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Abstract:
This paper examines the substitution between pension wealth and household saving by studying Norway's 2011 pension reform. The analysis identifies the effect of reductions in social security pension generosity on household saving using cohort, time and sector variation in pension wealth induced by the reform. Our study focuses on saving behavior between ages 57-61 for the 1954-1956 birth cohorts, who are the first three birth cohorts affected by a reduction in future pension wealth due to the reform. We find that they increased their saving rate around 1.2 percentage points (annually) after the reform, which corresponds to a five-year increase in household saving of about 27,000 NOK. When taking into account the remaining life-cycle changes to household saving, this corresponds to an offset effect of about 56 percent of the total loss in pension wealth.

Keywords: Pension reform, household saving, difference-in-difference, quasi-natural experiment

JEL classification: D14, E21, H55

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Sammendrag

Denne artikkelen undersøker i hvilken grad norske husholdninger oppveier tap av framtidig pensjon fra Folketrygden ved å spare mer på egen hånd. Analysen undersøker effekten av redusert pensjonsformue fra Folketrygden på husholdningenes sparing ved utnytte at reformen hadde ulik virking avhengig av hvilken sektor man jobbet i og hvilken fødselskohort man tilhører. Studien vår fokuserer på spareatferden til husholdninger født 1954–1956, som er de tre første fødselskohortene påvirket av en reduksjon i fremtidig pensjonsformue på grunn av reformen, og i alderen 57 til 61 år. Vi finner at husholdningene økte spareraten rundt 1,2 prosentpoeng (årlig) etter reformen, noe som tilsvarer en økning i husholdningenes sparing på om lag 27 000 kroner over en femårsperiode. Når man tar hensyn til gjenværende livsløp tilsvarer dette at husholdningene oppveier om lag 56 prosent av det totale tapet i pensjonsformuen.

1 Introduction

How do households' saving change if the social security pensions become less generous? The prospect of an aging population and increased fiscal burden has spurred pension reforms in many European countries that lower expected pension benefits, thus transferring more responsibility onto households to save privately for retirement. Although less generous pension systems may provide fiscal sustainability, it may also harm pension adequacy for the recipients. Substantial attention have been given to the topic on how individuals respond to reduced pension generosity by prolonging their working careers, but less attention have been given to whether individuals follow the consumption-smoothing behavior often assumed in standard life-cycle theory. We aim to provide causal evidence on the saving responses to reduced pension generosity, by using a quasi-natural experiment setting from a Norwegian pension reform.

Theoretically, the effect of a social security pension system on households' saving behavior is ambiguous. In a life-cycle framework, social security pension systems can be viewed as mandatory saving that diminish individuals' need to save for retirement themselves. We call this effect an "offset" mechanism, since an increase in one saving component offsets the other, i.e. more generous social security pensions decreases private saving and vice versa. However, changes to expected pension wealth/benefits can also an effect on labor supply both on the extensive and intensive margin. If the pension system causes people to retire earlier, thus extending the retirement period, this might induce saving instead. Furthermore, social security pension wealth is usually an illiquid asset, which may complicate a theoretical prediction about the substitution effect between mandatory social security pension accrual and private saving. The question of whether a social security pension system increases or decreases personal savings depends on the relative strength of these ambiguous forces, and thus remains an empirical question.

In this paper, we use a pension reform implemented in Norway in 2011 to study the private saving responses to changes in accumulated social security pension wealth. The 2011 Norwegian pension reform changed the accrual system in a way that caused a differential impact on individuals depending on their birth cohort and occupational sector. There are two key features of the reform that we use for identification. First, the pension reform introduced a new accrual system with a cutoff on birth cohort. Individuals born 1953 or earlier had their entire pension accrual from the National Insurance System (NIS) determined by the old system and were not affected by the new accrual system. Individuals born in 1954 and later were gradually transferred to the new system, which is overall less generous (see Section 2 for details). This constitutes a "pre" (1953 and earlier) and "post" (1954 and later) period. Second, public sector workers were (systematically) less exposed to the detrimental effects of the reform than their private sector counterparts. This is because public sector workers had access to a defined benefit scheme that ensured an annual pension benefit payout equal to 66 percent of their final year of earnings, which counteracted the less generous pension wealth through the reformed NIS. We therefore use the private sector as our "treatment group" and public sector as our “control group”, and we expect the treatment group to experience a larger negative effect on their expected pension wealth. An indication of an offset effect through private saving would therefore be an increase in the saving of the private sector workers relative to their public sector counterparts.[1] The structure of the reform described above calls for a difference-in-differences design. We extend the analysis to account for endogeneity of the offset effect by invoking the 2SLS method to retrieve the magnitude of the saving response, and correct for the re-optimization across the remaining life-cycle by using Gale’s Q (Gale, 1998).

We find that for those aged 57–61, the reform induced on average an increase in the saving rate of 1.2 percentage points, corresponding to a five-year increase in household saving of approximately NOK 27,000. This suggests that there is strong substitution between the social security pension wealth and private savings. Because we only observe five years of saving responses for each affected household, we correct for the fact that the household may re-optimize in both the period before and after our observations, i.e. since the announcement of the reform (the information year) and until their death. After correcting for the re-optimization across the remaining life-cycle, we obtain an offset effect of 56.5 percent, i.e. households save privately about 56.5 percent of the lost pension wealth. This is an estimate in the mid-range of what has been found in the literature.[2]

For the time being there seems to be no consensus on the magnitude of such offsetting effects between pension generosity and private saving. The contributions differ with respect to country, time period, identification strategy, endogeneity bias and sample selection. Moreover, the literature suggests that there is substantial heterogeneity across individuals. A key difficulty in estimating the relationship between pension wealth and household saving lies in how to account for unobserved characteristics that influence both variables; see Gale (1998) for a discussion of other biases in the estimation. Therefore, the literature has turned to exogenous shifts in pension wealth to estimate the offset effect. Attanasio and Brugiavini (2003), Attanasio and Rohwedder (2003), Bottazzi et al. (2006), Lachowska and Myck (2018), and Lindeboom and Montizaan (2020) use differential impacts across groups and time created by pension reforms as a source of variation and apply variants of the difference-in-difference or regression discontinuity approaches to estimate the effect of social security pension wealth on saving.[3]

[1]Although the public sector workers are systematically less exposed to the reform, the 66 percent gross replacement rate from the public occupational pension scheme did not relieve public sector workers *entirely* from any reform exposure. In the odd case that some public sector workers lost expected pension wealth, this will pull the responses in our control group in the same direction as our treatment group, and therefore our estimates should be interpreted as lower bound estimates.

[2] The seminal paper by Feldstein (1974) reports that a 1 dollar increase in social security wealth depresses private saving with about 40 cents, which is an offset effect of 0.40. Gale (1998) estimates the offset to be 0.50; Attanasio and Brugiavini (2003) report a range of effects between 0.30 and 0.70; Attanasio and Rohwedder (2003) report the offset effect to be between 0.65 and 0.75; Bottazzi, Jappelli, and Padula (2006) estimate it to be 0.65; Aguila (2011) reports it to be 0.50; Engelhardt and Kumar (2011) find it to be between 0.53 and 0.67; Feng et al. (2011) estimate it to be between 0.10 and 0.16; Lachowska and Myck (2018) report it to be 0.24; and Lindeboom and Montizaan (2020) estimate an overall effect of 0.38.

[3]In addition to studies about the effect of social security pensions on private saving, there is a closely related literature that studies the effect of private pension plans or tax incentives on private saving (Gale & Scholz, 1994; Engelhardt & Kumar, 2011; Chetty et al., 2014). This literature seems to find evidence of a large degree of substitution

The two papers closest related to our study are Lachowska and Myck (2018) and Lindeboom and Montizaan (2020). Similarly to our analysis, they also use actual pension reforms to identify the causal effects of pension wealth changes. The identification in our analysis hinges on variations of policy impact both across the birth cohorts and occupational sectors. In contrast, the above mentioned studies use mostly the variation across cohorts. Additionally, while they based their analysis on survey data, we have access to register data for the whole population that go back to the late 1960s. Furthermore, we use a detailed dynamic micro-simulation model to construct a comprehensive and precise measure of future expected pension wealth for everyone in the economy. Expected pension wealth in our analysis is defined as the present value of the sum of future pension benefits based on the individual's full income history and expected longevity (by gender, education, marital status and health), while previous papers rely on calibrated pension benefits based on observed individual demographics and the mean earnings histories. Furthermore, we have information about all pension plans, both national insurance, public and private pension plans. Finally, we have a separate measure of active saving in the household, not simply changes in net private wealth.

The remainder of this paper is organized as follows. Section 2 describes the Norwegian pension system and the 2011 pension reform. Section 3 describes the data and variables, while Section 4 describes our empirical strategy. Section 5 presents the results of our empirical analysis. Finally, Section 6 concludes.

2 Institutional background

2.1 The Norwegian pension system

The Norwegian pension system consists of three pillars. The first pillar is a social security pension, called the National Insurance System (NIS), which consists of a residence-based guarantee pension and an earnings-related pension. NIS is an integrated part of the central government budget and financed as pay-as-you-go. The social security pension is relatively generous, and more than 80 percent of all pension wealth in Norway is accrued through the NIS.

The second pillar consists of occupational pensions. Until 2018, occupational pensions in public sector were of the defined benefit type and coordinated with the corresponding benefits from NIS, in order to give a guaranteed replacement rate of 66 percent of one's final salary.[4] Occupational pensions in the private sector vary with respect to benefit levels, duration of benefits, indexation,

from non-tax-favored into tax-favored pension savings accounts, but little evidence of tax incentives resulting in overall increases in saving.

[4] A replacement rate of 66 percent in the public sector was only guaranteed under certain conditions. Benefits were reduced proportionally if the number of accrual years were less than 30 or if average working time was less than full time. After 2018, occupational pensions in public sector are still of the defined benefit type, but accrual is proportional to earnings.

and whether the schemes are defined benefit or defined contribution.

Until 2011 the retirement age for pension benefits in NIS was 67 years. To allow workers to retire before the age of 67 without using the disability pension scheme, an occupational early retirement scheme was introduced in 1989. By this agreement it became possible for private sector workers to retire at the age of 66. The scheme was gradually spread to other collective agreements including the public sector, and the earliest possible retirement age was gradually reduced to the age of 62. All employees in the public sector and about 60 percent of those employed in the private sector are covered by an early retirement agreement.

The third pillar consists of voluntary pension savings supplementing social security pensions and occupational pensions. These are offered by private insurance companies and typically yield annuity payments at retirement. Due to a comprehensive social security pension and well-established occupational pension schemes, the third pillar is less developed in Norway compared to other countries, and only accounts for 0.3 percent of all pension wealth.

2.2 The 2011 reform of the pension system

The identification strategy in this paper, which is described in further detail in Section 4.1, relies on changes due to the 2011 reform of the pension system. The three main changes in the NIS introduced by the reform in 2011 were: (i) a new accrual system, (ii) flexible retirement and old-age pension claiming age, and (iii) actuarial adjustments and lower indexation of pension payments. Several papers have studied the effects of the reform, see e.g. Brinch et al. (2021); Kudrna (2017); Hernæs et al. (2021) and Vestad and Wentzel (2022).

Prior to the reform, the accrual system was linked to the 20 best years of earnings and based on the accrual of pension points towards pension wealth. Between ages 17 and the age of retirement, individuals earned one point for annual earnings between 1 and 6 base amounts, 1/3 point for earnings between 6 base amounts and 12 base amounts, and no additional points for earnings above 12 base amounts.[5] Therefore, an individual with annual earnings of 12 base amounts could earn a maximum of 7 pension points each year. Upon retirement and claiming of pension benefits, the stock of pension wealth based on these accrued pension points (called "final pension points") is converted into an annual benefit payout, with final pension points being equal to the average number of pension points over the 20 years of highest annual earnings.

After the reform in 2011, the accrual of pension rights in the new accrual system takes place con-

[5]The unit "base amount", commonly denoted G, is set as the threshold for earnings to be pension awarding. The unit is set by the government and indexed annually according to wage inflation. In 2021, one base amount is equivalent to NOK 106,399, or approximately USD 12,500/EUR 11,700. Earnings must exceed this threshold to be pension awarding.

tinuously over the life-cycle with a fixed rate of 18.1 percent of annual earnings up to a ceiling at approximately 1.3 times the average full-time wage. The accumulated stock of pension wealth on individual notional accounts are converted into an annuity upon retirement, with annual pension benefits being adjusted by divisors reflecting remaining life expectancy for a cohort retiring at that age. Calculation of divisors for a cohort is based on common mortality tables for men and women. Early retirement leads to lower annual benefits because accumulated pension wealth must be divided by more years. This is also the case when life expectancy increases for a given retirement age. Lower benefits when life expectancy increases may be counteracted by postponing retirement. Before 2011 the standard claiming age was fixed at 67. Under the new system, pensions may be drawn partly or completely between the age of 62 and 75 (without any earnings test), allowing for flexible retirement. In the new system, pension wealth during accumulation are indexed according to the average wage rate. After retirement, the annual pension payout is indexed to the average wage rate but a fixed component of 0.75 percent per year is subtracted.

