C ENTER FOR
E CONOMIC
B EHAVIOR &
I NEQUALITY

CEBI WORKING PAPER SERIES

Working Paper 05/24

THE IMPACT OF SOCIAL SECURITY ELIGIBILITY AND PENSION WEALTH ON RETIREMENT

Johan Sæverud

ISSN 2596-447X

CEBI
Department of Economics
University of Copenhagen
www.cebi.ku.dk

The Impact of Social Security Eligibility and Pension Wealth on Retirement*

Johan Sæverud†

April 29, 2025

Abstract

I investigate a Danish policy reform that postpones social security eligibility in response to increases in life expectancy. The reform creates sharp discontinuities based on exact birth dates, enabling the identification of causal effects. Using both administrative and survey data, I document a substantial increase in labor force participation of 20 percentage points as a result of the delay. The effect is strongest among individuals with low pension wealth. This pattern holds across multiple retirement age thresholds and cohorts, including both those who have already retired and younger cohorts still in the labor force. The findings provide new evidence on the effects of life expectancy-based adjustments to social security eligibility. The estimated net fiscal gain of delaying eligibility by half a year is €8.4k per affected individual. However, the effect is unevenly distributed, with low-wealth individuals contributing more than high-wealth individuals, raising concerns about the equity implications of such reforms.

Keywords: retirement age, social security, labor supply

JEL Codes: J26, H55

*The author is grateful for support from CEBI. The activities of CEBI are financed by the Danish National Research Foundation, Grant DNRF134.

†University of Copenhagen, CEBI. Email: js@econ.ku.dk

1 Introduction

Rising life expectancy in developed nations has led to increased public expenditures due to extended retirement periods. A prevalent strategy to mitigate these costs is to delay the eligibility age for social security benefits, thereby incentivizing later retirement[1]. At the same time, another change has taken place globally: a shift in pension schemes away from defined benefits towards defined contributions[2]. This shift has resulted in substantial individual occupational pension wealth in many developed countries. All else equals, the rise in pension assets gives more freedom for the individual to choose when to retire and may lead to earlier retirement, potentially weakening the effects of delayed social security eligibility[3].

Understanding the joint effect of pension wealth accumulation and delayed eligibility on retirement decisions is crucial for assessing labor supply responses and the policy's distributive consequences. Securing fiscal sustainability may require many countries to delay social security eligibility much beyond what has currently been implemented. However, empirical examination of the implications of delaying social security eligibility ages remains sparse.

I examine how delayed social security eligibility across different levels of pension wealth in defined contributions (DC) pension accounts impacts retirement decisions in Denmark. Denmark is a front-runner due to its 2006 pension reform, which linked social security eligibility to life expectancy, positioning cohorts now entering the labor force to qualify for benefits around their mid-70s. At the same time, collective bargaining agreements have mandated worker contributions to DC pension accounts since the early 1990s, leading to significant accumulation of individual retirement wealth.

To establish the causal impact of the delayed eligibility age on retirement behavior, I use a regression discontinuity design centered around specific age thresholds where eligibility age increases sharply at a cutoff. For older cohorts affected by the reform, I rely on population-wide administrative records, documenting pension savings and retirement decisions following the policy change. This allows me to estimate the causal effect of the policy change on labor market participation.

[1]Some countries implement this delay as a one-off measure, yet it is plausible they will have to implement further delays as life expectancy continues to rise to maintain fiscal sustainability. For an analysis of retirement age reforms in different countries, see Börsch-Supan and Coile (2018); Lee and Mason (2011); OECD (2021); Börsch-Supan and Coile (2023).

[2]This transition to defined contribution schemes is documented in Choi et al. (2002); Beshears et al. (2014); Poterba et al. (2011); Saez and Zucman (2016); Clark and Mitchell (2002); Diamond (2009); Gruber and Wise (1999); Skinner (2007).

[3]Brown et al. (2010) show how unexpected inheritances increases the probability of retiring.

Relying only on administrative data, we can only speculate about the effect of the reform for future retirees. To address this concern, I leverage a novel survey of 5,000 respondents from younger cohorts who have not yet reached their social security eligibility age. In the survey I ask respondents to state their preferred retirement age in various hypothetical scenarios, and this enables me to quantify the expected effect of the policy for cohorts who have not yet become eligible and who can expect to become eligible well into their 70s. In this way, I am able to project the long-term implications of the reform on retirement decisions.

