but considerably less valuable, options were offered to those in the four preceding cohorts who had not taken up the old AFP. The 1947 cohort (whose members were 63 at the time of the reform) were offered 60% of the normal new AFP, the 1946 cohort 40%, the 1945 cohort 20%, and the 1944 cohort 10%.

3. Data and descriptive evidence

The analyses in this paper are based on individual data from merged administrative registers. These files are linked by unique encrypted personal identification numbers, and cover the entire population of Norway. The data provide detailed information on individual characteristics and labor market histories. They are not subject to the self-reporting and attrition problems common in survey-based data. Outcomes of interest include employment, hours worked, and social insurance claims (based on records from the social insurance administration), recorded in the calendar year workers reached age 63 and 64.

Observations on employment and hours worked are derived from administrative records on annual earnings, with the hourly wage rate imputed from earnings and work-hours recorded at age 60, which are available for all persons used in our analysis. Since earnings data are accurately recorded at the annual level only, all outcomes are measured as calendar year averages. We avoid the year persons reached 62, since the reform effects in this year depend on the exact date of birth and since we only have access to annual earnings data. We focus on the actual level of labor supply, measured by the average weekly hours of work, as well as on

various qualitative outcomes describing labor market behavior relative to behavior at age 60 - two years before any early retirement option became available. In particular, we will be interested in the propensity to continue working more or less as before, as opposed to continuing with reduced hours or withdrawing from the labor force altogether.

Our empirical analysis is based on workers who at age 60 were employed and did not receive any form of disability insurance payment. The reason why we condition our analysis on employment at age 60 rather than 61 is that we wish to minimize possible endogeneity problems associated with early (*ex ante*) responses to changes in future work incentives. Since the new early retirement system was formed through the wage agreements in 2008 and 2009 this does not necessarily eliminate the risk of endogeneity completely, as the members of the latest cohort used in our analysis (1950) reached the age of 60 in 2010 and, hence, could respond to the new incentive structures already in this year, e.g., by working more or less than they otherwise would have done. In order to assess robustness with respect to this potential endogeneity problem, we have therefore also done the empirical analysis conditioned on employment at age 58 instead. As this did not change the results to any noticeable extent beyond introducing some extra measurement error in the mapping of persons into the six groups described in Table 1, we have relegated these results to an Appendix.

Our empirical strategy relies on two complementary approaches. The first, and purely descriptive, approach exploits the fact that different groups of worker-types were affected in completely different ways by the reform – some were subject to new work incentives, some were subject to changes in the earliest access age, some were subject to a combination of the two, and some were subject to almost no changes at all (Table 1). By comparing labor market outcomes for these worker groups just before and just after the reform potentially made its influence on work incentives and/or access age, we seek to identify the shifts that are directly attributable to the reform. The second approach, to which we return in the next section, is based on econometric models where we seek to identify and quantify the causal relationship between work incentives and labor supply at age 63 and 64, and also the impacts of liquidity as measured by the lowest access age in the pension system.

Our analysis will be based on a comparison between the 1946-1947 birth cohorts – who reached 63 years in the two years prior to the reform (2009-2010) – and the 1949-1950 cohorts – who reached 63 in the years after the reform year (2012-2013).[6]

From a macroeconomic viewpoint, the outcome period used in our statistical analysis (2009-2013) was relatively stable in Norway, with the aggregate (registered) rate of unemployment varying between 2.5 and 2.9% (2.7% of the labor force in 2009, 2.9% in 2010, back to 2.7% in 2011, down to 2.5% in 2012, and up again to 2.6% in 2013); hence, we can more or less rule out that any significant changes in employment patterns were generated by cyclical fluctuations.

Table 2 provides descriptive statistics for the six groups distinguished in Table 1, including a number of pre-reform and post-reform labor market outcomes. Figure 1 gives a graphical illustration of the development of labor supply by age for the pre- and post-reform cohorts, in the form of average weekly hours worked. Interpreting the differences in outcomes between pre- and post-reform cohorts after the age of 62 as indicative of the reform's causal effects, it is notable that it is only in the two groups in which work incentives were radically changed by the reform (groups 2 and 5) that we see any clear signs of labor supply responses.[7] The differences between the pre and post-reform cohorts identified for groups 2 and 5 represent sizeable impacts. For both groups, the reform apparently caused the average weekly hours of work at age 63 to increase by 6.7 hours. At age 64, the implied reform effect is 7.6 hours in Group 2 and 7.1 hours in Group 5. For both groups, these responses primarily came about through increases in the fractions that continued to work more or less as before; i.e., as they did at age 60; see Table 2. The apparent similarity in response to the reform across these two groups is striking, given that the Group 5 members, in addition to experiencing the same kind of incentives improvement as Group 2 members, also lost the opportunity to claim full pen-

[6] Because of the special incentives embodied in the transitional rules explained in Section 2, the 1948 cohort is dropped from the analyses. Even workers belonging to the two pre-reform cohorts (1946-1947) were in principle affected by the reform from age 65 or 64, respectively, as they were then allowed to claim a strongly reduced "new" AFP, provided that they had not already taken out the "old" AFP at that point. This could possibly have triggered higher labor supply already at age 63 and thus generated a corresponding reform effect even in our control group. While we will show here that there are no indications of such an effect in the data, it is worth noting that this would make us underestimate the true effects of the reform, and hence that the impact estimates reported for Group 2 are on the conservative side.

[7] In a previous working paper version of this paper (Hernæs et al., 2015), we show that there were no pre-reform trends in labor supply behavior at age 63 for the 1944-47 birth-year cohorts in any of six groups, with a possible exception for the public sector workers in Group 1, where there were indications of a slight pre-reform increase.

sion benefits already at age 63. We do not see any clear indications of reform-initiated labor supply responses at all in Group 3, even though members of this group gained the opportunity to start drawing on their FTP entitlements five years earlier than before. It appears that the early retirement access age is of minor importance relative to work incentives.

Table 2. Descriptive statistics

	Group 1 Public AFP Access to early FTP		Group 2 Private AFP Access to early FTP		Group 3 No AFP Access to early FTP		Group 4 Public AFP No access to early FTP		Group 5 Private AFP No access to early FTP		Group 6 No AFP No access to early FTP	
	Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform
Number of observations	18,084	19,305	15,787	15,330	15,363	15,290	8,700	7,582	2,713	2,000	8,040	6,838
Baseline characteristics												
Women (%)	47.3	52.7	17.5	20.2	12.2	14.3	96.8	96.0	91.2	88.4	71.7	69.7
Immigrants (%)	0.9	1.5	1.0	1.5	0.6	1.3	2.6	5.6	5.9	9.8	4.0	6.6
High school (%)	30.8	32.4	62.1	64.7	55.0	54.8	60.4	61.7	60.7	60.3	59.3	58.4
College (%)	63.9	62.5	19.1	18.4	27.7	30.6	18.1	19.5	4.6	5.3	13.4	16.6
Labor earnings age 60 ($1000)	91.9	92.2	104.9	101.5	103.7	105.3	55.9	57.6	57.2	55.1	54.1	55.8
Weekly work hours age 60	39.7	38.9	41.4	41.1	37.9	37.9	32.7	32.9	33.1	33.0	30.1	30.9
Outcomes												
Labor earnings ($1000)												
Age 63	70.3	72.4	58.7	72.8	84.0	84.0	41.0	42.3	31.1	48.3	42.6	44.5
Age 64	61.6	65.4	45.2	62.3	76.1	75.0	35.3	37.2	23.7	35.2	38.1	39.6
Weekly work hours												
Age 63	29.7	30.0	22.1	28.8	30.7	29.9	23.8	24.0	17.8	24.5	24.2	24.3
Age 64	25.7	26.7	16.7	24.3	27.4	26.9	20.4	21.2	13.4	20.5	21.6	21.6
Working as before (%)												
Age 63	61.5	65.7	40.8	57.4	62.2	61.2	58.2	59.9	39.6	61.5	59.6	60.9
Age 64	51.5	55.8	29.5	46.1	54.1	52.2	48.0	50.6	28.2	47.3	51.1	51.7
Working reduced hours (%)												
Age 63	26.1	24.7	35.3	30.2	27.1	28.5	23.8	24.3	32.6	25.7	26.7	25.9
Age 64	25.4	25.7	26.7	30.3	30.9	32.9	21.3	22.9	24.4	30.2	28.1	28.9
Retired without DI (%)												
Age 63	10.4	8.0	20.9	9.5	5.9	7.0	13.6	11.9	21.3	4.5	7.2	7.2
Age 64	19.4	15.7	39.3	19.1	9.3	10.4	23.7	19.6	38.7	9.3	10.7	10.1
Retired with DI (%)												
Age 63	2.0	1.6	3.0	2.9	3.7	3.2	4.4	3.9	6.4	8.4	6.5	6.0
Age 64	3.7	2.7	4.4	4.4	5.7	4.5	7.0	6.9	8.7	13.3	10.0	9.3

Note: For age 64 outcomes, data for the 1950-cohort are not available; hence post reform outcomes at age 64 are based on the 1949-cohort only. Working "as before" is defined as having work hours at least as high as 80% of work hours recorded at age 60. "Retired without DI" is defined as being non-employed (earn less than $2000) and not receiving any disability insurance benefit. "Retired with DI" is being non-employed and receiving a disability insurance benefit.