While the actuarial part of the new pension system was effective for all new retirements from 2011, a transitional arrangement was introduced for the reform of the accrual system. Individuals born in 1953 or earlier would accumulate their pension wealth according to the old system. In the group born from 1954–1962, pension wealth will be partly calculated from the old system and partly from the new, with an increasing share; for example, pension wealth for individuals born in 1954 would be 90 percent based on the old rules and 10 percent on the new. Individuals born in 1963 and later will earn their pension wealth completely according to the new system.

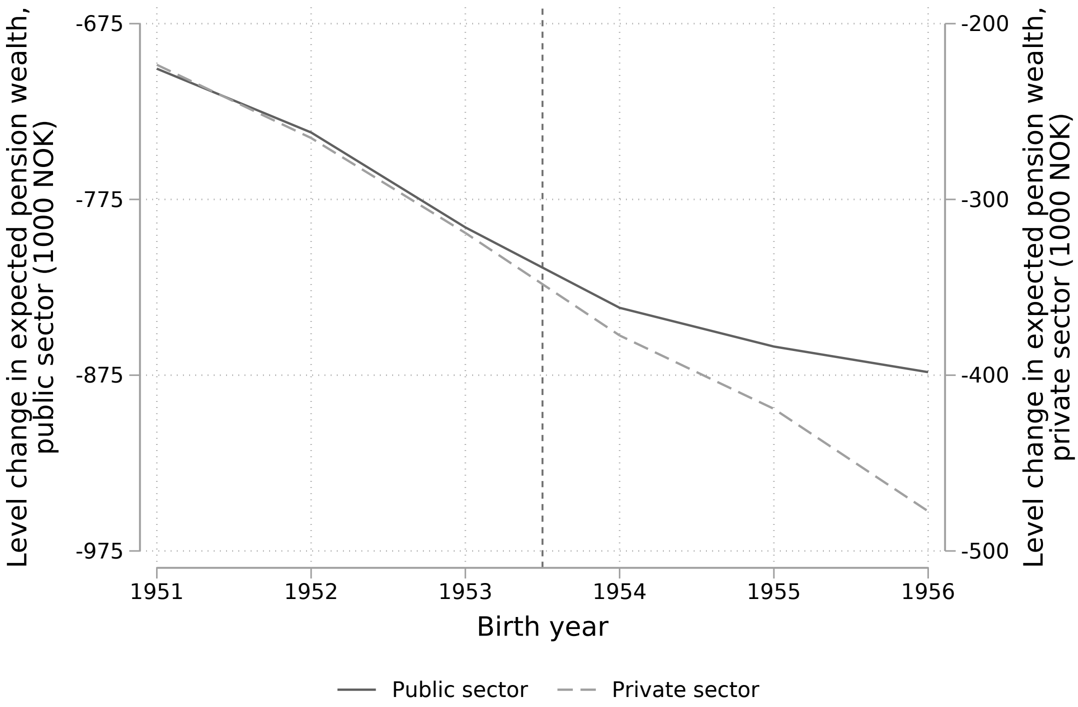
Due to the reform of NIS, changes were also introduced to the early retirement scheme in the private sector, aiming at a better integration with NIS. An earnings test was removed and actuarial adjustments were introduced. Moreover, receiving pension from NIS and the early retirement scheme at the same time became possible. However, occupational pensions in the public sector remained largely unchanged.[6]

2.3 The impact across cohorts and sectors

The implementation of the reform created treatment variation depending on individuals' birth cohort and sector. First, all those born before 1949 remained in the old system. Regardless of sector, those born in the years 1950–1953 were affected by flexible retirement, actuarial adjustments and lower indexation of annual pension income (pension in payment), but not the new accrual system. Those born between 1954 and 1962 are subject to a mix of the old and the new accrual system, with a gradually increasing share of pension wealth being calculated according to the new system. Finally, because occupational pensions schemes in the public sector were so tightly integrated with the NIS pension system as to almost completely neutralize changes in accrual system in NIS (at least until

[6] For further details on this, see Kudrna (2017).

Figure 1: Expected pension wealth loss by sector and cohort



Notes: Estimated loss of expected pension for a given cohort and sector affiliation. Workers are pre-determined to work in the given sector at age 55. Annual data from 2008–2017 using cohorts 1951–1956.

the 1958 cohort), workers in the public sector were largely unaffected by the reform, apart from actuarial adjustments and lower indexation of the annual pension payout. Therefore, their effective marginal contribution rates to the publicly provided old-age pension wealth did not change in the same way as for the private sector workers, whose occupational pension were not integrated with NIS and were not granted equivalent compensation in response to the new accrual rules.

To illustrate the effect of the reform components on pension wealth, we compute the change in expected pension wealth for workers under the pre- and post-reform rules by sector. All calculations are done under the assumption of a fixed retirement age, even though a flexible retirement age was part of the reform, in order to highlight the structural changes due to accrual system and indexation. The results from this exercise are shown in Figure 1.

In Section 4 we will use this differential impact across cohorts and sectors to investigate the private savings responses to changes in expected social security pension wealth, by means of comparing the responses in saving rate before and after the reform for public and private sector workers using a difference-in-differences approach.

3 Data sources and important variables

The data set is derived from a combination of administrative registers covering the whole Norwegian population. Data are assembled from annual tax records, the central population register, the social security register administered by the Norwegian Labor and Welfare Administration, and a pension wealth database administered by Statistics Norway. These data are of high quality because most information is third-party reported and very little is self-reported. Employers, banks, brokers, insurance companies and any other financial intermediaries are obligated to send information on earnings, the value of the asset owned by the individual and administered by the employer or the intermediary, as well as information on the income earned on these assets both to the individual and to the authorities.

We are interested in two main outcomes; the saving rate and saving, both measured at the household level. Household saving is defined as the change in the household's net financial assets, minus revaluations. The financial assets we observe and define in the tax data are broad asset classes of bank deposits, bonds, mutual funds, stocks and debt. In our data, the change in nominal financial assets from one year to the next consists of two parts; changes in the stock of an asset and changes in the valuation of an asset. We do not want unrealized changes in the asset's price, i.e. unrealized capital gains and losses, to be part of our saving measure as they do not reflect the household's active consumption and saving behavior. Thus, what we call "active saving" is the nominal change in financial assets minus capital gains and losses; see Fagereng and Halvorsen (2017) for more details. The saving rate is defined as household saving divided by household disposable income.[7]

In addition to financial assets, our data also contain information about educational level, marital status, number of children, housing values, inheritances received, and details about various pension plans that the household participates in.

We calculate pension wealth by using a dynamic micro-simulation model called MOSART, see Andreassen et al. (2020). MOSART utilizes the same administrative data registers available in Statistics Norway as we use in the empirical part of this paper. The data covers the entire population starting in 1967, which is also the year when the NIS was introduced. Its main use is for calculating pension accrual rights and future pension benefits, based on earnings history and other relevant characteristics according to an accurate description of the pension system. Thus, as opposed to many other studies that have to rely on estimated income profiles or cohort averages, we are able to calculate future pension benefits accurately on the individual level, given the current set of rules or a counterfactual set of rules, i.e. the old pension system. The model also takes into account the inter-

[7]Measures of saving as the first difference in wealth tend to have large variance and extreme outliers. Several strategies can be chosen to avoid problems stemming from highly influential extreme values, such as deleting or manipulating the observations identified as problematic, or, by transforming variables so that the distribution of all variables has a lesser spread than the non-transformed. One such possible transformation is the inverse hyperbolic sine transformation: $s = \ln(S + \sqrt{S^2 + 1})$ that behaves as $\log(S)$ everywhere with the exception of in the neighborhood of zero (Burbidge et al., 1988). We present results using both saving in levels and saving rates transformed by inverse hyperbolic sine.

dependencies between the rules of different pensions schemes, i.e. national insurance, occupational pensions and early retirement schemes.

We use the model to calculate the discounted present value of future pension benefits using the rules of the pre-reform and the post-reform system respectively, given each individual's accrual history. The discount rate is set equal to the long-run productivity growth. Given a long-run inflation rate at the target of 2 percent, this gives us a discount rate of about 3 percent. In calculations of both the pre-reform and the post-reform system we use the same assumptions about longevity and future retirement age, fixed at the standard retirement age of 67. In other words, future expected pension wealth is calculated independently of actual or subjectively expected retirement age.[8]

4 Empirical strategy

4.1 The identification strategy

As discussed in Section 2, the 2011 pension reform had a different impact on the accumulated pension wealth for individuals of different birth cohorts in the private and public sector. This is due to the gross replacement rate of 66 percent that the public sector workers maintained throughout our estimation period, which greatly dampens the loss of expected pension wealth among these workers. This variation allows us to identify exogenous loss of expected pension wealth across otherwise comparable groups.

Thus, we can apply a difference-in-differences (DID) empirical strategy to identify the effect of changed pension generosity on savings behavior. In a nutshell, we compare the changes in outcomes between the post-treatment cohorts who were affected by the new accrual system (1954–1956 cohort) and the pre-treatment cohorts who were not (1951–1953 cohort) in the private sector with the corresponding change in the public sector. The key identifying assumption is then that the mean outcomes for the treatment group over different cohorts would, in the absence of treatment, have followed the same trend as that for the control group.

[8]As a way to get unbiased estimates of the pension wealth given the uncertainty of longevity, we use variance-reducing methods of stratified repeated simulations, and we repeat 100 times. Technically, the model assigns a random seed common to all 100 runs of the model, and assigns a "z-value" to all individuals in all runs in such a way that each run is randomly distributed over the entire spectrum of z. We assume that the z's are uniformly distributed $(0,1)$. This ensures that two individuals are uncorrelated, and each individual will be assigned a mortality probability on each percentile of z. The method drastically reduces the variance of longevity on the individual level, which ensures unbiased estimates of the expected pension wealth.

4.2 The causal effects of the reform on saving behavior

Formally, we estimate the following equation using OLS:

$$Y_{i,t} = \delta T_i + \beta S_i + \lambda_a + \theta_c + \gamma X_{i,t} + \varepsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ is the outcome variable for individual i at year t, either household saving rate or levels of household saving. S_i is the sector dummy which takes the value 1 if individual i is a private sector worker. $T_i = S_i \times \mathbb{I}_{c\geq 1954}$ is an indicator for individual i being treated (i.e. a private sector worker born in 1954 or later). Thus δ captures the average treatment effect of the reform for the treated. λ_a and θ_c measures the age- and cohort-fixed effects. Due to the age-year-cohort collinearity problem, we cannot include year dummies. The vector $X_{i,t}$ includes a number of controls such as the number of children, marital status, educational level, and the level of pre-reform earnings (average earnings up to age 55).

As mentioned in Section 2, the reform reduced the generosity of future pensions for the affected cohorts in private sector differently. We therefore expect that the treatment effects will differ across these three cohorts (1954–1956) as well. However, we are only able to estimate an "overall" effect, δ, using Eq. (1), which masks the potential heterogeneous treatment effects. More importantly, the estimate of δ recovers a weighted average of treatment effects for those who are treated over the post-reform period. Recent research has shown that weights implied by OLS may lack economic interpretation, and in some cases can even be negative (Borusyak et al., 2022; Goodman-Bacon, 2021; Sun & Abraham, 2021). In these cases, an estimate of the average treatment effects from Eq. (1) does not provide a valid estimate of the causal effect of interest. One way to address this problem is to follow the suggestion of Sun and Abraham (2021) and estimate the following equation:

$$Y_{i,t} = \sum_{j=1951}^{1956} \delta_j \cdot S_i \cdot \mathbb{I}_{\mathbb{C}_i=j} + \lambda_a + \theta_c + \gamma X_{i,t} + \varepsilon_{i,t}, \tag{2}$$

where $\mathbb{I}_{\mathbb{C}_i=j}$ is an indicator for individual i being in cohort j, thus δ_j measures the cohort-specific reform effects. Note that we need to exclude one δ_j, and we follow Borusyak et al. (2022) and drop the last cohort which is not affected by the reform. This implies that we use 1953 cohort as the baseline, i.e. drop δ_{1953} in Eq. (2). With this specification, we can then recover the reform effect from cohort-specific effects as $\sum_{j=1954}^{1956} \omega_j \delta_j$, where ω_j is the share of private workers in cohort j among all treated private workers. One additional benefit of the specification in Eq. (2) is that the pre-reform (placebo) treatment effects, δ_{1951} and δ_{1952}, are included. Therefore, we can directly test the hypothesis that there is a common trend in the pre-treatment cohorts for the private and public sector workers. Although the insignificance of the placebo treatment effects cannot guarantee that the common trend assumption holds for the post-treatment cohorts, it is nevertheless reassuring.