I find that the policy reform increases labor force participation by 20 percentage points measured three months after birth date cutoff where the control group is eligible but not the treated group. This change reflects a causal increase in labor force participation in response to the reduced eligibility for social security benefits. However, the effect of the reform on labor supply is not uniform but varies significantly by accumulated pension wealth. Administrative data suggests that those with low pension wealth are considerably more likely to increase labor force participation (by 36 percentage points), when affected by the reform. In contrast, individuals with substantial pension wealth only increase labor force participation by 8 percentage points. Effects based on expectations data for younger cohorts mirror these findings, indicating that the reform is likely going to affect expected retirement in a similar way in the future, especially for those with low expected pension wealth. By conducting a similar exercise with retirement age as the outcome variable I find that individuals delay retirement by 3.3 months when the eligibility age increases by half a year (a 55% pass-through rate). Similarly, there is a gradient in pension wealth: those with low pension wealth delay by 4.5 months and those with high pension wealth delay by 1.8 months.

Finally, I quantify the net fiscal effects of the reform. A potential concern is that, in the absence of social security, older workers may substitute into other welfare programs, leaving the government budget largely unaffected. To address this, I calculate the net fiscal impact of delaying the eligibility age by half a year by comparing the difference in average annual taxes minus transfers before and after retirement, scaled by the behavioral response to the reformthat is, the increase in actual retirement age. I find that the average net fiscal gain per affected individual is €8.4k. However, the effect varies substantially by pension wealth: individuals with low pension wealth contribute €10.6k, while those with high pension wealth contribute only €6.1k. This gradient underscores the distributional nature of the reform, raising equity concerns as those with fewer resources contribute more to achieving the policys fiscal objective.

I contribute to a large body of literature on retirement policies, employing quasi-experimental data to estimate causal impact of policies designed to increase labor supply for people close to retirement.

I provide estimates that align with findings from other countries, such as Austria (Staubli and Zweimüller (2013); Manoli and Weber (2016)), France (Rabate and Rochut (2020)), Germany (Geyer and Welteke (2021)), the Netherlands (Rabaté et al. (2024)), Switzerland (Lalive et al. (2017)), the UK (Cribb et al. (2016)), and the US (Mastrobuoni (2009); Behaghel and Blau (2012)).

Another strand of retirement literature highlights the role of reference dependence in retirement decisions, suggesting that merely financial incentives cannot fully explain observed behaviors (Seibold (2021)). Similarly, Lalive et al. (2023) demonstrates how claiming and retirement decisions are coupled in a non-optimal way, partly due to reference dependence. Although I do not explicitly address the underlying mechanisms behind the observed responses, my work contributes by documenting the overall response and its implications.

Furthermore, my research adds a dimension to the literature by explicitly considering the role of pension wealth in shaping retirement decisions. Previous studies, such as those considering liquid wealth in the Netherlands (Rabaté et al. (2024)) and housing wealth in the UK (Cribb et al. (2016)), have found minimal or non-existent effects of wealth on retirement decisions in response to reforms. Contrasting these findings, my research shows a significant impact of pension wealth on retirement decisions. By analyzing how the increase in social security eligibility age influences different population groups stratified by pension wealth, my work uncovers a potential increase in inequality. This redistributive aspect of the reform, previously unexplored, is made possible by the extensive Danish registries, which uniquely combine detailed pension wealth with high-frequency data documenting labor supply.

Common to previous retirement studies is the exclusive use of ex-post data and the omission of consideration for eligibility ages above 65[4]. In contrast, my work addresses these open questions by examining both the realized responses of older workers to eligibility ages up to 67 and the anticipated labor supply trajectories of younger generations, who will become eligible well into their 70's. Using survey data provides insights into the long-term effects of a social security reform that ties the eligibility age to life expectancy. I

[4]Except for Rabaté et al. (2024) where they consider ages up to 66 years and 4 months

demonstrate that even in expectation, the reform's impact is heterogeneous across levels of anticipated pension wealth.

While I investigate how pension wealth influences retirement decisions, other research has probed the opposite relationship. Specifically, García-Miralles and Leganza (2024) examined the same Danish reform I discuss, emphasizing the effects of delaying early retirement ages (from 60 to 61) on voluntary pension savings, concluding that reduced social security provisions amplify the savings rate. Conversely, Etgeton et al. (2023) measure the effects of a German retirement reform and show that a higher early retirement age decreases the savings rate. Similarly, both Artmann et al. (2023) and Becker et al. (2022) use another German reform to document that a boost in social security wealth leads to reduced labor participation for mothers during their careers.

1.1 Road map

The paper is organized as follows. Section 2 explains the Danish pension system and the reform in question. Section 3 presents the administrative data and the survey data. Section 4 explains regression discontinuity as identification strategy. Section 5 presents the results from both administrative and survey data across different pension wealth levels. Section 6 computes the net fiscal effects on the government budget. Section 7 concludes.