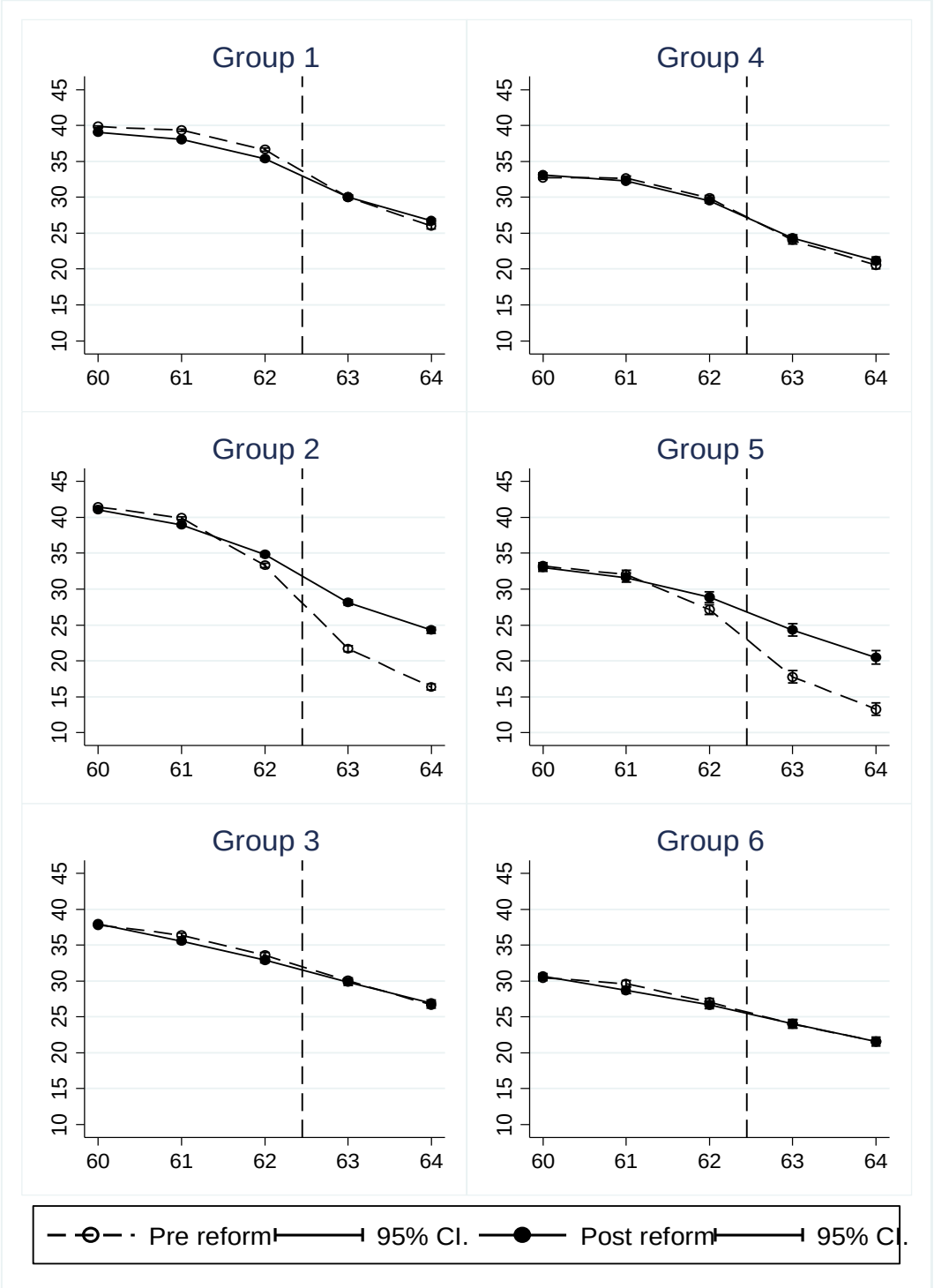

Figure 1. Average weekly work hours at age 60-64 for pre-reform cohorts (born in 1946-47) and post-reform cohorts (born in 1949-50), conditional on employment at age 60, with 95% confidence intervals.

Note: For the number of observations, see Table 2. Due to data limitations, the post-reform outcomes at age 64 are based on the 1949 cohort only.

To assess the margins at which the labor supply responses took place, Figure 2 depicts the employment and retirement patterns for the treated and the non-treated cohorts in the groups which experienced changes in incentives and/or access age. The three upper panels illustrate the steep decline in employment that coincided with access to early retirement in groups 2 and 5 prior to the reform, and how the reform generated a more gradual exit pattern. The six lower panels show the extent to which workers pulled completely out of the labor force with and without claiming a disability insurance benefit, respectively. In group 2, where the members were subjected to improved work incentives only, we see that the increased labor supply was mirrored in a corresponding reduction in voluntary labor market exits (without disability insurance). In group 3, where the members were subjected to improved liquidity only, and labor supply was more or less unchanged, we see a drop in disability insurance claims matched by a corresponding increase in voluntary retirement. And in group 5, where members were subjected not only to improved incentives, but also a *reduction* in liquidity, we see the opposite development, i.e., a shift from voluntary retirement to disability program participation. Hence, there appears to be an element of substitutability between voluntary retirement and disability program participation: Raising the pension fund access age, this reducing liquidity, raises the demand for disability insurance.