4.3 The substitution between social security pension and private saving

The difference-in-differences regressions in Eq. (1) and Eq. (2) have the advantage of being easy to interpret, i.e. they recover either the average or cohort-specific effect of reform on the saving rate. However, it is not informative about the degree of substitution between public pension wealth and private saving. There is a large empirical literature to address this question. Unfortunately, the results differ widely, from almost complete offset of pension wealth against non-pension wealth (Attanasio & Rohwedder, 2003) to almost no effect (Feng et al., 2011). To shed more light on this problem, we supplement our study with an analysis that aims to quantify the offset effect with the help of the exogenous variation generated by the reform. As accumulated pension wealth is a function of past labor market attachment, individuals experience different level of changes in their pension wealth as the result of the pension reform. So the reform (treatment) is not simply binary ("on" or "off"), but also has different treatment intensities (doses), which enables us to evaluate the offset effect within a continuous-treatment DID framework.

In particular, we replace the binary treatment variable T_i in Eq. (1) by a continuous variable $Z_{i,t}$ which measures the change in pension wealth due to the pension reform:

$$Y_{i,t} = \alpha Z_{i,t} + \beta S_i + \lambda_a + \theta_c + \gamma X_{i,t} + \varepsilon_{i,t}. \tag{3}$$

As we are interested mainly in the offset effect, the outcome variable $Y_{i,t}$ now is the level of household saving. The variable $Z_{i,t}$ is derived from a micro-simulation of expected pension wealth in the reformed system minus simulated expected pension wealth in the pre-reform, or old, system:

$$Z_{i,t} = \mathbb{E}\sum(\text{All pensions in reformed system})_i - \mathbb{E}\sum(\text{All pensions in old system})_i \tag{4}$$

If this is positive (negative) then individual i gained (lost) pension wealth as a result of the reform. We can use this to pin down the incentive to adjust private saving. In case of substitution between pension wealth and private saving, we expect $Z_{i,t}$ to have a negative effect on household saving, as a gained (lost) pension would reduce (increase) private household saving.

Under the "Strong Parallel Trends" assumption proposed by Callaway et al. (2021), α in Eq. (3) recovers the average causal response, which measures the average change in household saving in response to a marginal change in the changes in pension wealth induced by the reform.

There is potentially an endogeneity problem as the variable $Z_{i,t}$, which is a function of previous labor market attachment, may be potentially correlated with the unobserved "tastes of saving" ($\varepsilon_{i,t}$). To deal with this problem, we apply the method of two-step least squares (2SLS), where we use the binary variable of being a private sector worker in an affected cohort (T_i) as the instrument for $Z_{i,t}$.

We can formally test the instrument relevance based on the following first-stage regression:

$$Z_{i,t} = \sigma T_i + \beta_1 S_i + \gamma_1 X_{it} + \lambda_a + \theta_c + \varepsilon_{i,t}. \tag{5}$$

The exclusion restriction hinges on sector affiliation in the treated cohorts being excludable from the 2SLS in Eq. (3). In other words, being a private sector worker born in post-reform cohorts does not induce workers to adjust their saving (in comparison to their public sector counterparts). The saving response runs *through* the loss of expected pension wealth only. We find this highly plausible, although as always, the exclusion restriction cannot be empirically verified.

In fact, if the common trend assumption that the outcome variable $Y_{i,t}$ in the public and private sector follows parallel trends in absence of reform, then the 2SLS estimate is a consistent estimate of Eq. (3). This is so because if the common trend assumption holds, we are able to be estimate the the causal effect of the instrument variable on the outcome variable consistently. Note that the 2SLS estimates can be written as the ratio between the corresponding estimates from the reduced form in Eq. (1) and the first stage in Eq. (5), which are both consistent, so by the Slutsky's Theorem the consistency of the 2SLS estimate is guaranteed.[9]

Another problem with Eq. (3) is that the estimate α only reflect the change in private saving for a given year rather than the change over the remaining planning horizon. As Gale (1998) pointed out this implies that the estimate from Eq. (3) is a biased estimator of the offset effect between public pension wealth and private savings. To deal with this problem, Gale (1998) proposed to multiply the accumulated public pension wealth with a factor, known as the Gale's Q. This adjustment factor depends on subjective discount factor, time preference, interest rates and the age when the change of pension wealth is publicized so that the behavior adjustment is possible. In our study, we use the following adjusted factor for the relationship between savings at age t and life time pension wealth:

$$Q(i,t) = \frac{\frac{1}{R^t}\left[\beta R\right]^{\frac{t}{\gamma}}}{\sum\limits_{j=t_0^i}^{T} \frac{1}{R^j}\left[\beta R\right]^{\frac{j}{\gamma}}}. \tag{6}$$

where t_0^i is the individual specific age when she was informed about the pension reform. β is the subjective discount factor, T is the life expectancy, R equals to 1 plus the interest rate. $1/\gamma$ is the intertemporal elasticity of substitution between consumption in any two periods, i.e., it measures the willingness to substitute consumption between different periods. The detailed derivation can be found in Appendix C.

[9]Note that consistency in both the reduced form and first stage regressions is a sufficient but not a necessary condition for the 2SLS estimate to be consistent.

5 Results

5.1 Sample and descriptive statistics

We use data for the years 2008–2017 and birth cohorts 1951–1956. This allows us to follow three treatment cohorts (1954–1956), and three comparison cohorts (1951–1953), over a five-year period after the reform was implemented. We also use three years before the reform to test for common trends. As described in Section 2, the reform affected workers in the public and private sector differently. We study the ages 57–61 for all cohorts, as these are the five years leading up to the legal early retirement age (62 years) and are observed for all cohorts in our data period. By using age 57 as the first observation, the 1954-cohort is covered in 2011. In 2017, the 1956-cohort is 61 years of age, i.e. this is the oldest age for which we have data for the last cohort. This ensures that we can follow all treatment cohorts for the same ages.

In our analysis, we pre-determine sector affiliation at age 55, which is two years before our outcome period. Sector mobility is generally low after age 55, see Appendix A.[10] Furthermore, we use the sector affiliation of males as a proxy for household head in our analysis.[11]

We restrict our attention to persons without a disability insurance history, as the rules governing the pension wealth accumulation for disability insurance recipients differs somewhat from non-disabled workers. This restriction reduces the sample size by about one third, but is essential to the identification of the incentive change as a result of the loss of expected pension wealth. We also exclude non-residents and individuals with less than 20 years of membership in the National Insurance System, and the reasoning is similar to excluding disability insurance recipients; this group face significantly less incentive changes due to the reform, despite being classified within the same sector.

Table 1 presents descriptive statistics as well as the main outcome variables and their components at age 55 for the estimation sample. We have split the statistics into the four groups that we use in our empirical design. The first group is the pre-reform cohorts in the control group (public sector), while the second is the post-reform cohorts for the control group. The third group is the pre-reform cohorts in the treatment group (private sector), while the fourth group is the post-reform cohorts for the treatment group. For a perfectly comparable control group, we would ideally have all pre-reform statistics be as equal as possible between the two groups. However, it is a known caveat to this particular grouping that the private sector educational attainment level is far lower than in the public sector. We do, however, observe that there is far more equality in terms of after-tax income,

[10]This means that we include the years 2006 and 2007 when we pre-determine variables such as sector affiliation, since these are the years when the 1951 and 1952 cohorts are 55 years of age, respectively.

[11]In a robustness check in Section 5.5, we do the exercise of defining household head as the main earner. We did not use this as our main specification for two reasons: First, in these birth cohorts the majority of main earners are male anyway. Second, household head defined as the main earner may change over time.

in fact so that the lower educated private sector workers on average earn more than their public sector counterparts. In terms of pension accrual, it is the income level and not the education that matters, which speaks in favor of comparing the two groups.[12] It is not crucial for our design that the pre- and post-reform cohorts are similar to each other in terms of observables. In fact, it is likely a reflection of macro-trends that shows an increase in particularly after-tax income and improved longevity for the younger cohorts.

Because the reform was *announced* earlier than 2011, we expect that also the 55-year olds may have started to adapt to the reform. This is why there is significant changes between the pre- and post-reform cohorts in the bottom half of Table 1. We do take this fact into account in our estimations later, when we allow for re-optimization of the saving behavior from the announcement of the reform until death. In fact, the 2011 timing is *not at all* essential to our design, as birth cohort define who's treated or not. What is important in the bottom half of Table 1 is, again, that the pre-reform cohorts are similar between the treatment and control group. As we observe, the outcome variables are remarkably similar between the two groups in the pre-reform cohorts.

[12] Several papers have used a similar or exactly the same grouping in empirical designs similar to ours, e.g. Hernæs et al. (2016) and Kruse (2021) among others.

Table 1: Descriptive statistics of household characteristics (upper), income and assets (middle) and outcome variables (lower) at age 55, only men.

	Public sector		Private sector	
	1951–1953	1954–1956	1951–1953	1954–1956
Number of children	2.1	2.1	2.0	2.0
Higher education	0.46	0.45	0.12	0.13
Final pension point at retirement	5.37	5.33	5.36	5.38
MOSART computed expected longevity	82.62	84.99	80.92	83.75
After-tax income	491,070	556,274	509,997	572,992
Total household after-tax income	889,269	988,819	855,662	961,091
Housing value	344,939	562,351	326,115	522,207
Household debt	827,191	1,065,000	786,485	1,006,000
Household bank deposits	223,136	261,421	218,705	250,576
Saving rate	-0.007	0.009	-0.007	0.020
Financial saving	-4,410	2,604	-4,909	8,439
Household assets	283,561	314,880	284,478	315,440
Expected pension wealth	11,120,000	11,380,000	7,049,000	6,989,000
No. of observations	8,079	8,212	15,276	16,414

Notes: Annual data from 2006–2011 using cohorts 1951–1956 at age 55. Only men. Higher education is the share within the group with any education on university level. Final pension point at retirement is a mapping from lifetime earnings to the pension payout level in the old NIS, see Section 2. Expected pension wealth is the sum of all pension components in the new NIS (public old-age pension, AFP and occupational pension). Income, debt and asset variables are measured in 2021-NOK. In 2021, the NOK-to-USD exchange rate was ≈ 8.5. MOSART computed longevity is the average expected time of death within the group, see footnote 8.

5.2 The effects of the reform on saving behavior

Table 2 provides the estimated reform effects on household saving rates. We find that the reform induced a statistically significant increase in saving rates between ages 57–61. The results indicate a treatment effect of 1.2 percentage points in our preferred specification in column (1), which includes all controls. As a way of testing the robustness of our results, we switch off our fixed effects and control variables in turn. In column (2), we include age fixed effects, but drop all control variables. Our point estimate for the treatment effect does not change substantially. In column (3) we include age fixed effects, but take out control variables. In column (4), we include neither fixed effects, nor control variables, essentially regressing the saving rate on the treatment indicator and the group variable (being in the private sector). Reassuringly, results are similar across specifications.

Coefficients on other controls are reported in Appendix B. These other variables include controls for the number of children, marital status, educational level, and level of pre-reform earnings.

Table 2: Main results using financial saving rate. Average treatment effect.

	(1)	(2)	(3)	(4)
Treatment effect	0.012***	0.009***	0.012***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)
Private sector worker	-0.001	-0.005***	-0.001	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	231,536	231,536	231,536	231,536
Age FE	YES	YES	NO	NO
Controls	YES	NO	YES	NO
F-stat	18.40	13.77	18.42	13.78
DF numerator	30	2	30	2
DF denominator	46,350	46,350	46,350	46,350

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Notes: Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors in parentheses, clustered on the individual level. Control variables reported in Appendix Table B.1.

As we discussed in Section 4, the results in Table 2 may not provide a valid estimate of the average treatment effect across all three outcome cohorts 1954, 1955 and 1956 if the OLS-implied weights are incorrect. To address this problem, we have also estimated the model in Eq. (2) with a full set of controls to recover the cohort specific effects, following the suggestion in Sun and Abraham (2021). The results are presented in Figure 2. We observe that there is variation in the responses to the pension reform across the three different cohorts. The effect reported in Table 2 is mainly driven by responses from the 1955 and 1956 birth cohorts. This is not surprising as the 1954 cohort was only exposed to 1/10 of the full reform, while the subsequent two cohorts were exposed to 2/10 and 3/10, respectively.[13] We have also calculated the average effect based on these cohort specific reform effects. For our preferred specification, it is 0.011 with a standard error of 0.0025.[14] This is quite similar to what we get from the basic specification in column 1 in Table 2 (0.012 with a standard error of 0.001), and confirms that the Goodman-Bacon critique does not apply to our analysis.