2 Structure of the Danish Pension System

The Danish pension system consists of three main components: social security benefits, occupational pensions, and individual supplementary pensions. See Chetty et al. (2014) for more details on the Danish pension system.

2.1 Social Security

Social security in Denmark is a monthly benefit that is paid out to all citizens above an age cutoff (age 65 until the reform in question). The benefit consists of a base amount of €10,800 annually and a supplement of €12,500 for single-residents (€6,400 for people living as a couple)[5]. The benefit level is universal[6] and not tied to labor market performance.

However, the base amount is means-tested against earnings with a high disregard (€48,100 annually), meaning that only earnings above a certain threshold reduce the benefit. The supplement is means-tested against all personal income, including pensions

[5]All amounts are CPI-indexed to 2023 levels and converted to Euros: 1€= 7.46 DKK.

[6]The level of benefits is reduced proportionally for individuals who have lived less than 40 years in Denmark.

and investments, with a lower disregard (€12,200 annually for single-residents, €24,500 for people living as a couple). These means-testing thresholds are adjusted annually to reflect shifts in average wages and living costs. As a result, while the basic structure remains universal, the actual benefit received may vary significantly based on individual income levels.

An important characteristic of the Danish social security system is its progressive transfer system. The benefit amount typically decreases as lifetime earnings increase because individuals with higher lifetime earnings often accumulate greater pension wealth, which reduces their social security benefit through the means-testing mechanism. This structure results in relatively higher benefits for individuals with lower lifetime earnings and lower benefits for those with substantial pension wealth.

This design contrasts with less progressive transfer systems observed in many other countries, where social security benefits are more closely tied to lifetime earnings. For example, in the United States, the Social Security system uses the average indexed monthly earnings (AIME) over the 35 highest-earning years, with a formula that still favors lower earners to some extent but significantly ties benefits to contributions. Similarly, in Germany, the public pension system calculates benefits based on a points system that reflects individual contributions over the working life, leading to higher benefits for those with higher lifetime earnings.

In Sweden, the pension system follows a similar less progressive pattern. The Swedish system is predominantly based on notional defined contributions (NDC), where benefits depend on lifetime earnings and contributions. While there is a guaranteed pension for low-income individuals, the primary public pension is linked to the individuals average lifetime income, resulting in higher payouts for those with higher lifetime earnings.

Claiming is voluntary at the eligibility age and postponing the claim results in an actuarially fair increase in the benefit level.

The Danish social security system is unfunded, meaning that the benefits are paid for over the general tax system rather than through dedicated contributions or accumulated funds.

2.2 The Welfare Agreement and Its Implications

In 2006, a majority (158 members out of 179) in the Danish parliament supported the Welfare Agreement, a reform that introduced a delay in statutory eligibility age for social security. Instead of maintaining a universal eligibility age of 65, the reform linked eligibility age to projected life expectancy to ensure fiscal sustainability. The stated goal is that in expectation the period on social security should be 15 years. In practice, the parliament votes every five years on the eligibility age 15 years in the future[7]. The timing of the 2006 reform was moved up in 2011, resulting in the reform taking effect as early as 2019.

This reform exclusively addresses eligibility ages and does not affect other aspects of the retirement system, such as benefit levels or the calculation of pension wealth. This change introduces some uncertainty in retirement planning, as individuals may face varying eligibility ages depending on future adjustments to life expectancy projections. Nonetheless, the reform is designed to align social security provisions more closely with demographic trends.

Table 1 provides an overview of the affected cohorts (column 1), at what age they are affected (column 2), when they are affected (column 3), and the sample sizes used in the analysis in Section 5 (column 4). The same information is visualized in Figure 1 where the steps show the eligibility ages and the shaded areas indicate cohorts who are compared. The figure clearly shows how the reform was phased in steeply in 2019 and then more gradually from 2022.