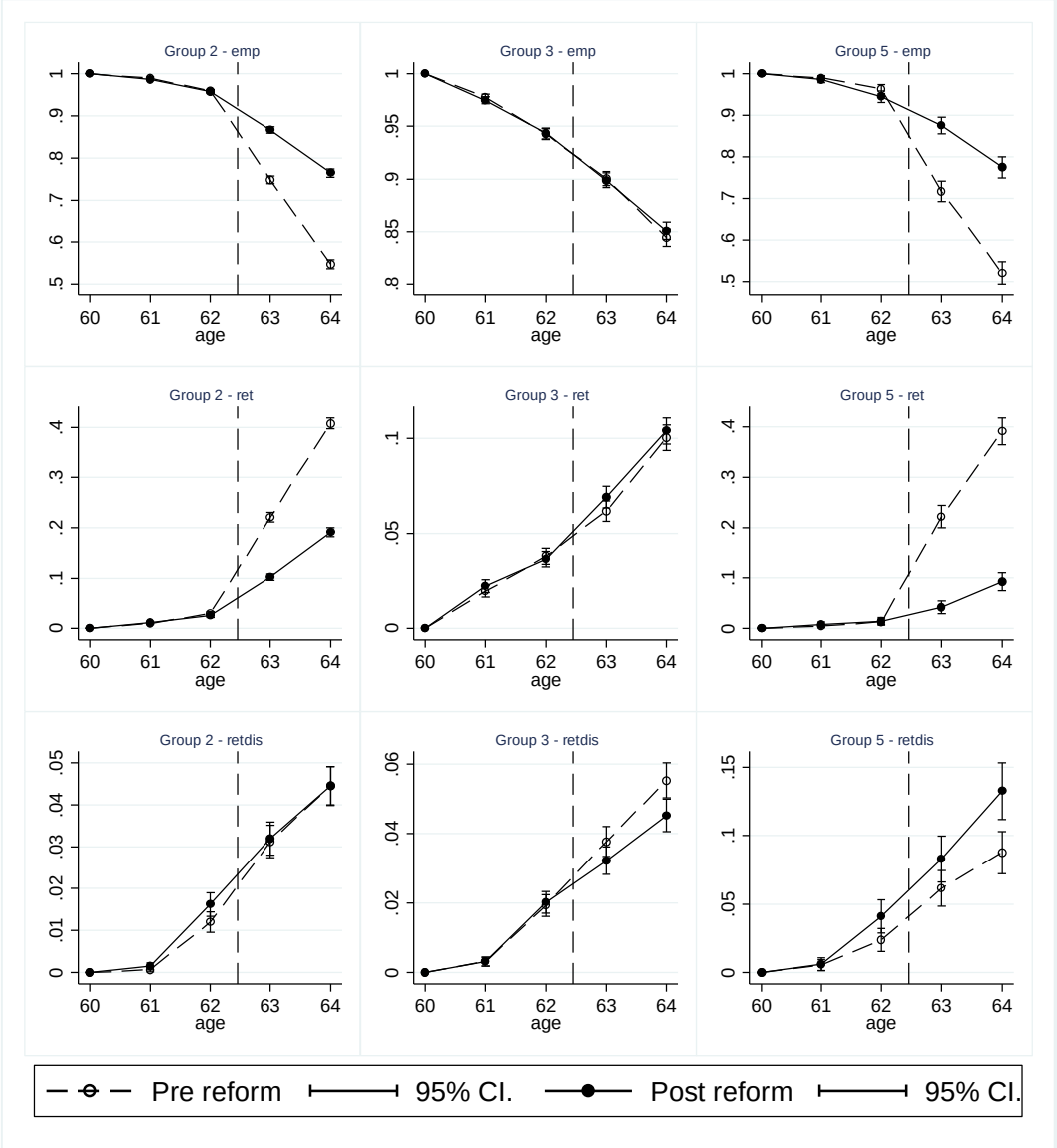

Figure 2. Labor market outcomes at age 60-64 for pre-reform cohorts (born in 1946-47) and post-reform cohorts (born in 1949-50), conditional on employment at age 60. With 95% confidence intervals.

Note: For the number of observations, see Table 2. Due to data limitations, the post-reform outcomes at age 64 are based on the 1949 cohort only

The nature of the data and policy differentials identified in Table 1 and Figure 1 at first sight suggests an econometric analysis based on a simple difference-in-difference approach, whereby the largely untreated groups (Groups 1, 4, 6 in Table 1) serve as controls for the treated groups (Groups 2, 3, and 5). However, this is problematic for two reasons. The first is that although the public sector workers in Group 1 did not experience any change in the re-

turn to continued work, they did get the option of starting to draw their FTP five years earlier than before. Even though this required that they gave up their AFP, around one fifth took this opportunity. As can be seen from Figure 1, the decline in labor supply at age 63 was also a bit smaller for the post-reform than for the pre-reform cohorts, indicating that the opportunity to combine work and access to the FTP was valued. The public sector workers in Group 1 must therefore be viewed as "partly treated" and not well suited for a difference-in-difference approach. The second reason is that the treatment and control groups not only differ in terms of treatment exposure, but also in terms of their composition, particularly with respect to gender, education, and initial earnings and labor supply levels (at age 60); see Table 2. For example, while there are almost 97% women in Group 4, there are less than 20% women in Group 2.

A standard difference-in-difference approach is also unattractive for the reason that it disregards the large variation in reform-initiated incentive changes within the groups of treated workers, and thus fails to relate the labor supply responses to the intensity of incentive improvement. In the econometric analysis in the next section, we therefore focus exclusively on the treated worker groups, and seek to quantify the labor responses to pure changes in work incentives or access age.

4. Econometric analysis

To motivate and frame our econometric analysis, we first discuss the effects we seek to identify within the framework of a simple stylized theoretical model.

4.1. Theoretical background

Consider the decision problem facing workers approaching an age of *potential* retirement. To keep things simple, we assume that agents maximize a two-period utility function, where the first period corresponds to the period in which continued work is a realistic option and the second period corresponds to a period of full retirement. Disregarding discounting, the inter-temporal utility function may then be expressed as $U = u_1(c_1, l) + u_2(c_2)$, where c_1 and c_2 are consumption in the first and second period, respectively, l is the amount of leisure, and the two *u*-functions satisfy the standard requirements of concavity and positive first order derivatives. As a starting point, we may assume that agents also face an inter-temporal budget restriction of the form $c_1 + c_2 \leq w(1-l) + W + P$, where w is the wage (which we assume fixed

at the individual level), W is liquid private wealth and P is pension wealth.[8] Without credit constraints, and with the two wealth variables (W, P) considered as given, we obtain the standard results that the marginal utility of consumption is equalized across the two periods, and labor supply is determined such that the marginal utility of leisure divided by the marginal utility of consumption equalizes the net wage. Abstracting from distortions outside the pension system, individual choices are optimal also from a social point of view.

In the Norwegian pre-reform early retirement system, the pension wealth *P* was not at all *given.* Instead it involved an earnings test such that the pension wealth was reduced proportionally to actual annual earnings relative to "normal" earnings in the years prior to the early retirement age.[9] We can write this as $P = P*[1-(w(1-l)/w(1-l^*))]$, where $P*$ is the maximum pre-reform pension wealth and $(1-l^*)$ is the labor supplied prior to the early retirement access age. The pre-reform net wage after having reached the early retirement access age thus becomes $w - P^*/(1-l^*)$. For workers with high pension entitlements relative to the last years' earnings, the net wage could be extremely low, and in some cases even negative. The reform of the private sector AFP essentially removed the extra tax implicit in the earnings test and set $P = P**$, with $P** < P*$, as explained in Section 2. Hence, the reform not only changed slope of the budget line, it also changed its location; i.e., the consumption possibilities in the full retirement state.

[8] For simplicity, we disregard general income taxation in this sub-section. These are re-introduced in our empirical specification of work incentives in the next sub-section.

[9] Normal earnings were defined as the average earnings in the three best out of the five last years prior to the year of early retirement.