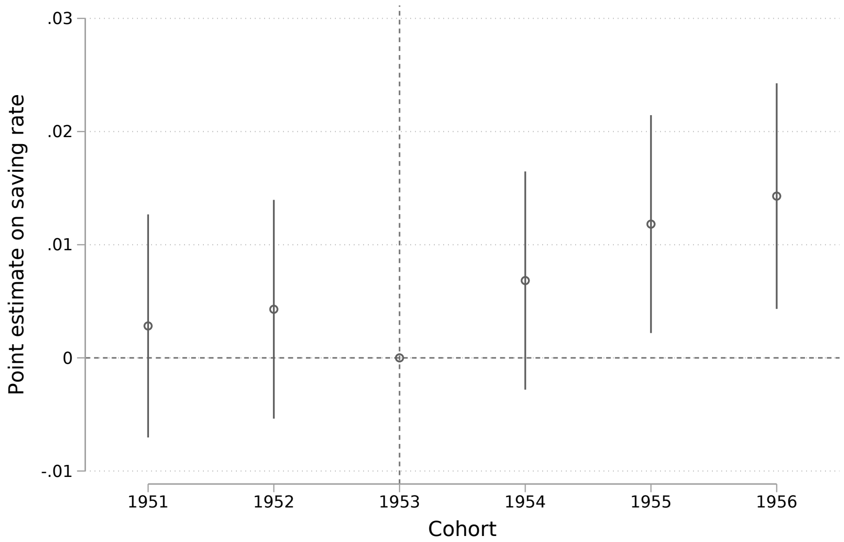
Another important observation is that the (placebo) treatment effects for pre-reform cohorts are not significantly different from zero, which suggests that there is a common trend for the treatment and control group in the pre-reform cohorts. As discussed earlier, although this is not a guarantee for the common trend assumption to hold for the post-reform cohorts, this is quite reassuring.

We now proceed by estimating an efficient DID setup, where we exclude the placebo treatment

[13]The reform will gradually take full force, and the 1963 cohort is the first to be fully exposed to the reform.

[14]This is the (weighted) average effect of the point estimates in Figure 2, and the corresponding standard errors.

Figure 2: Cohort specific effects on saving rates



Notes: Estimated coefficients and corresponding 95%-confidence intervals on saving rate for each cohort. Annual data from 2008–2017 using cohorts 1951–1956. The 1953-cohort serves as baseline.

dummies for pre-reform cohorts. In other words, we use all the pre-reform cohorts as the baseline. The results are presented in Table 3. The main picture remains unchanged. We observe that the 1955-cohort responds significantly stronger than the 1954-cohort, while there is no statistically significant difference between the point estimate on the 1955- and 1956-cohorts, although the point estimate for the youngest cohort is higher.

Table 3: Efficient DID on saving-rate responses. Average treatment effect and cohort specific effects.

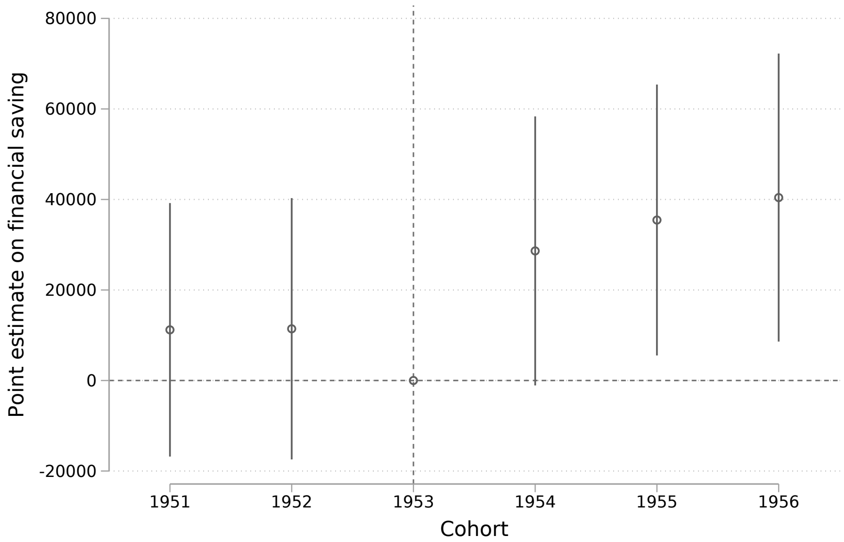
	(1)	(2)	(3)	(4)
Average Effect	0.011***	0.009**	0.011***	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
Cohort specific effect				
1954 cohort δ_{1954}	0.007	0.006	0.007	0.006
	(0.005)	(0.005)	(0.005)	(0.005)
1955 cohort δ_{1955}	0.012**	0.010*	0.012**	0.010*
	(0.005)	(0.005)	(0.005)	(0.005)
1956 cohort δ_{1956}	0.014***	0.011**	0.014***	0.011**
	(0.005)	(0.005)	(0.005)	(0.005)
Private sector worker	-0.001	-0.007*	-0.001	-0.007*
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	156,506	156,506	156,506	156,506
Age FE	YES	YES	NO	NO
Cohort FE	YES	YES	YES	YES
Controls	YES	NO	YES	NO
Number of clusters	31,327	31,327	31,327	31,327
F-stat	11.31	6.875	11.32	6.877
DF numerator	35	7	35	7
DF denominator	31,326	31,326	31,326	31,326

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Notes: Annual data from 2010–2017 using cohorts 1953–1956. The 1953-cohort serves as baseline. Standard errors in parentheses, clustered on the individual level. Control variables reported in Appendix Table B.2. The average effect is calculated using the cohort specific effects as $\sum_{j=1954}^{1956} \omega_j \delta_j$, where ω_j is the share of private workers in cohort j among all treated private workers, and we use the variance-covariance matrix in Appendix Table B.6 to retrieve the standard error.

We have also looked at the reform effects on the levels of household saving instead of the saving rate. We find that the reform on average increase the private household saving by around 27,000 NOK during the 5 years period we study, as shown by the reduced form results in Table 4. As we see in Figure 3, the overall pattern of the reform effects over different cohorts are very similar to that we found for the saving rate (Figure 2). The placebo reform effects are close to zero and statistically insignificant, which implies that the common trend assumption holds also for household saving for the pre-reform cohorts. Additionally, the reform effects increase over the cohorts, suggesting potentially a dosage effect.

Figure 3: Cohort specific reform effects on sum of financial saving (between ages 57–61)



Notes: Estimated coefficients and corresponding 95%-confidence intervals on saving rate for each cohort. Annual data from 2008–2017 using cohorts 1951–1956. The 1953-cohort serves as baseline.

5.3 Assessing the magnitude of the saving response

To assess the magnitude of the response, we use the saving measured in levels rather than the saving rate, as discussed in Section 4.3. We also define the change in pension wealth (Δ Pension) as the expected pension wealth in the reformed system minus the expected pension wealth in the non-reformed system (measured in NOK). Both components in Δ Pension are calculated using the micro-simulation model MOSART, see Section 3. In the pension wealth measure, we include all relevant pension wealth data, which covers the entire social security pension system, the occupational pensions and the early retirement (AFP) scheme.

The estimation results are shown in Table 4. The coefficients are interpreted as follows. The first-stage shows that on average the private sector workers lost approximately 244,000 NOK ($\approx$ USD 28,000) more than their public sector counterparts as a result of the reform. This is the average across all treated cohorts (1954–1956). The point estimate on the sector affiliation shows that there is generally a level difference between private and public sector workers common to both pre- and post-reform cohorts. The reduced form coefficient shows that on average the private sector workers saved 27,000 NOK more than their public sector counterparts as a result of the reform. The 2SLS coefficient is a rescaling of the reduced form by dividing by the first stage. The interpretation is that a one unit increase in "Δ Pension", i.e. an *increase* in pension wealth of 1 NOK, induces a reduction in household saving of 0.111 NOK. When individuals lose benefits, they therefore save about 11.1 percent of that loss across these 5 years.

Table 4: Average reform effect on household saving (sum of ages 57–61).

	OLS	2SLS	First stage	Reduced form
	Saving	Saving	Δ Pension	Saving
Δ Pension	0.003	-0.111***		
	(0.003)	(0.037)		
Treatment effect			-244,239***	27,270***
			(10,333)	(8,898)
Private sector worker	21,871***	5,877	-25,630***	8,650
	(5,688)	(7,287)	(9,386)	(6,662)
Observations	229,224	229,224	231,536	229,224
Number of clusters	45,889	45,889	46,351	45,889

$^{*} p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$

Notes: Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors in parentheses, clustered on the individual level. Control variables reported in Appendix Table B.3.

As we discussed in Section 4.3, we need to correct our first stage outcome variable (the change in the expected pension wealth) for individuals' remaining planning horizon. Since using the MOSART model allows us to retrieve unbiased estimates of expected longevity on the individual level, we can also compute Gale's Q (Gale, 1998) at the individual level using this information.

We adjust the first stage effect (on Δ Pension) by assuming that the reform was known in 2008. This is the year when the reform was publicly announced following the Pension Commission's report (Ot.prp. nr. 37, 2008–2009). Individuals have thus had since 2008 to reoptimize their saving behavior, and will have until death to continue to reoptimize. We assume full information, i.e. that every individual knows about the reform at the same time. We also assume that the individuals follow a CRRA-class utility function of the form presented in Eq. (C.2) in Appendix C. We parameterize using an impatience parameter $\beta = 0.98$, a long-term real interest rate $r = 1.5\%$ (which means that $\mathrm{R} = 1 + r = 1.015$), and an (inverse) intertemporal elasticity of substitution $\gamma = 1.5$ (i.e. $\mathrm{IES} = 1/\gamma = 2/3$).

On average the rescaling is about 25–30 times the annual level, or about 5–6 times the 5-year sum from Table 4, and the exact rescaling varies on the individual level according to the expected longevity. The results are shown in Table 5.

Table 5: Two-stage least squares. Average treatment effect across ages 57–61. Male-head households. Correcting first-stage using Gale's Q.

	OLS	2SLS	First stage	Reduced form
	Saving	Saving	Δ Pension	Saving
Δ Pension	0.015	-0.565***		
	(0.017)	(0.186)		
Treatment effect			-48,116***	27,270***
			(1,857)	(8,898)
Private sector worker	21,766***	11,971*	5,813***	8,651
	(5,678)	(6,241)	(1,743)	(6,662)
Observations	229,224	229,224	231,536	229,224
Number of clusters	45,889	45,889	46,351	45,889

$^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$
Notes: Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors clustered on the individual level. First-stage outcome variable (Δ Pension) corrected for Gale's Q using R = 1.015, β = 0.98 and γ = 1.5. Control variables reported in Appendix Table B.4.

Because this adjustment, or rescaling, takes into account that the reoptimization can occur over the entire remaining lifetime, the 2SLS estimate can directly be interpreted as the offset effect of pension wealth on household saving. The interpretation is thus that the offset effect amounts to 56.5 percent.

5.4 Heterogeneity in saving responses

An offset effect of 0.5–0.6 is in line with what Bottazzi et al. (2006) found for pension reforms in Italy, Aguila (2011) in Mexico, and Attanasio and Rohwedder (2003) in the UK, all using methods similar to this paper. However, none of the above-mentioned provide results for the exact same age group as in our analysis. Usually, the offset effect refers to all households, but in cases where estimates are reported for sub-groups of the age distribution, responses tend to be stronger for older households as in Attanasio and Rohwedder (2003), and Lachowska and Myck (2018). The latter, who find a comparatively low overall offset effect of 0.29, still find a stronger effect of 0.39 for the "older" cohorts in their treated sample.[15] Furthermore, they find a stronger offset effect among more highly educated households.

Since it may have been difficult to comprehend the exact implications of the pension reform, one reason for the less than full offset effect in our results could be a lack of financial literacy among individuals with low education. Alessie et al. (2013), for example, find that among individuals

[15] In the Polish reform that they study, only individuals who were age 50 and younger were affected by the reform.

with low education pension wealth does not replace private wealth, whereas for individuals with high education the substitution is almost complete. Furthermore, Bottazzi et al. (2006), find a crowd out effect of 0.80 among individuals informed about the pension system, compared to only 0.44 among uninformed individuals, and Chetty et al. (2014) find that better educated individuals are more likely to be "active" savers. If so, we would expect the response of a highly educated household to be stronger than one with low education. Using an extended life cycle model, Jia and Zhu (2012) show that the existence of risks and market imperfections, such as uninsured risk on earnings, mortality risk, borrowing constraints, as well as bequest motive are important reasons why the empirical estimates of the offset effect is much lower than 1.