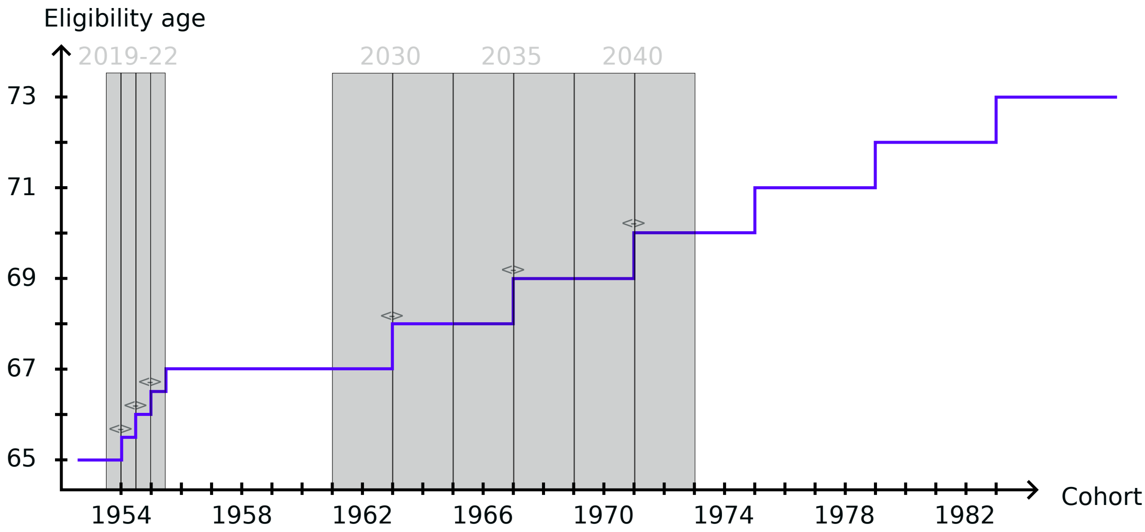
Table 1: *Reform details*

	Birth dates	Eligibility age	Starting year	# of observations control / treatment
Admin	-31 December 1953	65.0		
Admin	1 January 1954-	65.5	2019	10,979 / 11,931
Admin	1 July 1954-	66.0	2020	11,931 / 10,930
Admin	1 January 1955-	66.5	2021	10,930 / 12,099
Survey	1 July 1955-	67.0	2022	
Survey	1 January 1963-	68.0	2030	898 / 893
Survey	1 January 1967-	69.0	2035	887 / 821
Survey	1 January 1971-	70.0	2040	773 / 729
	1 January 1975-	71.0*	2045	
	1 January 1979-	72.0*	2050	
	1 January 1983-	73.0*	2055	

Note: The table provides an overview of the cohorts affected by the reform. The leftmost brackets indicate which part of the analysis the cohort pertains (administrative or survey data), column 1 lists the birth dates defining each cohort, column 2 shows the statutory eligibility age for social security, column 3 indicates the year in which that age becomes effective, and column 4 reports the sample sizes used in the analysis in Section 5, split into control and treatment groups. Eligibility ages marked with * are based on projected life expectancy and have not yet been voted on by parliament. Under the Welfare Agreement, eligibility ages are formally decided 15 years in advance, following a parliamentary vote every five years. The cohorts used in the administrative analysis are those born between mid-1953 and early 1955 (i.e., 1953.5, 1954.0, 1954.5, 1955.0), while the survey analysis focuses on later cohorts born between 1961 and 1972.

[7] In 2025 the eligibility age of 70 for people born 1971 or later was agreed upon. In 2030 there will be a vote on eligibility age of people born in 1975 or later-current projections put it at age 71.

Figure 1: *Post-reform statutory eligibility age by cohort*



Note: The figure illustrates the information presented in Table 1. The blue step line indicates the statutory eligibility age for social security across cohorts, reflecting the effect of the reform. Shaded bars highlight the specific cohorts used in the empirical analysis. The figure shows that the reform was implemented with a sharp increase in 2019, followed by a more gradual phase-in from 2022 onward. Grey numbers above the bars denote the calendar year in which each eligibility age becomes effective.

2.3 Occupational Pensions

Occupational pensions form the second pillar of the Danish pension system and play a crucial role in ensuring income security during retirement. These pensions are established by employers, often through collective bargaining agreements, and can only be withdrawn prematurely at a steep penalty. Occupational pensions were gradually introduced in Denmark from the early 1990s and are now the norm.

In the Danish labor market, 80% of workers are covered by a collective bargaining agreement that specifies the contribution rate to the worker's occupational pension. The negotiated contractual wage is net of pension contributions, and these rates are generally not considered a choice variable. Typical contribution rates are between 12-18%, and contributions are tax-exempt.

Occupational pension schemes in Denmark are predominantly defined contribution (DC) plans, where both the employer and the employee make contributions. Unlike defined benefit (DB) plans, which guarantee a specific payout, DC plans result in benefits that depend on accumulated contributions, investment returns, and the annuity conversion rates at retirement. This structure places more responsibility on the individual to manage their retirement savings effectively.

The accumulated funds can be taken as a lump sum, converted into an annuity, or a combination. Individuals can start to withdraw from their pension accounts five years before becoming eligible for social security, and the payouts are taxed as personal income.