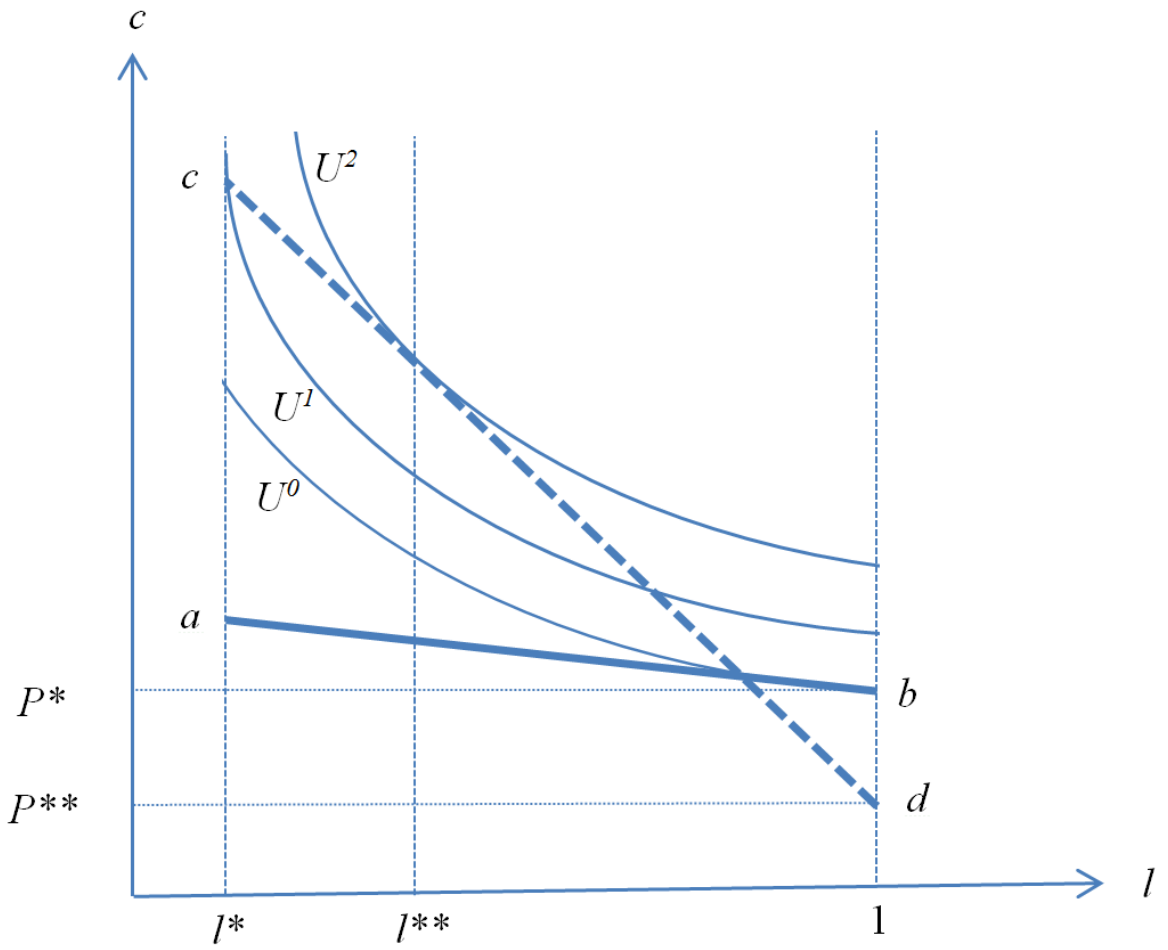

Figure 3. An example of labor supply behavior with pre-reform and post-reform budget lines for workers in Group 2.
Note: The pre-reform budget line is marked *ab* and the post-reform budget line is marked *cd.*

Abstracting from the intertemporal nature of the decision problem, Figure 3 shows in a simplified form how the reform changed the situation for AFP-eligible workers in the private sector, and illustrates with an example what kind of responses we can expect to see.

In theory, individuals can choose to continue working more or less as before; to pull out of the labor force; or to seek some compromise with reduced work hours.[10] While the two first alternatives will almost always be available to the worker (due to employment protection legislation), the alternative of continuing with reduced hours either requires the existing employer's consent or the ability to find new employment. The latter is typically considered to be difficult at early retirement age. Hence, the choice set may in many cases be confined to

[10] It is obviously also possible to *increase* the labor supply at the age of potential retirement. But this rarely happens in practice, and we are going to abstract from that alternative in this subsection.

the alternatives of continuing as before or retiring completely. Looking at the data, we actually find that virtually the whole increase in employment at age 63 from the pre- to the post reform cohorts came in the form of more workers continuing with the same employer as they had at age 60.

The line *ab* is the budget line prior to the reform, whereas *cd* is the budget line after the reform. Based on the pre-reform budget line, our exemplar agent would have chosen the corner solution of pulling out of the labor market (*l=1)*, and obtained a utility level corresponding to U^0. Based on post-reform incentives, utility is maximized at the level U^2 by choosing the internal optimum $l=l^{**}$.

This alternative may not be available, however, such that the agent must choose between $l=l^*$ (the pre-existing employment contract) and $l=1$ (full retirement). As we have drawn the indifference curves in Figure 3, $l=l^*$ is then the preferred choice, and with the utility level U^1 the agent still obtains higher welfare with post-reform than with pre-reform incentives.

Figure 3 also illustrates that the labor supply responses to the early retirement reform potentially involves both substitution and income effects. As both pension entitlements (P^*, P^{**}), wages (w), and initial employment (l^*) vary across individuals, the slopes and locations of budget lines will also vary. As explained above, in some cases the pre-reform budget line will be almost flat, or even upward sloping. Plausible examples can easily be constructed where the shift from the pre-reform to the post-reform budget line entails an approximately unchanged utility level, and thus generates a pure substitution effect. But in many cases, it is likely that the new incentives involve either a welfare improvement or a welfare reduction, in which cases the labor supply responses also reflect income effects. We return to this issue when we interpret our empirical findings in the next subsection.

For most AFP-eligible workers in the private sector who belonged to the first cohorts affected by the pension reform (Group 2 in Table 1), the shifts in the slope and position of the budget line were the *only* reform element of importance; hence, we can use these workers to examine the impacts of a pure incentives improvement. And with detailed information about the magnitude of incentives improvement at the individual level, we can relate the labor supply responses quite accurately to the intensity of incentive change.

Another potential labor supply distortion comes from liquidity constraints caused by restricted access to the pension wealth *P*. In the context of our simplified model, a "prescription reform" which raises the access age can be represented by adding to the worker's optimization problem the constraint that period 1 consumption cannot exceed the sum of period 1 labor earnings and private wealth; i.e., $c_1 \leq w(1-l)+W$. With little private wealth (or wealth/earnings from other household members), this restriction may very well be binding, and hence compel the worker to supply (more) labor in period 1. An important element of the Norwegian pension reform was to *reduce* the access age to FTP from 67 to 62 years; i.e., a prescription reform in reverse, albeit with actuarial adjustment. For most private sector workers without AFP (Group 3 in Table 1), this was the only reform element of importance. Hence, we can use these workers to examine the labor supply responses to a pure change of access age within an actuarially fair pension system – isolating the influence of liquidity.

As a result of the Norwegian pension reform, there was also a small group of AFP-eligible workers who were exposed to an *increase* in access age (Group 5 in Table 1), as their pension entitlements were too small to make them eligible for early claiming in the new system. These workers were also subjected to the incentive improvement following from the removal of the earnings test. Hence they were exposed to the combination of improved incentives and reduced liquidity.

To isolate the impacts of the two main policy instruments that are relevant in a pension reform context, in the remainder of this section we examine more closely the labor supply behavior in the two groups that were exposed to either pure incentive and income changes (Group 2) or to pure liquidity changes (Group 3). While we will focus on the overall impacts on labor supply in the form of average work hours, we are also interested in the frequencies of alternative response types, e.g., continuing work approximately as before, continuing with reduced hours, or withdrawing from the labor force (with or without disability insurance benefits). The descriptive evidence presented in the previous section clearly indicated that the reform primarily affected labor supply by changing the propensity to continue working in an existing job. To reiterate, this probably reflects the restricted job opportunities that mature workers have. In practice, they often have to choose between the two "corner solutions": Continue with the job they already have or pull out of the labor market.

4.2. Work incentives

We now turn to a more formal statistical analysis intended to exploit the reform-initiated variation in incentives across cohorts in Group 2 to quantify the causal relationship between labor supply and work incentives. In general, we do not expect labor supply incentives to be randomly assigned, as persons with different wage levels are likely to differ systematically along other dimensions as well, such as health status, motivation, job characteristics, and valuation of leisure. Hence, in order to facilitate estimation of the *causal* relationship between work incentives and labor supply, we need to isolate the random-assignment-like variation generated by the reform. We do this by estimating regressions where we not only include the actual work incentive as the central explanatory variable, but also add in the corresponding *hypothetical* pre-reform and post-reform work incentives. The basic idea is that while any spurious correlations between labor supply and pre and post-reform work incentives should be the same before and after the reform, the causal correlation should shift toward the incentives *actually applying*. Hence, by studying changes in the correlation patterns pre- and post-reform, we can trace out the causal effects.

More specifically, we set up regression models that link labor market outcomes directly to the economic returns to work for the members of Group 2:

$$y_i = \mathbf{x_i}'\lambda + \delta\Delta_i^O + \gamma\Delta_i^N + \varphi\left[(1-R_i)\Delta_i^O + R_i\Delta_i^N\right] + \varepsilon_i, \qquad (1)$$

where (Δ_i^O, Δ_i^N) are individual i's net economic returns to work (to be specified more precisely below) as they *would have* applied under the old (Δ_i^O) and new (Δ_i^N) pension systems, respectively, and R_i is a dummy variable equal to 1 for workers belonging to the reform cohorts (1949-50) and equal to 0 for the pre-reform cohorts (1947-48). The term $(1-R_i)\Delta_i^O + R_i\Delta_i^N$ thus gives the work incentive actually applying for both the pre- and the post-reform cohort members. The vector of control variables $\mathbf{x_i}$ includes gender, education (nine fields and eight levels), country of origin for immigrants (five regions), and labor earnings and weekly hours of labor supply at baseline (age 60).

The coefficient of interest is φ, which represents the causal effect of the work incentive on the outcome variable y_i. Note that it is the "extra" correlations between incentives and outcomes that arise in the periods in which the two respective incentive variables actually apply

that traces out the causal effect. Identification then hinges on the assumption that the pre- and post-reform cohorts do not behave differently for reasons unrelated to the reform; i.e., that R_i is uncorrelated with ε_i.