In order to investigate potential heterogeneity in responses across sub-groups of the sample, we employ interactions between our outcome variable in the 2SLS and dummies for subgroups. Column (1) in Table 6 reports estimates of the saving rate responses. Furthermore, column (2) reports estimates of the unadjusted coefficients corresponding to the overall estimate in Table 4, while column (3) reports estimates of adjusted estimates based on individual Gales' Q coefficients. Since the individual Gales' Q depend on individual expected longevity, which correlates with education and income, we note that the adjusted coefficients may become very large for subgroups with higher than average longevity, such as those with high education.

Among observable characteristics, education seems to play an important role for the saving response to a loss in pension wealth. As discussed above, this finding is in line with the literature and may indicate that individuals with high education are more informed and more likely to be active savers. We find a similar pattern for earnings and final pension point, although less pronounced.

Earnings denotes here the household earnings level at the time of pre-determination, i.e. two years before out sample period, while final pension point is a mapping from life-time income as described in Section 2. Both measures are strongly correlated with education, so unsurprisingly, for both income measures we find a stronger offset effect for high incomes. Furthermore, we find smaller but statistically insignificant offset effects for lower incomes with the exception of the lowest earnings quintile.

Finally we investigate responses across quintiles of the financial assets distribution. Here we find much less dispersion in responses. For households in the lowest wealth quintile we observe an estimate of the unadjusted saving response that is in line with what we find for the lowest earnings group and lowest educational group. Since there characteristics are correlated these are likely to be more or less the same households. From a policy maker's viewpoint one might worry that the low income, low asset group are not saving enough to compensate for the loss in pension wealth. However, our results indicate that even this group did respond to the reform, albeit with a weaker offset effect than high income households.

Table 6: Heterogeneity of reform effects on saving behavior: saving rates and offset effects

		Offset effect	
Subgroups	Saving rate	Unadjusted	Gale's Q adjusted
#Primary and lower secondary education	0.008***	-0.058*	-0.215
	(0.002)	(0.033)	(0.166)
#Upper secondary education	0.014***	-0.123***	-0.537**
	(0.003)	(0.044)	(0.223)
#College/university	0.024***	-0.368***	-2.105***
	(0.004)	(0.101)	(0.580)
#Earnings 1st quintile	0.011***	-0.068**	-0.347**
	(0.004)	(0.030)	(0.162)
#Earnings 2nd quintile	0.010***	-0.044	-0.194
	(0.003)	(0.036)	(0.192)
#Earnings 3rd quintile	0.012***	-0.067	-0.302
	(0.003)	(0.056)	(0.300)
#Earnings 4th quintile	0.008**	-0.115	-0.536
	(0.003)	(0.080)	(0.423)
#Earnings 5th quintile	0.019***	-0.210***	-1.114***
	(0.003)	(0.068)	(0.365)
#Final pension point [0,4)	0.001	-0.021	-0.080
	(0.007)	(0.035)	(0.196)
#Final pension point [4,6)	0.011***	-0.061*	-0.278
	(0.002)	(0.037)	(0.189)
#Final pension point>= 6	0.019***	-0.217***	-1.143***
	(0.003)	(0.061)	(0.323)
#Financial assets 1st quintile	0.009**	-0.065*	-0.329
	(0.004)	(0.038)	(0.211)
#Financial assets 2nd quintile	0.013***	-0.118**	-0.603**
	(0.003)	(0.046)	(0.246)
#Financial assets 3rd quintile	0.011***	-0.101**	-0.503*
	(0.003)	(0.049)	(0.257)
#Financial assets 4th quintile	0.013***	-0.119**	-0.605**
	(0.003)	(0.052)	(0.282)
#Financial assets 5th quintile	0.015***	-0.141**	-0.723**
	(0.004)	(0.061)	(0.328)

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

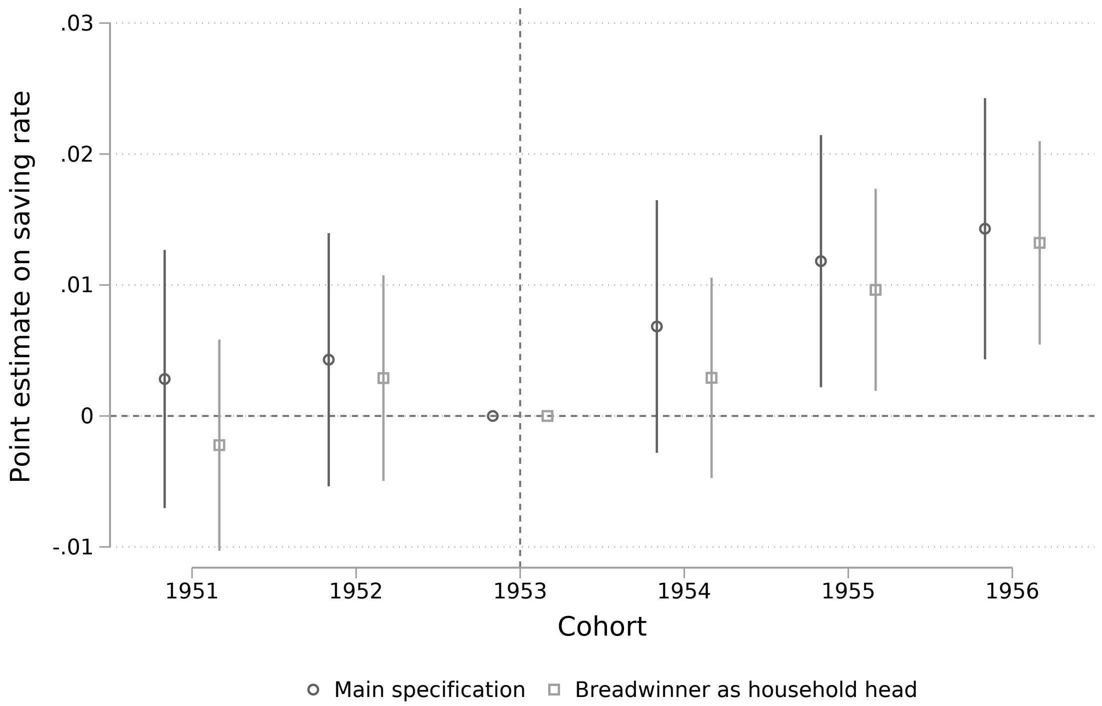
Notes: Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors clustered on the individual level. First-stage outcome variable (Δ Pension) corrected for Gale's Q using R = 1.015, β = 0.98 and γ = 1.5.

5.5 Robustness checks

5.5.1 Household head

We argued in Section 4 that in these cohorts, we may use men as a proxy for household head. We test this assumption in this section by redefining household head as the household member with the highest earnings in the year when the *youngest* member is aged 55. On average, women in the relationship is about 2 years younger than their male counterparts, which tends to bias toward men having higher earnings.[16] The number of observations increases for two reasons. First, single women are included in the estimation, whereas when using men we only include single men. Second, we now include *all* households where the head of the household is born in 1951–1956, whereas in our main specification we only use the men born in these cohorts.[17] Overall, we get relatively more single households (divorced/separated or single with no partner) in this exercise.

Figure 4: Cohort specific effects, by definition of household head



Notes: Estimated coefficients and corresponding 95%-confidence intervals on saving rate for each cohort. Annual data from 2008–2017 using cohorts 1951–1956. The 1953-cohort serves as baseline.

[16]A third option would be to define household head by comparing earnings in the year when each member respectively is aged 55 (or some other age). We chose not to do this, because it would involve macroeconomic effects.

[17]Theoretically, the latter may have ambiguous effects. If many men born in 1951–1956 have spouses born outside of those cohorts who also are the head of the household, the couple will no longer be in the sample. The exercise shows that the sample size increases because there are in fact about 30 percent of households where the woman earns the most when the youngest spouse is 55 years of age.

From Figure 4, we observe that the cohort specific treatment effects do not change substantially by changing the definition of household head. We observe that the point estimate for the 1955 and 1956 cohorts suggest a marginally stronger response when using male as the definition of the household head, although the difference is far from statistically significant. When we include some women as household head, a similar response in terms of saving rate will generally reduce the offset-effect due to the average income being lower for women than men.

From Table 7, it is clear that allowing for women as household heads if they earn more than their husbands, reduces both the saving response (DID) and the treatment effect, i.e. the reduction in expected pension wealth, which might be due to the Norwegian pension system quite significantly favoring women (see Halvorsen & Pedersen, 2019). Nevertheless, the offset effect is somewhat smaller than in our main specification, but not by unreasonable amounts. The estimates are well within one standard deviation of one another.

Table 7: Two-stage least squares. Average treatment effect across ages 57–61. Main earner when youngest spouse is 55 years of age as household head. Correcting first-stage using Gale's Q.

	OLS	2SLS	First stage	Reduced form
	Saving	Saving	Δ Pension	Saving
Δ Pension	0.020	-0.502***		
	(0.014)	(0.155)		
Treatment effect			-42,328***	21,255***
			(1,617)	(6,490)
Private sector worker	16,084***	-470	-11,662***	5,379
	(4,323)	(6,214)	(1,447)	(5,015)
Observations	347,354	347,354	350,859	347,354
Number of clusters	69,534	69,534	70,235	69,534

$^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

Notes: Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors clustered on the individual level. First-stage outcome variable (Δ Pension) corrected for Gale's Q using R = 1.015, β = 0.98 and γ = 1.5. Control variables reported in Appendix Table B.5.

5.5.2 Estimation of Gale's Q

Gale's Q hinges on several critical assumptions; the timing of the reform (in baseline set to 2008) and the parameters R, β and γ. Moreover, Gale's Q assumes that individuals reoptimize in *every* period of the remaining lifetime, whereas one could argue that in practice the reoptimization might stop at some earlier time. If the reoptimization period stopped sooner, the point estimate would generally be lower. However, a later reoptimizing *starting* period would increase the point estimate in the 2SLS, because of the assumption of linearity in the responses.

We proceed to test the robustness of the Gale's Q correction factor, by varying the information period and the parameters. The latest that the reform can be argued to be known by the public is at its implementation in 2011. We therefore use this year as a robustness check. Furthermore, we test the critical assumptions of the levels of the parameters β, γ and R specifically. The results are presented in Table 8.

Table 8: Robustness check of Gale's Q. 2SLS-coefficients.

Information year	2008	2011	2008	2008	2008	2008
Interest rate	r = 1.5	r = 1.5	r = 0	r = 1.5	r = 1.5	r = 1.5
Discount factor	$\beta = 0.98$	$\beta = 0.98$	$\beta = 0.98$	$\beta = 0.99$	$\beta = 0.98$	$\beta = 0.98$
Risk aversion coef.	$\gamma = 1.5$	$\gamma = 1.5$	$\gamma = 1.5$	$\gamma = 1.5$	$\gamma = 1.0$	$\gamma = 2.0$
2SLS	-0.565***	-0.627***	-0.594***	-0.605***	-0.555***	-0.570***
	(0.186)	(0.206)	(0.195)	(0.199)	(0.183)	(0.187)

$^{*} p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$

Notes: First column (grey) is the baseline specification. Annual data from 2008–2017 using cohorts 1951–1953 (control) versus 1954–1956 (treatment). Standard errors clustered on the individual level. No of observations = 229,224, no of clusters = 45,889.

Table 8 confirms that the choice of parameters will affect the estimated offset-effect, but not by unacceptable amounts. Our choices of parameter values have reasonable arguments. We set the real interest rate to 1.5 percent, which reflects the average of the historical real interest rates in Norway quite well.[18] The impatience-parameter β affects the offset-effect, and in general, if individuals are more impatient, the offset effect is smaller. The mechanism is straightforward: more impatient individuals care less about a future loss in expected pension wealth, and therefore do not increase their saving as much as more patient individuals. In the literature, a commonly assumed value of β is 0.98 (see e.g. Lachowska & Myck, 2018; Attanasio & Brugiavini, 2003; Bottazzi et al., 2006; Feng et al., 2011). We also observe that γ has little impact on the offset-effect, and a smaller γ (i.e. a higher IES) leads to only a slightly larger offset-effect. The mechanism is that individuals then tend to care slightly more about consumption smoothing. In the literature, assumed values for γ in the CRRA-utility class ranges from about 1.5 to 2 (see e.g. Low & Pistaferri, 2015; Low et al., 2018; Galaasen & Kruse, 2020). As is evident from Table 8, the choice of these structural parameter values is of minor importance to the estimated offset-effect.

6 Conclusion

In this study, we have exploited variation across cohorts in (loss of) expected pension wealth due to a pension reform in Norway. This variation allowed us to study the impact of pension benefit gen-

[18] For short periods of time, the real interest rate may vary significantly from this broad average. For individuals who reoptimize in periods where the real interest rate is lower, this would give a higher offset effect and vice versa.

erosity on the intertemporal saving behavior of households. Our main estimate suggests a lifetime offset effect of about 56 percent in Norwegian households, i.e. for every dollar less in expected lifetime pensions, households save about 56 cents over their remaining lifetime.

We argue that our estimate is of particular interest due to the rich nature of our data and the clean identification of incentives due to our reform-setup. Our rich register data allowed us to follow individuals over long period as well as both birth cohorts who were affected by the reform and birth cohorts who were unaffected by the reform. Firstly, we find that over a period of five years, households increased their savings rate around 1.2 percentage points (annually) and on in total saved about 27,000 NOK more as a result of the loss of about 244,000 NOK in expected pension wealth, i.e. about one tenth. Secondly, using Gale's Q to rescale this effect and retrieve the lifetime saving response, we find an estimated offset effect of 56 percent by using reasonable parameter values and the CRRA-utility class, as well as individual-specific expected longevity computed using a dynamic microsimulation model. Finally, we find that although saving rate responses and offset effects are larger for highly educated households and high-income households, even the low income, low asset households responded to the reform by saving more.

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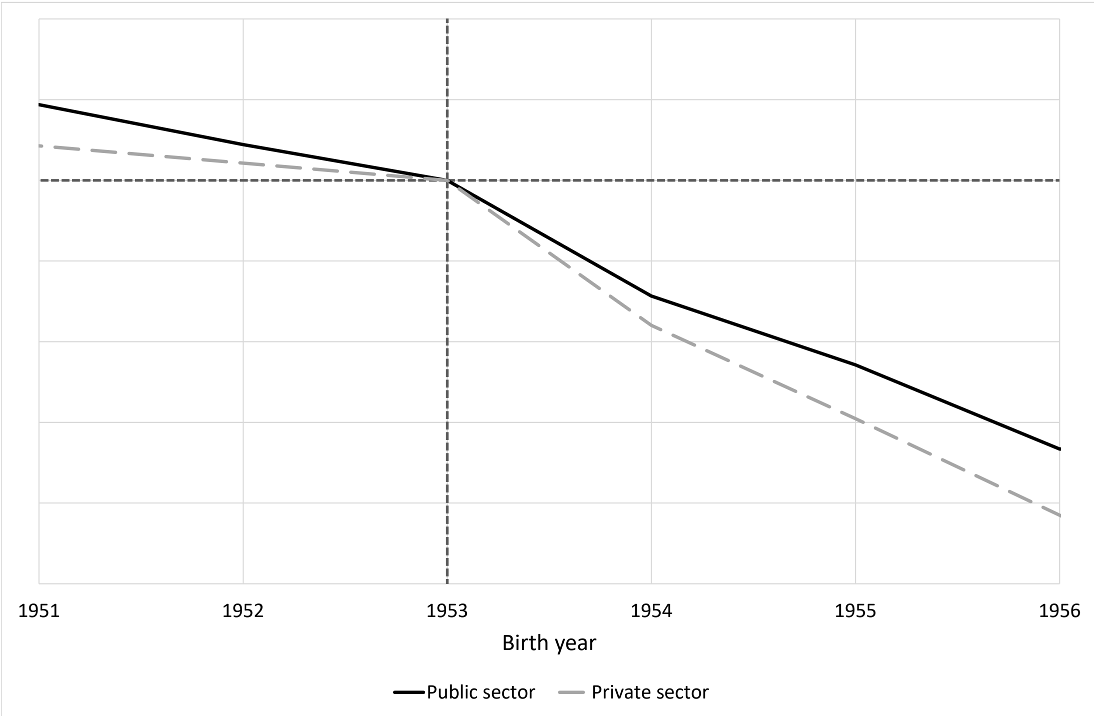
A Additional statistics

Figure A.1: Expected pension wealth loss for a typical male household with constant income



Notes: Between the lines, the only variation is the sector affiliation of the individual. Everything else is held fixed, so that the line represents the pure shift in expected pension wealth from sector affiliation. The household is a male who claims pension and retires at age 67, has 5,5G in constant income for 40 years and dies at age 85.

Table A.1: Labor market transition rates between ages 57–61

		Stayed in sector	Switched sector	Left labor market
Public sector at age 55, cohorts 1951–1953	Age 57	0.907	0.0736	0.0192
	Age 58	0.885	0.0843	0.0308
	Age 59	0.873	0.0938	0.0337
	Age 60	0.841	0.116	0.0428
	Age 61	0.828	0.122	0.0499
Public sector at age 55, cohorts 1954–1956	Age 57	0.915	0.0641	0.0205
	Age 58	0.894	0.0748	0.0313
	Age 59	0.882	0.0847	0.0336
	Age 60	0.848	0.107	0.0452
	Age 61	0.829	0.114	0.0566
Private sector at age 55, cohorts 1951–1953	Age 57	0.973	0.0173	0.0098
	Age 58	0.964	0.0221	0.0142
	Age 59	0.955	0.0255	0.0191
	Age 60	0.943	0.0285	0.0281
	Age 61	0.929	0.0330	0.0376
Private sector at age 55, cohorts 1954–1956	Age 57	0.977	0.0147	0.0086
	Age 58	0.965	0.0225	0.0125
	Age 59	0.955	0.0289	0.0161
	Age 60	0.941	0.0338	0.0249
	Age 61	0.927	0.0359	0.0373

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).

B Extended results

Table B.1: Control variables for specifications (1) and (3) in Table 2.

	(1)	(3)
Immigrant	-0.719	-0.723
	(0.452)	(0.453)
Number of children	-0.003***	-0.003***
	(0.001)	(0.001)
Cohabitation basis = 1, Married / cohabitant with children	-0.008**	-0.008**
	(0.004)	(0.004)
Cohabitation basis = 2, Cohabitant couple	-0.020***	-0.020***
	(0.005)	(0.005)
Cohabitation basis = 3, Divorced	0.005	0.005
	(0.005)	(0.005)
Cohabitation basis = 4, Separated	0.015***	0.015***
	(0.005)	(0.005)
Cohabitation basis = 5, Widow / widower	-0.003	-0.002
	(0.010)	(0.010)
Cohabitation basis = 6, Cohabitant couple, partner deceased	-0.015	-0.014
	(0.023)	(0.023)
Cohabitation basis = 7, Married, living apart	-0.008	-0.007
	(0.015)	(0.015)
Has defined contribution plan, private sector	-0.011***	-0.011***
	(0.002)	(0.002)
Has defined benefit plan, private sector	-0.008***	-0.008***
	(0.003)	(0.003)
Has a pension capital certificate	-0.000	-0.000
	(0.002)	(0.002)
Has a paid-up policy	-0.000	-0.000
	(0.002)	(0.002)
Spouse treated	0.002	0.002
	(0.002)	(0.002)
Mosart computed expected longevity	-0.000	-0.000
	(0.000)	(0.000)
Education group = 2,	0.002	0.002
	(0.002)	(0.002)
Education group = 3	0.001	0.001
	(0.002)	(0.002)
Final pension point group = 2,	-0.001	-0.001
	(0.004)	(0.004)
Final pension point group = 3	-0.002	-0.002
	(0.004)	(0.004)
Pre-earnings quintile = 2,	0.005**	0.005**
	(0.002)	(0.002)
Pre-earnings quintile = 3	0.007***	0.007***
	(0.002)	(0.002)

Continued...

Table B.1 – continued

	(1)	(3)
Pre-earnings quintile = 4	0.004	0.004
	(0.003)	(0.003)
Pre-earnings quintile = 5	0.005	0.005
	(0.004)	(0.004)
Household assets quintile = 2,	0.012***	0.012***
	(0.002)	(0.002)
Household assets quintile = 3	0.024***	0.024***
	(0.002)	(0.002)
Household assets quintile = 4	0.032***	0.032***
	(0.002)	(0.002)
Household assets quintile = 5	0.034***	0.034***
	(0.002)	(0.002)

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).
Cohabitation basis: 0 = single (base)
Education groups: 1 = primary school, 2 = upper secondary education, 3 = higher education
Final pension point groups: 1 = 0-4G, 2 = 4-6G, 3 = 6G+

Table B.2: Control variables for specifications (1) and (3) in Table 3.

	(1)	(3)
Immigrant	-0.778	-0.786
	(0.931)	(0.926)
Number of children	-0.002***	-0.002***
	(0.001)	(0.001)
Cohabitation basis = 1, Married / cohabitant with children	-0.010**	-0.010**
	(0.005)	(0.005)
Cohabitation basis = 2, Cohabitant couple	-0.021***	-0.021***
	(0.005)	(0.005)
Cohabitation basis = 3, Divorced	0.007	0.007
	(0.006)	(0.006)
Cohabitation basis = 4, Separated	0.016**	0.016***
	(0.006)	(0.006)
Cohabitation basis = 5, Widow / widower	-0.014	-0.013
	(0.012)	(0.012)
Cohabitation basis = 6, Cohabitant couple, partner deceased	-0.022	-0.020
	(0.029)	(0.029)
Cohabitation basis = 7, Married, living apart	-0.008	-0.006
	(0.016)	(0.016)
Has defined contribution plan, private sector	-0.012***	-0.012***
	(0.002)	(0.002)
Has defined benefit plan, private sector	-0.007**	-0.007**
	(0.003)	(0.003)
Has a pension capital certificate	0.000	0.000
	(0.002)	(0.002)
		Continued...

Table B.2 – continued

	(1)	(3)
Has a paid-up policy	0.001	0.001
	(0.002)	(0.002)
Spouse treated	0.005*	0.005*
	(0.003)	(0.003)
Mosart computed expected longevity	-0.000*	-0.000*
	(0.000)	(0.000)
Education group = 2b,	0.003	0.003
	(0.002)	(0.002)
Education group = 3	0.004	0.004
	(0.003)	(0.003)
Final pension point group = 2b,	0.003	0.003
	(0.004)	(0.004)
Final pension point group = 3	0.002	0.002
	(0.005)	(0.005)
Pre-earnings quintiles = 2b,	0.004	0.004
	(0.003)	(0.003)
Pre-earnings quintiles = 3	0.006**	0.006**
	(0.003)	(0.003)
Pre-earnings quintiles = 4	0.003	0.003
	(0.003)	(0.003)
Pre-earnings quintiles = 5	0.001	0.001
	(0.005)	(0.005)
Household assets quintiles = 2b,	0.011***	0.011***
	(0.003)	(0.003)
Household assets quintiles = 3	0.023***	0.023***
	(0.003)	(0.003)
Household assets quintiles = 4	0.030***	0.030***
	(0.003)	(0.003)
Household assets quintiles = 5	0.031***	0.031***
	(0.003)	(0.003)

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).
Cohabitation basis: 0 = single (base)
Education groups: 1 = primary school, 2 = upper secondary education, 3 = higher education
Final pension point groups: 1 = 0-4G, 2 = 4-6G, 3 = 6G+

Table B.3: Control variables for Table 4.

	OLS	2SLS	First stage	Second stage
Birth year = 1952	15,234.185**	4,086.449	-100,940.405***	15,452.630**
	(6,526.247)	(7,484.191)	(11,001.002)	(6,528.047)
Birth year = 1953	14,317.231**	-4,657.137	-171,876.650***	14,523.806**
	(6,798.220)	(9,168.582)	(11,010.665)	(6,770.705)
Birth year = 1954	28,425.467***	-2,486.568	-116,910.840***	10,585.480
	(6,934.105)	(12,288.502)	(10,169.457)	(9,017.242)
				Continued...