We specify the work incentives (Δ_i^O, Δ_i^N) as the net hourly (take-home) wages. Abstracting from the tax system, we would have had $\Delta_i^O = w - P^* / (1 - l^*)$ and $\Delta_i^N = w$; see the previous subsection. However, when we take the complete tax system into account (including the degree of progressivity and the differential treatment of pension and labor earnings), the net hourly wage rate becomes dependent on the level of labor supply. Hence, in order to use (Δ_i^O, Δ_i^N) as exogenous explanatory variables, we compute the net hourly wage associated with a fixed labor supply level. Since the descriptive evidence in the previous section indicated that the reform primarily changed the probability of continuing working "as before", we have chosen to compute the net hourly wage at the baseline (age 60) level of labor supply; i.e., (Δ_i^O, Δ_i^N) are the net hourly wages at age 63 and 64 derived from continuing working as at age 60. Figure 4 illustrates how these net hourly wages changed as a result of the reform. It is evident that the reform shifted the whole work incentives distribution to the right, and for the vast majority of workers, the hourly take-home wage increased by between \$13 and \$18.

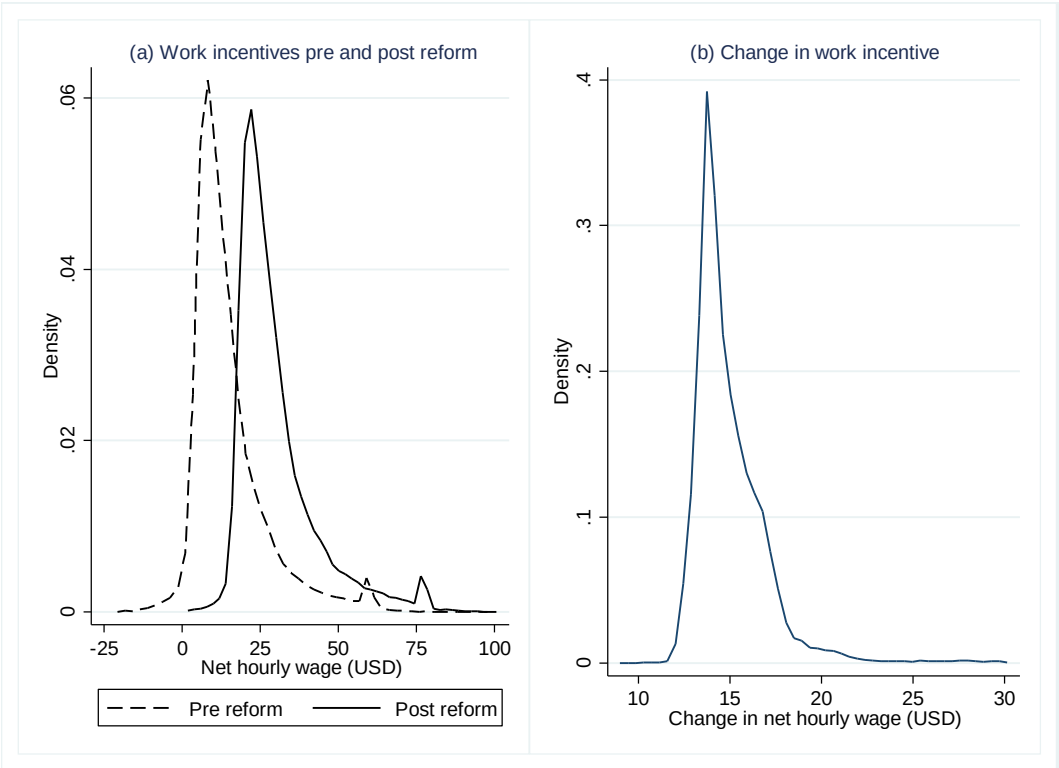

Figure 4. The distributions of predicted net hourly wage rates at ages 63 and 64 with pre- and post-reform rules (Group 2 only).
Note: Net hourly wage rates are predicted on the basis of the calculated gross wage rates at age 60.

The main outcome of interest is the weekly number of work-hours supplied, but we also examine discrete outcomes measuring labor supply at age 63/64 relative to labor supply at age 60. With the number of work-hours used as the dependent variable in Equation (1), it may be argued that the relationship between the wage rate and hours worked should be formulated in terms of logarithms, enabling the computation of labor supply elasticities. This is problematic in the present setting, however, both because many workers in the pre-reform cohorts had extremely low, and in some cases even negative, net wage rates, and because persons who exit the labor market work zero hours. We thus estimate the relationship between hours worked and the net wage with a linear model. However, to facilitate comparison with findings in the existing literature, we compute labor supply elasticities at more aggregate levels; i.e., we report the percentage changes in average labor market outcomes relative to the percentage change in average incentives for groups of workers.

Table 3. Estimated labor supply effects of the net hourly wage rate

	I Marginal effect measured in hours or percentage points (pp.) of a dollar increase in net wage (standard error)	II Implied aggregate elasticity [95% confidence interval]
Weekly number of work-hours		
Age 63	0.394 hours*** (0.019)	0.305*** [0.263, 0.345]
Age 64	0.453 hours*** (0.019)	0.452*** [0.380, 0.509]
Probability of working "as before" (>80% of age 60 hours)		
Age 63	0.969 pp. *** (0.051)	0.414*** [0.354, 0.476]
Age 64	0.984 pp. *** (0.054)	0.570*** [0.471, 0.673]
Probability of working with reduced hours		
Age 63	-0.292 pp. *** (0.042)	-0.159*** [-0.214, -0.103]
Age 64	0.206 pp. *** (0.050)	0.129*** [0.031, 0.206]
Probability of being retired without disability insurance		
Age 63	-0.664 pp.*** (0.039)	-0.651*** [-0.727, -0.576]
Age 64	-1.185 pp. *** (0.052)	-0.594*** [-0.670, -0.512]

Probability of being retired with disability insurance		
Age 63	-0.013 pp. (0.012)	-0.118 [-0.349, 0.085]
Age 64	-0.005 pp. (0.016)	-0.051 [-0.310, 0.136]

Note: Column I reports the estimates of the coefficient φ in Equation (1). The control variable vector $\mathbf{x}_i$ includes gender, education (nine fields and eight levels), country of origin for immigrants (five regions), and labor earnings and weekly hours of labor supply at baseline (age 60). Column II reports aggregate elasticities calculated as the predicted reform-initiated percentage change in the average outcome divided by the percentage change in the average net take-home hourly wage. Number of observations: 31,117 for age 63 outcomes and 23,385 for age 64 outcomes. Standard errors are clustered on educational groups (6 levels and up to 10 fields, in total 34 educational groups) and birth cohort, in total 136 clusters. The estimate and confidence intervals for the aggregate elasticities are based on clustered non-parametric bootstrap with 500 trials.
* (**) (***): Significant at the 10 (5) (1) % level.

Our main results are presented in Table 3. Each dollar increase in the net hourly wage raises weekly labor supply at age 63 by 0.394 hours and at age 64 by 0.453 hours. The labor supply response comes about almost exclusively by inducing more workers to stay on with approximately the same level of employment that they had at age 60. For each dollar increase in the hourly net wage, the probability of staying on "as before" rises by approximately 1 percentage point at both ages 63 and 64. However, while this response to some extent comes about through a reduction in reduced-hours work at age 63 it is accompanied by an increase in reduced-hours work at age 64. Hence, the overall labor supply response is considerably larger at age 64 than at age 63. This reflects the absorbing nature of labor market exits at high ages. Those who leave employment at age 63 due to poor work incentives rarely return to work at age 64 (or later); hence each year's new retirees just add to the total number of exits.

The absorbing nature of retirement also means that there is an option value to continuing to work, which is not reflected in our work incentives variables (Stock and Wise 1990, Gruber and Wise, 1999). Although this holds both before and after the reform, it is reasonable to assume that the option value is greater after the reform, as the annual returns to work are larger. Therefore, we probably underestimate the increase in the incentive and overestimate the per dollar labor supply responses. Yet, our estimates do appropriately represent the behavioral responses to the repeal of the earnings test.