Table B.3 – continued

	OLS	2SLS	First stage	Second stage
Birth year = 1955	45,861.047***	5,619.048	-199,315.259***	27,903.010***
	(7,057.232)	(14,881.116)	(10,068.420)	(9,052.208)
Birth year = 1956	51,640.509***	1,460.021	-286,669.628***	33,371.749***
	(7,467.483)	(18,114.832)	(10,202.510)	(9,508.337)
Immigrant	-698,125.328**	-749,501.284**	-536,714.403***	-696,679.898**
	(353,820.136)	(353,188.406)	(141,203.990)	(353,532.585)
Number of children	-13,464.512***	-14,072.117***	-5,118.683*	-13,467.666***
	(1,927.315)	(1,953.171)	(2,685.497)	(1,927.365)
	(9,191.645)	(10,021.647)	(15,378.772)	(9,172.244)
Cohabitation basis = 1,	22,174.377**	35,069.520***	112,920.468***	22,317.083**
Married / cohabitant with children	(9,191.645)	(10,021.647)	(15,378.772)	(9,172.244)
Cohabitation basis = 2,	-7,393.535	-574.029	59,575.451***	-7,302.595
Cohabitant couple	(10,415.953)	(10,662.504)	(16,874.541)	(10,407.346)
Cohabitation basis = 3,	13,716.812	8,663.314	-42,851.408**	13,415.283
Divorced	(10,312.941)	(10,627.603)	(17,276.376)	(10,311.915)
Cohabitation basis = 4,	13,288.734	6,570.848	-58,032.747***	12,876.726
Separated	(9,720.317)	(10,136.215)	(16,670.557)	(9,720.099)
Cohabitation basis = 5,	-24,427.933	-25,920.418	-9,092.819	-24,912.082
Widow / widower	(21,895.760)	(22,145.277)	(31,491.551)	(21,903.084)
Cohabitation basis = 6,	-38,415.573	-42,169.522	-30,691.474	-38,838.692
Cohabitant couple, partner deceased	(33,540.161)	(34,208.608)	(61,767.934)	(33,523.705)
Cohabitation basis = 7,	7,910.706	-9,777.992	-148,725.564***	6,654.167
Married, living apart	(25,730.516)	(26,796.644)	(37,283.234)	(25,748.415)
Has defined contribution plan, private sector	-29,880.597***	-35,742.309***	-32,149.924***	-32,059.293***
	(4,966.611)	(5,494.912)	(8,467.749)	(5,034.761)
Has defined benefit plan, private sector	-22,033.036***	-3,164.365	176,314.575***	-23,057.546***
	(8,321.872)	(10,375.180)	(13,201.685)	(8,325.719)
Has a pension capital certificate	-4,631.903	-8,264.654	-31,162.378***	-4,715.981
	(4,920.392)	(5,178.064)	(8,659.007)	(4,920.754)
Has a paid-up policy	-1,242.917	17,711.226**	168,783.169***	-1,188.436
	(4,714.012)	(7,822.517)	(7,514.481)	(4,678.066)
Spouse treated	5,416.273	7,973.578	23,493.963*	5,348.065
	(7,902.708)	(8,092.132)	(12,161.127)	(7,897.668)
Mosart computed expected longevity	-400.460***	-24.778	3,377.839***	-404.193***
	(146.164)	(192.418)	(220.724)	(145.744)
Education group = 2	5,538.092	11,362.637**	51,593.981***	5,536.330
	(4,302.109)	(4,761.505)	(7,400.585)	(4,297.001)

Continued...

Table B.3 – continued

	OLS	2SLS	First stage	Second stage
Education group = 3	-1,365.960	-22,875.362**	-184,914.597***	-2,220.031
	(6,466.408)	(9,590.166)	(8,446.762)	(6,443.594)
Final pension point group = 2	11,387.117	18,694.204**	66,389.402***	11,272.010
	(7,000.423)	(7,524.192)	(12,082.626)	(6,999.636)
Final pension point group = 3	10,992.508	16,649.939	54,574.008***	10,760.869
	(10,283.166)	(10,603.893)	(16,164.262)	(10,286.863)
Pre-earnings quintile = 2	16,975.412***	24,046.791***	62,354.972***	17,397.676***
	(6,001.782)	(6,522.099)	(9,958.642)	(6,006.144)
Pre-earnings quintile = 3	28,085.438***	34,262.659***	54,719.559***	28,593.158***
	(7,389.491)	(7,697.456)	(10,708.969)	(7,389.232)
Pre-earnings quintile = 4	28,771.516***	32,041.469***	29,667.002**	29,234.245***
	(9,312.392)	(9,417.001)	(12,182.524)	(9,312.540)
Pre-earnings quintile = 5	37,260.483**	35,241.540**	-16,290.639	37,944.148**
	(15,814.216)	(15,866.584)	(18,984.040)	(15,806.482)
Household assets quintile = 2	25,800.098***	28,205.607***	21,675.331**	25,741.948***
	(5,773.905)	(5,931.774)	(9,387.867)	(5,771.195)
Household assets quintile = 3	55,698.989***	59,339.755***	33,818.611***	55,741.041***
	(5,937.800)	(6,158.483)	(9,469.666)	(5,936.128)
Household assets quintile = 4	88,388.121***	91,695.798***	28,759.800***	88,505.227***
	(6,133.175)	(6,324.956)	(9,608.235)	(6,132.520)
Household assets quintile = 5	87,197.593***	91,979.422***	40,796.196***	87,280.595***
	(7,040.792)	(7,314.935)	(9,875.222)	(7,033.918)
Constant	[-1,029.993]	[-25,149.947]	[-303,364.457]	[8,432.526]

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).
Cohabitation basis: 0 = single (base)
Education groups: 1 = primary school, 2 = upper secondary education, 3 = higher education
Final pension point groups: 1 = 0-4G, 2 = 4-6G, 3 = 6G+

Table B.4: Control variables for Table 5.

	OLS	2SLS	First stage	Second stage
Birth year = 1952	15,266.489**	4,282.281	-19,483.448***	15,452.630**
	(6,527.116)	(7,451.272)	(2,132.750)	(6,528.047)
Birth year = 1953	14,367.925**	-4,160.110	-32,900.680***	14,523.806**
	(6,802.075)	(9,051.884)	(2,095.389)	(6,770.705)
Birth year = 1954	28,486.186***	-842.213	-20,031.168***	10,585.480
	(6,937.205)	(11,837.503)	(1,861.912)	(9,017.242)
Birth year = 1955	45,913.004***	8,793.504	-33,561.444***	27,903.010***
	(7,057.413)	(13,959.610)	(1,841.245)	(9,052.208)
Birth year = 1956	51,665.466***	6,938.606	-46,635.052***	33,371.749***
	(7,463.195)	(16,475.348)	(1,857.668)	(9,508.337)
Immigrant	-698,240.975**	-738,503.487**	-83,686.049***	-696,679.898**
	(353,789.158)	(354,512.415)	(25,509.791)	(353,532.585)
Number of children	-13,467.238***	-13,890.184***	-692.501	-13,467.666***

Continued...

Table B.4 – continued

	OLS	2SLS	First stage	Second stage
	(1,927.219)	(1,946.324)	(488.584)	(1,927.365)
Cohabitation basis = 2,	-7,410.968	-782.723	11,446.326***	-7,302.595
Cohabitant couple	(10,416.333)	(10,638.551)	(3,133.594)	(10,407.346)
Cohabitation basis = 3,	13,736.019	8,577.954	-8,456.438***	13,415.283
Divorced	(10,312.873)	(10,618.586)	(3,228.987)	(10,311.915)
Cohabitation basis = 4,	13,307.042	6,733.095	-11,038.007***	12,876.726
Separated	(9,720.179)	(10,110.105)	(3,124.290)	(9,720.099)
Cohabitation basis = 5,	-24,420.782	-26,002.065	-1,847.578	-24,912.082
Widow / widower	(21,896.123)	(22,118.951)	(5,845.581)	(21,903.084)
Cohabitation basis = 6,	-38,401.383	-42,229.998	-6,032.548	-38,838.692
Cohabitant couple, partner deceased	(33,543.276)	(34,014.344)	(11,286.139)	(33,523.705)
Cohabitation basis = 7,	7,892.407	-6,812.659	-23,678.185***	6,654.167
Married, living apart	(25,732.273)	(26,612.799)	(6,951.644)	(25,748.415)
Has defined contribution plan, private sector	-29,817.570***	-37,396.459***	-9,299.639***	-32,059.293***
	(4,972.715)	(5,714.426)	(1,530.201)	(5,034.761)
Has defined benefit plan, private sector	-22,014.884***	-6,275.298	29,196.656***	-23,057.546***
	(8,320.909)	(9,792.270)	(2,298.089)	(8,325.719)
Has a pension capital certificate	-4,598.311	-9,081.067*	-7,664.315***	-4,715.981
	(4,920.330)	(5,235.262)	(1,568.116)	(4,920.754)
Has a paid-up policy	-1,249.740	15,542.478**	29,389.181***	-1,188.436
	(4,712.554)	(7,257.707)	(1,356.249)	(4,678.066)
Spouse treated	5,413.645	7,746.141	4,230.361*	5,348.065
	(7,902.434)	(8,064.581)	(2,180.979)	(7,897.668)
Mosart computed expected longevity	-393.952***	-321.263**	141.902***	-404.193***
	(145.762)	(148.711)	(19.419)	(145.744)
Education group = 2	5,527.295	11,028.227**	9,571.307***	5,536.330
	(4,302.499)	(4,707.493)	(1,347.067)	(4,297.001)
Education group = 3	-1,351.709	-20,662.640**	-32,533.291***	-2,220.031
	(6,464.861)	(9,054.156)	(1,517.425)	(6,443.594)
Final pension point group = 2	11,373.341	18,283.497**	12,337.507***	11,272.010
	(6,999.922)	(7,459.624)	(2,190.376)	(6,999.636)
Final pension point group = 3	10,980.865	16,369.260	10,238.550***	10,760.869
	(10,281.849)	(10,561.976)	(2,922.084)	(10,286.863)
Pre-earnings quintile = 2	16,962.576***	23,630.421***	11,465.999***	17,397.676***
	(6,002.397)	(6,460.248)	(1,813.894)	(6,006.144)
Pre-earnings quintile = 3	28,072.707***	33,956.876***	10,167.237***	28,593.158***

Continued...

Table B.4 – continued

	OLS	2SLS	First stage	Second stage
	(7,389.864)	(7,659.258)	(1,943.833)	(7,389.232)
Pre-earnings quintile = 4	28,756.907***	32,179.908***	6,018.605***	29,234.245***
	(9,312.607)	(9,410.369)	(2,207.223)	(9,312.540)
Pre-earnings quintile = 5	37,257.507**	35,613.886**	-2,750.545	37,944.148**
	(15,813.907)	(15,848.927)	(3,437.192)	(15,806.482)
Household assets quintile = 2	25,790.006***	28,282.478***	4,391.024**	25,741.948***
	(5,774.129)	(5,923.661)	(1,708.885)	(5,771.195)
Household assets quintile = 3	55,681.180***	59,552.812***	6,926.193***	55,741.041***
	(5,938.346)	(6,161.218)	(1,722.254)	(5,936.128)
Household assets quintile = 4	88,371.939***	91,889.489***	5,985.019***	88,505.227***
	(6,133.389)	(6,326.164)	(1,747.431)	(6,132.520)
Household assets quintile = 5	87,186.885***	91,775.240***	7,651.189***	87,280.595***
	(7,040.599)	(7,287.514)	(1,794.041)	(7,033.918)
Pre-earnings (at age 55), demeaned	-4,356.699**	-4,087.220*	269.692	-4,372.532**
	(2,205.899)	(2,195.285)	(377.055)	(2,204.370)
Constant	[-1,559.288]	[-1,858.713]	[-18,674.723]	[8,432.526]

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).
Cohabitation basis: 0 = single (base)
Education groups: 1 = primary school, 2 = upper secondary education, 3 = higher education
Final pension point groups: 1 = 0-4G, 2 = 4-6G, 3 = 6G+

Table B.5: Control variables for Table 7

	OLS	2SLS	First stage	Second stage
Birth year = 1952	10,226.606**	997.589	-18,359.500***	10,337.566**
	(5,188.715)	(5,881.722)	(1,643.633)	(5,190.640)
Birth year = 1953	10,623.753**	-7,236.258	-34,913.826***	10,342.753*
	(5,361.204)	(7,549.481)	(1,617.391)	(5,330.469)
Birth year = 1954	24,694.328***	-1,884.027	-28,557.545***	12,607.551**
	(5,469.438)	(9,554.203)	(1,435.786)	(6,395.638)
Birth year = 1955	39,980.396***	8,225.352	-38,891.499***	27,824.772***
	(5,558.969)	(10,793.831)	(1,411.709)	(6,377.616)
Birth year = 1956	47,759.410***	9,824.380	-50,714.852***	35,362.919***
	(5,786.992)	(12,693.339)	(1,395.873)	(6,671.511)
Immigrant	-567,480.092**	-583,465.881**	-41,305.794**	-568,564.477**
	(229,212.487)	(230,270.423)	(19,296.891)	(228,898.589)
Number of children	-12,306.783***	-13,117.174***	-1,473.512***	-12,327.097***
	(1,552.835)	(1,579.997)	(386.106)	(1,552.204)
Cohabitation basis = 1,	37,743.955***	37,898.864***	242.698	37,640.968***
Married / cohabitant with children	(6,898.956)	(6,934.480)	(2,006.813)	(6,896.626)
Cohabitation basis = 2,	3,004.517	4,005.996	1,954.339	2,960.858
Cohabitant couple	(8,129.171)	(8,183.886)	(2,237.843)	(8,126.534)

Continued...