For the group as a whole, our estimates imply labor supply elasticities equal to 0.31 at age 63 and 0.45 at age 64. This is at the higher end of typical estimates of the compensated (Hicksian) labor supply elasticities reported in the literature; see, e.g., the recent reviews in Chetty et al. (2011a; 2011b), McClelland and Mok (2012), and Chetty (2012). Given that our esti-

mates also embody income effects that presumably have dampened the substitution effects somewhat (see the previous subsection) this may at first sight appear surprising. However, an important distinguishing feature of our analysis is that we examine the labor supply behavior in a population where everyone is employed to start with (at age 60), and hence can be assumed to have at least one feasible employment option (due to employment protection legislation). Moreover, we study these individuals at a time where non-employment is a realistic alternative, given that they have obtained access to their pension wealth. Finally, the incentive changes we use to identify the labor supply responses are large, compared to margins of variation typically encountered in the literature, implying that inertia caused by frictions of the type discussed in Chetty (2012) is of minor importance.

As pointed out above, the elasticity concept is somewhat problematic in our setting, given the large number of zero outcomes and, in particular, the wide range of reform-generated relative incentive changes. To illustrate this point, we have divided the Group 2 population into 10 deciles based on the size of the percentage change in the net hourly wage rate generated by the reform, and estimated the models separately for each group. Figure 5 reports the resulting estimated marginal effects on hours worked at age 63, as well as the implied group-elasticities (defined as the percentage change in average labor supply divided by the percentage change in average work incentives). On the horizontal axis, we report the average percentage net wage increase generated by the reform in each decile; hence, it can be seen that these incentive improvements varied from around 40 to more than 400 percent.

A first point to note from Figure 5 is that the marginal effects are relatively stable across the 10 groups, indicating that our linear model specification probably represents the data fairly well. A second point is that the implied group-elasticities decline monotonically with the size of change in the relative incentives. One reason for this appears to be that there are “natural” limits to the size of the labor supply responses in our setting, given that some persons choose to work almost regardless of work incentives. In our data, the maximum reform response across the 10 deciles appears to be a labor supply increase at age 63 of approximately 30-40%, and responses of this magnitude are achieved in all groups experiencing net wage increases exceeding 100%. Finally, for the more moderate incentive changes observed in the first decile (with an increase in average net wage of 40%), the implied labor supply elasticity is considerably larger than for the population as a whole.

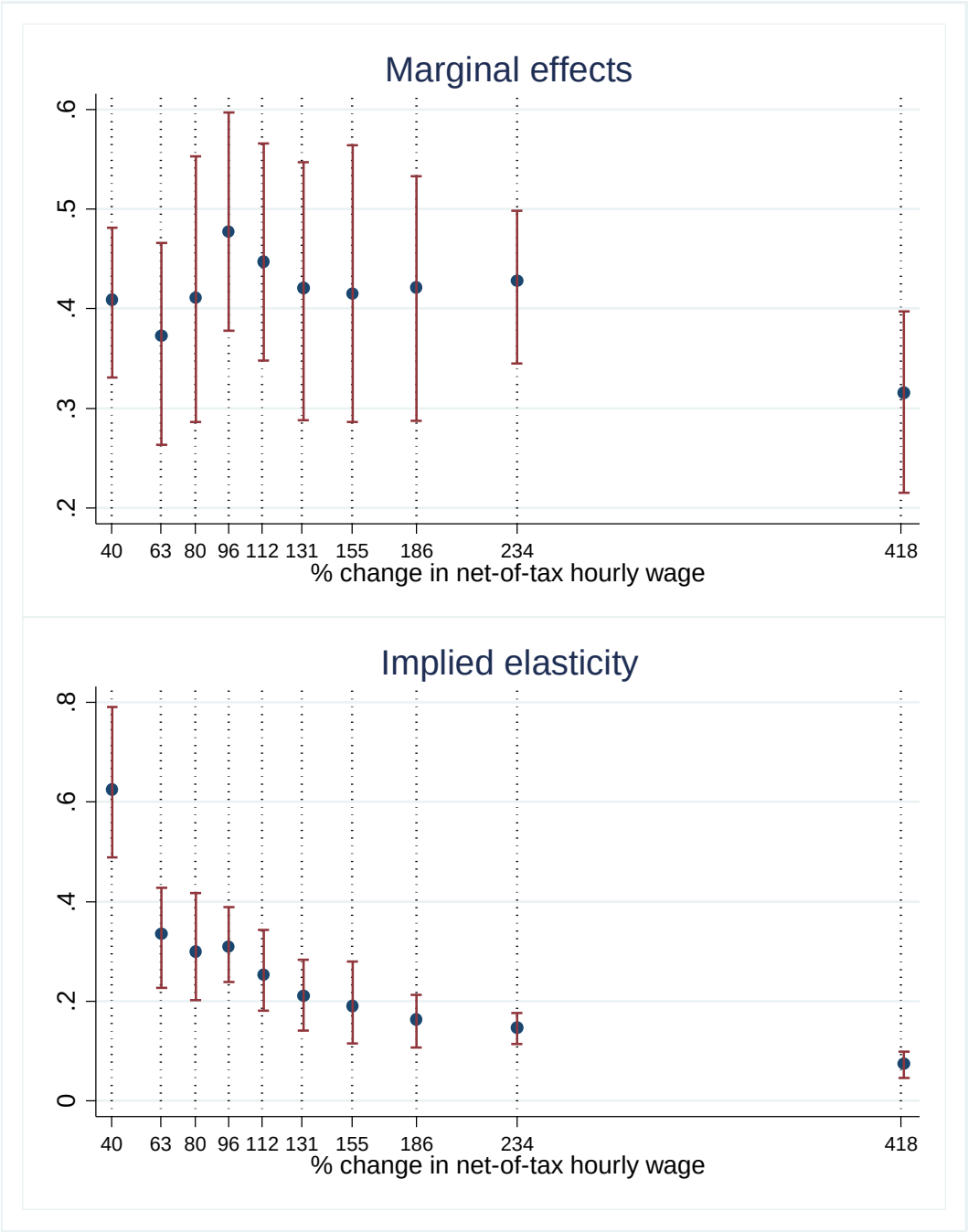

Figure 5. Estimated hours-of-work responses to a dollar increase in the net take-home wage rate, by decile in the distribution of percentage improvements in the net wage generated by the reform and with 95% confidence intervals.

Note: The numbers on the horizontal axis indicate the percentage improvement in average incentives within each of the 10 deciles, i.e. $100 \times (\overline{\Delta}^{n} - \overline{\Delta}^{o}) / \overline{\Delta}^{o}$. Confidence intervals are based on clustered (clustered by education and birth-year, in total 136 clusters) non-parametric bootstraps with 120 trials.

Since we do not follow workers over time, but compare separate cohorts with different sets of incentives, we cannot directly decompose the reform effects into extensive and intensive margin effects (Chetty et al., 2011b). We are simply not able to identify the number of hours worked among the workers who would have chosen to be employed with both pre-reform and post-reform work incentives. However, as the main reform effect appears to have been that a much larger group of workers tends to continue working with roughly the same hours as they had at age 60, whereas a much smaller group of workers tends to pull out of the labor market, it appears that the extensive margin is of major importance.

4.3. Liquidity (access age)

Members of Group 3 did not experience any change in work incentives $(\Delta_i^O = \Delta_i^N = \Delta_i)$. However, they experienced a considerable improvement in liquidity, as the reform allowed them to draw on their public pension entitlements from the age of 62 instead of 67. Our theoretical discussion above suggests that this change unequivocally reduced labor supply at ages 63 and 64, since those who were liquidity-constrained with pre-reform rules – and hence chose to work more than their optimum at age 63 and 64 to avoid a too low consumption in this period – would take the opportunity to work less with post-reform rules. We estimate linear models designed to capture the effects of the reform on the various labor market outcomes y_i; i.e.

$$y_i = \mathbf{x_i}'\lambda + \delta\Delta_i + \theta R_i + \varepsilon_i, \tag{2}$$

where θ is now the coefficient of interest. The estimation results are presented in Table 4. In contrast to what was indicated by the descriptive statistics in the previous section, but in line with economic theory, we now see a statistically significant negative labor supply impact of improved liquidity. On average, the reform yielded approximately a one hour reduction in weekly labor supply at age 63 and 0.6 hours at age 64. The employment rate was reduced by only around one percentage point, however: the liquidity effect appears to have operated more on the intensive than the extensive margin. Some workers took the opportunity to reduce work hours somewhat, rather than withdrawing from the labor force. We also see evidence indicating that improved liquidity triggered a small drop in disability program participation.

Although we cannot compute elasticities for the reduced access age, it is striking that the reduction in hours worked of around 3 percent at age 63 (refer Table 2) is only around one tenth of the 30 percent increase in work resulting from the improved work incentives in the reform. However, it is difficult to compare this to other results. The complete decoupling of labor supply and pension claim decisions implies that the reduced access age did not change either the returns to work or the size of pension wealth. The studies we have found on changes in the access age all include changes in at least one of these parameters as well. A study that comes close to our own is by Behaghel and Blau (2012), where impacts of a change in the full retirement age are investigated in a setting where there were no accompanying changes in work incentives. The authors find that this entailed considerable labor supply responses, which they interpret as a result of "reference dependence" with loss aversion. We would argue that such effects are not likely to be relevant in our case, since a majority of workers already had 62 years as the access age, and since the reference point of a full retirement age of 67 was not changed.

While the impact on hours worked from the increased liquidity was quite small, there was indeed a huge shift in pension claiming, toward the new and lower access age. Around half of those who were given access to the FTP five years earlier started claiming almost immediately (not shown in the table). This may seem at odds with the small reduction in labor supply and indicates that the pension only to a small degree was used to finance reduced labor earnings. Apart from financing work reduction, there could be a number of reasons for the early claiming, such as risk aversion and private mortality information. While this is not the topic of this paper, we note that the claiming spike is in line with other studies.

Table 4. Estimated effects of improved liquidity (early access to pension wealth)

	Overall effect of the reform measured on hours or percentage points (pp.) (standard error)
Weekly number of work-hours	
Age 63	-0.905 hours*** (0.288)
Age 64	-0.596* hours (0.360)
Probability of working "as before" (>80% of age 60 hours)%	
Age 63	-3.147 pp.*** (0.876)
Age 64	-2.633 pp.*** (0.941)
Probability of working with reduced hours%	
Age 63	2.370 pp.*** (0.617)

Age 64	2.444 pp.*** (0.761)
Probability of being retired without disability insurance	
Age 63	0.943 pp.*** (0.360)
Age 64	0.860 pp. (0.536)
Probability of being retired with disability insurance	
Age 63	-0.166 pp. (0.210)
Age 64	-0.670 pp. (0.409)

Note: The table reports the estimates of the coefficient θ in Equation (2). The control variable vector $\mathbf{x}_i$ includes gender, education (nine fields and eight levels), country of origin for immigrants (five regions), and labor earnings and weekly hours of labor supply at baseline (age 60). Number of observations: 30,653 for age 63outcomes and 23,032 for age 64 outcomes. Note: Standard errors are clustered on education (6 levels and up to 10 fields, in total 34 groups) interacted with birth year, in total 136 clusters. * (**) (***) Significant at the 10 (5) (1) % level.

5. Discussion and concluding remarks

Many countries around the world are trying to encourage greater mature labor force participation, as a response to an ageing society, and especially increasing mature age life expectancy. In this paper we analyze the labor supply response to a comprehensive reform of the Norwegian retirement system introduced in 2011. The reform followed policy debate in which two possible reform options were discussed: To increase the earliest access age (leaving annual pension benefits untouched) or to remove all disincentives to work, allowing workers to claim pension benefits while working, with actuarially fair deferral, rather than increasing the access age.

The Norwegian reforms embraced the second of these approaches, which we have termed the "flexibility approach". Essentially, it combines automatic longevity adjustment with extension of pension entitlements that were previously reserved for those who actually stopped working (or reduced hours considerably) to all covered workers, regardless of labor supply behavior. Among the obvious advantages are that it deals with heterogeneity in the circumstances – health, wealth, family – of older workers. Relatedly, spillovers into disability programs, often a consequence of increasing access age, might be expected to be quite muted, since continuation in the workforce has longer term payoffs, allowing significant wage income to coincide with benefit receipt.

This was the core thrust of a comprehensive reform which brought more into line groups of private sector employees who had previously faced very different retirement income support structures. In this paper, we have analyzed the incentive and liquidity impacts of these changes, with a particular focus on labor supply. Our results suggest a strong labor supply response to the change, especially at the extensive margin. Many workers who, before the reform, would have exited the workforce at access age, now continue to work more or less as before.

The drive to encourage mature labor force participation is importantly informed by the need for fiscal sustainability in the face of an ageing demographic. The flexibility approach looked to be expensive, since the removal of the earnings test meant that pensions would be payable regardless of labor force participation. To contain the overall costs of the system, the maximum (present) value of the early retirement pension was reduced considerably. However, in order to "grease" the reform process – i.e., to make it more acceptable for the workers that were closest to the early retirement age – the first affected cohorts (those born before 1963) were largely sheltered from these reductions thorough a separate "compensation benefit". Hence, to make the reform a fiscal success, particularly for these first cohorts, a substantial labor supply response is required.

Based on the data used in this paper, we estimate the overall direct (net present) value cost of the pre-reform early retirement program, for persons who left the labor force at the earliest occasion, to approximately $137,000 per retiree net of tax (with a 3% annual discount rate). But, since many workers did not stop working, the earnings test implied that the cost per eligible worker was "only" $73,000. In the new system, the direct pension outlays amounted to around $104,000 per worker for the first affected cohorts, regardless of work and retirement behavior (based on current life expectancy tables); hence the direct fiscal impact of the reform was to raise the pension costs by around $31,000 per worker, or 42%. This increase was primarily caused by the extra compensation benefit given to AFP-eligible workers born before 1963. For subsequent cohorts, we estimate that the overall cost of the new AFP is approximately $78,000; i.e., just $5,000 more than the old one. However, as we have seen in this paper, the reform also raised labor supply considerably, and through that it also raised tax payments. Extrapolating our estimated labor supply responses at age 63 and 64 to the ages of 62, 65, and 66 (assuming that the labor supply responses estimated at age 63 are the same at age 62 and that the responses estimated at age 64 are the same at age 65 and 66), we actually find that the reform raised the overall expected tax income by as much as $32,000 per work-

er[11] Hence, even though the reform entailed considerable additional pension expenditures for the first affected cohorts, it turned out to be more than self-financing and more strongly over time. This conclusion may be modified if it turns out that claiming behavior is sensitive with respect to individual longevity, such that persons with short life expectancy systematically start to draw on their pension entitlements earlier than persons with long life expectancy.

Our findings suggest that the "flexibility approach" is a viable alternative to a more strict prescription policy of higher access age. This has something of the flavor of the Feldstein (2005) proposal that that the government can make up for a reduced public pension by stimulating occupational pensions. The descriptive evidence we presented in Section 3 actually indicates that the added labor supply effect obtained by also raising the access age is small. Moreover, whereas higher access age appears to entail a spillover to disability pension programs, no such unintended side effects are identified for those who were exposed to improved work incentives only.

The results presented in this paper, as well as other recent studies on preceding pension rules adjustments in Norway (Hernæs and Jia, 2013; Brinch et al., 2015) point to a highly elastic labor supply behavior among elderly workers. Like all country specific studies, these findings probably partly reflect contextual features of the reform process, as well as the design of the reform itself. First, the new and improved work incentives were forcefully communicated and subject to a lot of media attention. In contrast to what has been claimed regarding the deferral scheme in the US, it is highly unlikely that many of the affected workers in Norway misinterpreted the pension deferral scheme as being a pure tax. Our results then fit well with the finding of Chan and Stevens (2008) that "... well-informed people respond much more than previously thought". Second, but relatedly, allowing the deferral of pension benefits to be decided individually – independently of the labor supply decision – has probably contributed to prevent such misunderstandings. Moreover, the opportunity to combine labor earnings and pension benefits at will appears to have been highly valued, as a majority of those who decided to continue working as before nevertheless chose to start drawing on the pension benefits immediately. And third, the Norwegian pension reform was implemented at a time of macroeconomic stability and relatively low unemployment. There was a demand for mature work-

[11] In this calculation we have assumed a 30% average income tax rate on labor earnings plus a 14.1% payroll tax.

ers, and probably also to some extent a willingness among employers to allow “attractive” elderly workers to continue with reduced hours.