Table B.5 – continued

	OLS	2SLS	First stage	Second stage
Cohabitation basis = 3,	12,024.566	3,656.012	-15,764.816***	11,542.127
Divorced	(7,325.928)	(7,825.335)	(2,138.537)	(7,326.901)
Cohabitation basis = 4,	12,797.452*	5,186.566	-14,321.194***	12,307.137*
Separated	(6,974.624)	(7,409.880)	(2,078.761)	(6,973.593)
Cohabitation basis = 5,	-30,366.321***	-68,010.770***	-71,899.487***	-31,901.622***
Widow / widower	(10,925.260)	(15,901.396)	(3,453.092)	(10,906.315)
Cohabitation basis = 6,	-38,170.267*	-48,766.795**	-21,120.091***	-38,173.462*
Cohabitant couple, partner deceased	(22,937.138)	(23,436.820)	(5,727.609)	(22,908.567)
Cohabitation basis = 7,	31,555.738	17,016.273	-27,348.159***	30,783.142
Married, living apart	(20,738.823)	(21,306.142)	(5,015.496)	(20,751.630)
Has defined contribution plan, private sector	-26,284.811***	-29,039.829***	-1,364.221	-28,351.039***
	(4,291.119)	(4,474.191)	(1,322.501)	(4,368.114)
Has defined benefit plan, private sector	-16,387.694**	-4,254.509	25,751.159***	-17,283.832**
	(7,166.297)	(8,115.552)	(2,009.860)	(7,173.733)
Has a pension capital certificate	-7,843.628*	-8,269.662*	-912.655	-7,818.826*
	(4,312.624)	(4,378.644)	(1,381.075)	(4,312.789)
Has a paid-up policy	-4,613.121	10,647.945*	29,888.364***	-4,420.710
	(4,074.338)	(6,203.136)	(1,167.570)	(4,039.951)
Spouse treated	-1,239.530	7,126.823	16,226.579***	-1,050.978
	(7,861.426)	(8,357.500)	(2,180.368)	(7,853.242)
Mosart computed expected longevity	-229.826*	-170.840	134.176***	-240.786**
	(122.346)	(124.215)	(17.866)	(122.346)
Education group = 2	2,479.942	5,583.796	6,042.170***	2,472.675
	(3,568.954)	(3,713.441)	(1,076.213)	(3,568.181)
Education group = 3	1,767.933	-20,457.899**	-42,188.279***	696.352
	(4,930.625)	(8,283.916)	(1,144.889)	(4,892.743)
Final pension point group = 2	-2,581.825	10,080.610	24,862.035***	-2,426.072
	(5,018.951)	(6,274.605)	(1,370.734)	(5,014.013)
Final pension point group = 3	-1,979.718	13,109.140	30,012.634***	-1,781.024
	(8,795.934)	(9,880.851)	(2,281.992)	(8,801.021)
Pre-earnings quintiles = 2	11,016.645**	20,614.750***	17,883.797***	11,831.218**
	(5,468.946)	(6,211.393)	(1,466.080)	(5,471.548)
Pre-earnings quintiles = 3	25,162.382***	35,483.260***	18,900.751***	26,264.282***
	(6,883.557)	(7,572.496)	(1,741.937)	(6,887.196)
Pre-earnings quintiles = 4	28,962.659***	37,431.634***	15,816.582***	29,888.437***
	(8,365.225)	(8,745.617)	(1,934.932)	(8,366.718)
Pre-earnings quintiles = 5	34,304.031***	37,592.998***	5,526.120**	35,327.743***
	(12,676.984)	(12,735.423)	(2,708.118)	(12,678.226)
Household wealth quintiles = 2	20,687.215***	20,753.167***	223.965	20,635.580***
	(4,508.299)	(4,556.382)	(1,284.214)	(4,508.328)

Continued...

Table B.5 – continued

	OLS	2SLS	First stage	Second stage
Household wealth quintiles = 3	54,855.913***	55,331.737***	1,133.355	54,814.785***
	(4,663.686)	(4,709.627)	(1,310.559)	(4,664.285)
Household wealth quintiles = 4	84,710.426***	83,849.728***	-1,491.855	84,655.930***
	(4,820.871)	(4,870.837)	(1,336.785)	(4,820.417)
Household wealth quintiles = 5	75,050.053***	74,217.720***	-1,832.613	74,997.963***
	(5,570.790)	(5,624.659)	(1,371.877)	(5,570.818)
Pre-earnings (at age 55), demeaned	-4,616.565**	-3,855.224**	1,284.035***	-4,611.093**
	(1,939.996)	(1,949.579)	(331.430)	(1,937.618)
Constant	[-1,382.491]	[8,859.277]	[5,497.230]	[5,906.205]

Notes: Pre-determined earnings are measured in base amounts, i.e. a one unit increase in pre-determined earnings equals 106,399 NOK (in 2021).
Cohabitation basis: 0 = single (base)
Education groups: 1 = primary school, 2 = upper secondary education, 3 = higher education
Final pension point groups: 1 = 0-4G, 2 = 4-6G, 3 = 6G+

Table B.6: Variance-covariance matrix for Table 3.

		d	1954 cohort δ_{1954}	1955 cohort δ_{1955}	1956 cohort δ_{1956}
(1)	d	0.0000162	-0.0000129	-0.0000126	-0.0000123
	1954 cohort δ_{1954}	-0.0000129	0.0000242	0.0000128	0.0000128
	1955 cohort δ_{1955}	-0.0000126	0.0000128	0.0000242	0.000013
	1956 cohort δ_{1956}	-0.0000123	0.0000128	0.000013	0.000026
(2)	d	0.0000129	-0.0000129	-0.0000129	-0.0000129
	1954 cohort δ_{1954}	-0.0000129	0000244	0.0000129	0.0000129
	1955 cohort δ_{1955}	-0.0000129	0.0000129	0.0000243	0.0000129
	1956 cohort δ_{1956}	-0.0000129	0.0000129	0.0000129	0.0000258
(3)	d	0.0000162	-0.0000129	-0.0000126	-0.0000123
	1954 cohort δ_{1954}	-00.0000129	0.0000242	0.0000128	0.0000128
	1955 cohort δ_{1955}	-00.0000126	0.0000128	0.0000242	0.000013
	1956 cohort δ_{1956}	-00.0000123	0.0000128	0.000013	0.000026
(4)	d	0.0000129	-0.0000129	-0.0000129	-0.0000129
	1954 cohort δ_{1954}	-0.0000129	0000244	0.0000129	0.0000129
	1955 cohort δ_{1955}	-0.0000129	0.0000129	0.0000243	0.0000129
	1956 cohort δ_{1956}	-0.0000129	0.0000129	0.0000129	0.0000258

C The bias correction parameter: Gale's Q

C.1 The complete market life-cycle model

The periodic utility function for the household is denoted as $U(c_t)$, where c_t is the household consumption. At period (age) t, the household solves for the consumption level c_t that maximizes the corresponding remaining lifetime utility:

$$\max_{\{c_t \geq 0\}} u(c_t) + \sum_{i=t+1}^{T} \beta^{i-t} u(c_i). \tag{C.1}$$

Where $0 < \beta < 1$ is the subjective discount factor, and T is the life expectancy.

The households are assumed to have the constant relative risk aversion (CRRA) preference:

$$u(c) = \frac{c^{1-\gamma} - 1}{1-\gamma}. \tag{C.2}$$

$1/\gamma$ is the inter-temporal substitution elasticity between consumption in any two periods, i.e., it measures the willingness to substitute consumption between different periods.

The budget constraint is given by:

$$\begin{aligned} c_t + a_{t+1} &= Ra_t + y_t, \quad t < \tau \\ c_t + a_{t+1} &= Ra_t + p_t, \quad t \geq \tau \end{aligned}$$

Where y_t is the wage income, p_t pension income at time t, τ is the retirement age.and $R = 1 + r$.

The Euler equation at year t can be written as:

$$u'(c_t) = [\beta R] u'(c_{t+1})$$

we then have:

$$c_{t+1} = [\beta R]^{1/\gamma} c_t. \tag{C.3}$$

Based on this, we can solve for the optimal consumption, saving and asset levels for all t. Denote these as c_t^*, s_t^*, and a_t^*, respectively. All of these variables can be written as functions of pension wealth, lifetime earnings and preference parameters. Many empirical studies on the relationship between pension wealth and saving are based on this relationship. Note that in this setup, no reform is needed – individual variations in pension wealth are the key to identification of the offset effects.

C.2 Recovering the offset effect through a DiD design

There is reason to be skeptical to such identification strategy, due to the potential endogeneity problem of the pension wealth. Therefore, it is preferable to use exogenous shocks to pension wealth to identify the offset effect, such as an unexpected reform.

Suppose that households are informed about the unexpected pension reform at age $t_0 < \tau$, so they will re-optimize and start to change consumption behavior from age t_0 to the end of life. As there is no bequest motive, we must have:

$$\sum_{j=t}^{T} \frac{1}{R^{j-t}} c_j = Ra_t + \sum_{j=t}^{\tau-1} \frac{1}{R^{j-t}} y_j + \sum_{j=\tau}^{T} \frac{1}{R^{j-t}} p_j. \tag{C.4}$$

By using (C.3), it can be rewritten as:

$$c_t \cdot (\sum_{j=t}^{T} \frac{1}{R^{j-t}} [\beta R]^{\frac{j-t}{\gamma}} - 1) = a_{t+1} + \sum_{j=t+1}^{\tau-1} \frac{1}{R^{j-t}} y_j + \sum_{j=\tau}^{T} \frac{1}{R^{j-t}} p_j \tag{C.5}$$

Define $p^t = \sum_{j=\tau}^{T} R^{t-j} p_j$, which is the accumulated pension wealth evaluated at age t. Furthermore, denote $\omega_t = \sum_{j=t}^{T} \frac{1}{R^{j-t}} [\beta R]^{\frac{j-t}{\gamma}}$. We then have:

$$\frac{\partial a_{t+1}}{\partial p^t} = \left[\frac{\partial c_t}{\partial p^t} (\omega_t - 1) - 1 \right] \tag{C.6}$$

Note that using (C.3) and (C.4), the age specific consumption at t can be written as:

$$c_t = [\beta R]^{(t-t_0)/\gamma} C_{t_0} = \left[\left(\sum_{j=t_0}^{\tau-1} R^{t_0-j} y_j \right) + \left(\sum_{j=\tau}^{T} R^{t_0-j} p_j \right) + Ra_{t_0} \right] \frac{[\beta R]^{(t-t_0)/\gamma}}{\omega_{t_0}} \tag{C.7}$$

which is:

$$c_t = Y_t + p^t \frac{[\beta R]^{(t-t_0)/\gamma} R^{t_0-t}}{\omega_{t_0}} \tag{C.8}$$

By using (C.8), Eq. (C.6) becomes:

$$\begin{aligned}\frac{\partial a_{t+1}}{\partial p^t} &= \left[\frac{[\beta R]^{(t-t_0)/\gamma} R^{t_0-t}}{\omega_{t_0}}(\omega_t - 1) - 1\right] \\ &= -\frac{\sum_{j=t_0}^{t} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}}{\sum_{j=t_0}^{T} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}}.\end{aligned} \tag{C.9}$$

The above model describes the saving behavior under the complete market life-cycle model, which serves as the benchmark. The derived "offset effect" implied by (C.9) should be -1.

Thus the bias correction parameter can be defined as:

$$Q(t) = \frac{\sum_{j=t_0}^{t} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}}{\sum_{j=t_0}^{T} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}} \leq 1. \tag{C.10}$$

The intuition here is that when pension wealth changes, individuals' saving adjustment is gradually realized. So the response observed at a given age does not represent a full picture of the offset effect, a point which was emphasized by Gale (1998).

If we are running a regression on saving instead of the assets, then we have from (C.8):

$$s_t = y_t - Y_t - p^t \frac{[\beta R]^{(t)/\gamma} R^{-t}}{\sum_{j=t_0}^{T} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}}$$

This gives us the adjustment factor for the relationship between saving at age t and lifetime pension wealth:

$$Q(t) = \frac{\frac{1}{R^t}[\beta R]^{\frac{t}{\gamma}}}{\sum_{j=t_0}^{T} \frac{1}{R^j}[\beta R]^{\frac{j}{\gamma}}}. \tag{C.11}$$

Note that the adjustment factor defined in (C.10) is for the accumulated assets, while (C.11) is for the annual saving.

In our case, we have used a two-way fixed effects DiD design (Eq. 3):

$$Y_{i,t} = \alpha Z_{i,t} + \beta S_i + \lambda_t + \theta_c + \gamma X_{i,t} + \varepsilon_{i,t}. \tag{C.12}$$

Under the strong parallel trend assumption, a consistent estimate $\hat{\alpha}$ recovers the average causal responses $\frac{\partial s_t}{\partial Z_{i,t}}$, at age t. Note that in the above setting we have $Z_{i,t} = p_i^t - \bar{p}_i^t$ instead of p_i^t. However, this should not matter if we assume that given the observable characteristics and the pension wealth loss due to the reform $Z_{i,t}$, the pension wealth level under the old system $\bar{p}_i^t$ does not provide additional information on the saving behavior, i.e. $E\left(s_t | X_t, Z_{i,t}\right) = E\left(s_t | X_t, Z_{i,t}, \bar{p}_i^t\right)$. This implies that we can use the same adjustment factor defined in (C.11).