Previous empirical evidence indicates that changing established employment patterns among elderly persons is at best a slow-moving process, and that stagnant social norms and traditions probably play a significant role in offsetting the labor supply effects of incentives-focused pension reforms. Nevertheless, based on more recent data, we have shown in this paper that transparent, substantial, and successfully communicated improvements in work incentives may constitute a highly effective strategy for increasing mature labor force participation. If these results are supported by studies elsewhere, they will have major implications for pension reform designed to increase mature labor force participation.

Appendix

In this section, we briefly present the main descriptive evidence and regressions results with age 58 used as baseline instead of age 60; i.e., we condition on employment at age 58 and measure the discrete employment outcomes relative to that age. We focus on the groups affected by the reform in this exercise; i.e., groups 2, 3, and 5.

Figure A1 first summarizes the descriptive evidence corresponding to Figures 1 and 2 in Section 3. As can be seen, the labor supply and retirement patterns for both the treated and non-treated cohorts are very similar those reported there.

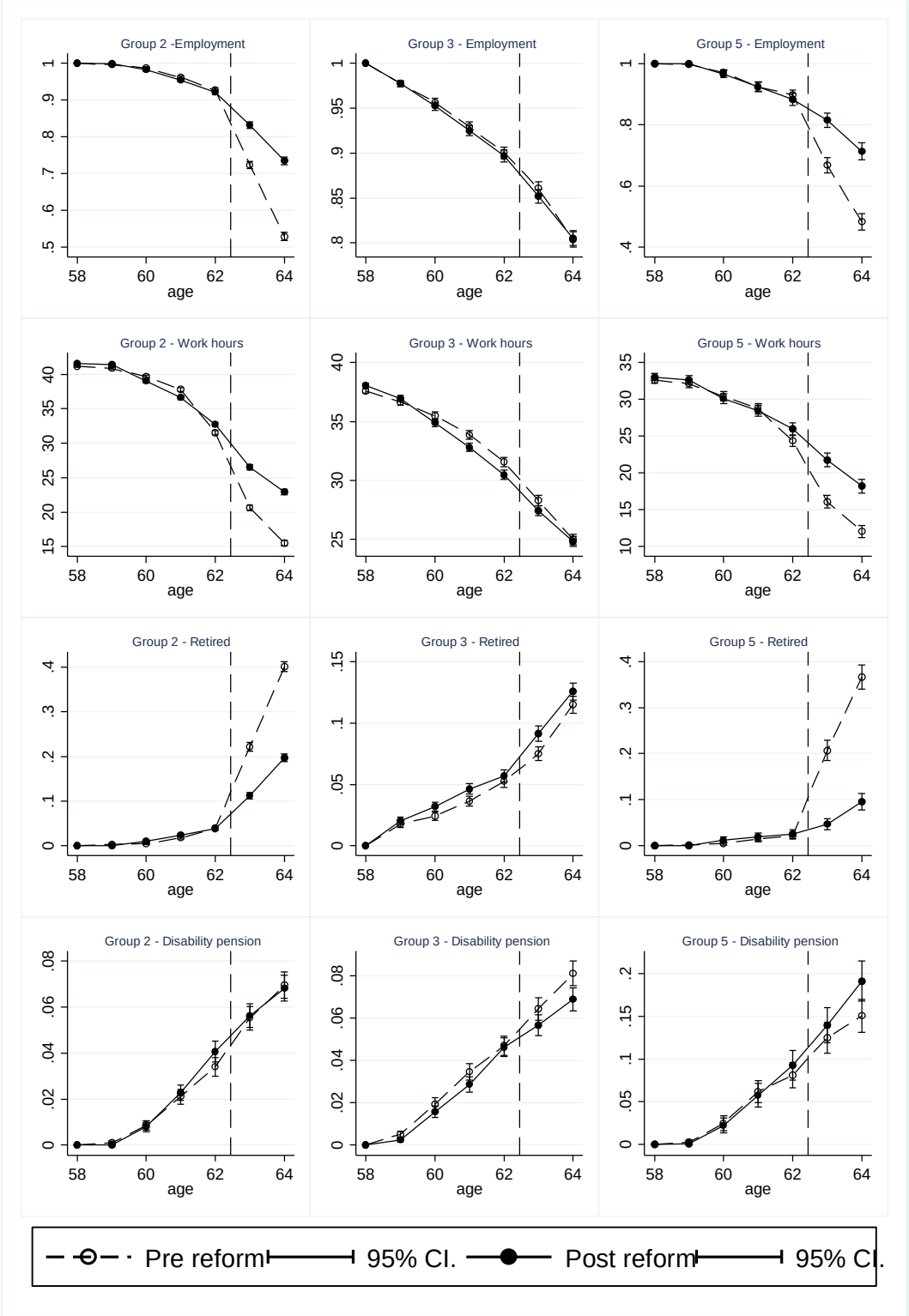

Figure A1. Labor market outcomes at age 58 to 64 for pre-reform cohorts (born in 1946-47) and post-reform cohorts (born in 1949-50), conditional on employment at age 58. With 95% confidence intervals.

Note: Due to data limitations, the post-reform outcomes at age 64 are based on the 1949 cohort only.

The regression results for Groups 2 and 3 are presented in Table A1 and A2. The estimated coefficients are again similar to those reported in Tables 3 and 4 above, but generally somewhat smaller. This probably reflects attenuation bias caused by less accurate allocation of workers into the six different groups (see Table 1) and by a larger fraction of the workers having changed their employment status (and/or hours) already before age 62.

Table A1. Estimated labor supply effects of the net hourly wage rate based on the population working at age 58.

	I Marginal effect measured in hours or percentage points (pp.) of a dollar increase in net wage (standard error)	II Implied aggregate elasticity [95% confidence interval]
Weekly number of work-hours		
Age 63	0.307 hours*** (0.018)	0.251*** [0.208, 0.288]
Age 64	0.412 hours*** (0.022)	0.442*** [0.370, 0.505]
Probability of working "as before" (>80% of age 60 hours)		
Age 63	0.639 pp.*** (0.046)	0.293*** [0.235, 0.346]
Age 64	0.804 pp.*** (0.056)	0.504*** [0.400, 0.600]
Probability of working with reduced hours		
Age 63	-0.064 pp.* (0.035)	-0.038 [-0.083, 0.007]
Age 64	0.364 pp.*** (0.050)	0.246*** [0.127, 0.325]
Probability of being retired without disability insurance		
Age 63	-0.571 pp.*** (0.036)	-0.537*** [-0.604, -0.470]
Age 64	-1.141 pp.*** (0.049)	-0.575*** [-0.648, -0.504]
Probability of being retired with disability insurance		
Age 63	-0.001 pp. (0.014)	-0.135 [-0.162, 0.119]
Age 64	-0.027 pp. (0.018)	-0.094 [-0.230, 0.030]

Note: Number of observations: 23,454 for age 63 outcomes and 17,432 for age 64 outcomes. Standard errors are clustered on educational groups (6 levels and up to 10 fields, in total 34 educational groups) and birth cohort, in total 136 clusters. The estimate and confidence intervals for the aggregate elasticities are bases on clustered non-parametric bootstrap with 500 trials. See also note to Table 3.
* (**) (***) Significant at the 10 (5) (1) % level.

Table A2. Estimated effects of improved liquidity (early access to pension wealth) based on population working at age 58

	Overall effect of the reform measured

	in hours or percentage points (pp.) (standard error)
Weekly number of work-hours	
Age 63	-1.623 hours*** (0.262)
Age 64	-0.950 hours** (0.316)
Probability of working "as before" (>80% of age 60 hours) %	
Age 63	-4.715 pp.*** (0.795)
Age 64	-4.000 pp.*** (0.907)
Probability of working with reduced hours %	
Age 63	3.400 pp.*** (0.577)
Age 64	3.773 pp.*** (0.754)
Probability of being retired without disability insurance	
Age 63	1.527 pp.*** (0.337)
Age 64	1.319 pp.*** (0.565)
Probability of being retired with disability insurance	
Age 63	-0.213 pp. (0.307)
Age 64	-1.092 pp.* (0.636)

Note: Number of observations: 23,454 for age 63 outcomes and 17,432 for age 64 outcomes. Standard errors are clustered on education (6 levels and up to 10 fields, in total 34 groups) interacted with birth year, in total 136 clusters. See also note to Table 4.
* (**) (***) Significant at the 10 (5) (1) % level.

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