There is significant variation in pension wealth across individuals, reflecting differences in earnings, contribution rates, and investment returns. For the cohort born in 1954, aged 60 in 2014, the quartiles of pension wealth were: Q25: €71,000, Q50: €134,000, Q75: €252,000, with a mean pension wealth of €191,000. A small fraction (3.5%) had pension wealth below €10,000, highlighting the unequal distribution of retirement savings.

Employees are automatically enrolled in occupational pension schemes unless they explicitly opt out, a policy that ensures broad coverage. The opt-out rate, however, is negligible. Additionally, the pension system supports portability, allowing individuals to transfer accumulated savings when changing jobs, which helps consolidate pension assets and reduces administrative costs.

2.4 Individual Supplementary Pensions

Supplementary pension schemes are voluntary and allow individuals to make additional savings beyond the state and occupational pension provisions. Contributions to individual supplementary pensions are typically tax-deductible and can be invested in a range of financial instruments, such as mutual funds or life insurance policies.

Annual contributions to supplementary pensions are around a tenth of the contributions to occupational pension wealth and they are particularly common among higher-income individuals who seek to enhance their retirement savings. These pensions provide an additional layer of financial security and flexibility, complementing the mandatory and occupational components of the system.

2.5 Early Retirement and Incentives

The Danish pension system offers an option for early retirement under specific conditions. Individuals may be eligible to receive early retirement benefits up to five years before reaching the statutory eligibility age (age 60 before the reform) if they have paid into the special early retirement scheme for at least 30 years and are members of an unemployment insurance fund. Introduced in the 1970s, this scheme has gradually been phased out as part of the Welfare Agreement, reflecting a shift toward promoting longer workforce participation and ensuring fiscal sustainability.

The annual early retirement benefit amount is €28,900, which is means-tested against earnings, occupational pensions, and pension stock. To encourage delaying early retirement, the system offers increased benefits for those who postpone by at least two years, raising the annual amount to €31,700.

The 2006 reform changed the eligibility age for early retirement in tandem with social security eligibility age and the effect can be seen in Figure 9 in Appendix A. Due to the ongoing phase-out of this scheme, I do not consider the group transitioning into early retirement in the analysis. Most younger workers will not be eligible when they reach the relevant age.

3 Data

3.1 Administrative Data

I use two detailed administrative datasets from the Danish population. The first dataset, covering the period 1987 to 2022, contains variables such as demographics, family ties, education, earnings, and savings. The second, a monthly record from 2008 to 2022, contains detailed employment information and comprehensive public transfer data, including social security payments.

For the analysis, I group individuals into half-year cohorts (e.g., 1953.5, 1954.0, 1954.5, 1955.0) based on date of birth (as eligibility ages differ), and use a regression discontinuity analysis to compare retirement decisions for adjacent cohorts. Referring to Table 1, these cohorts are in the first three rows and allow for two distinct comparisons (1953.5 vs 1954, 1954.0 vs 1954.5 and 1954.5 vs 1955.0). The rightmost column of Table 1 reports number of individuals in each comparison by control and treatment status. In Figure 1 the cohorts are depicted by the four narrow shaded bars around 1954 and the small arrows across the bars indicate a comparison. Cohorts 1955.5 and onwards are excluded as their retirement data is not yet available

3.1.1 Sample selection

In an effort to investigate the employment effects for the main target group of the reform, I focus on "regular" employees close to retirement. This means I exclude non-employees at age 59, the self-employed[8], early retirees, and individuals who previously received disability benefits. I restrict the sample to native Danes, due to the immigrant population's small size and heterogeneity. Refer to Appendix A for more details.

I define retirement as the first month where an individual is no longer employed, has no earnings, and starts receiving pensions from either social security or income from oc-

[8]Self-employment is defined herein as earning a larger portion of income from self-employment as opposed to wages.

cupational or private pension accounts. Supplementary analyses in Appendix D.1 confirm the robustness of this definition.

In line with the literature, I define labor supply as a binary variable measured three months after the control group's eligibility age. In a regression discontinuity setup this reflects the effect of the reform on the treated: how much do they work absent access to social security.

As an alternative measure I consider the change in retirement age of the treatment versus control group using a regression estimate. In this part of the analysis I limit a post-eligibility age that is available for the treated (the younger cohort is not observed as long as the older cohort).

I stratify individuals into quintiles based on accumulated pension wealth to estimate differential responses to the reform. I use pension wealth data from a year before potential withdrawals to address endogeneity concerns. Figure 12 in Appendix C reports the distribution of pension wealth at age 59 for the selected cohorts. Appendix D.2 compare different wealth types' impact on retirement decisions.

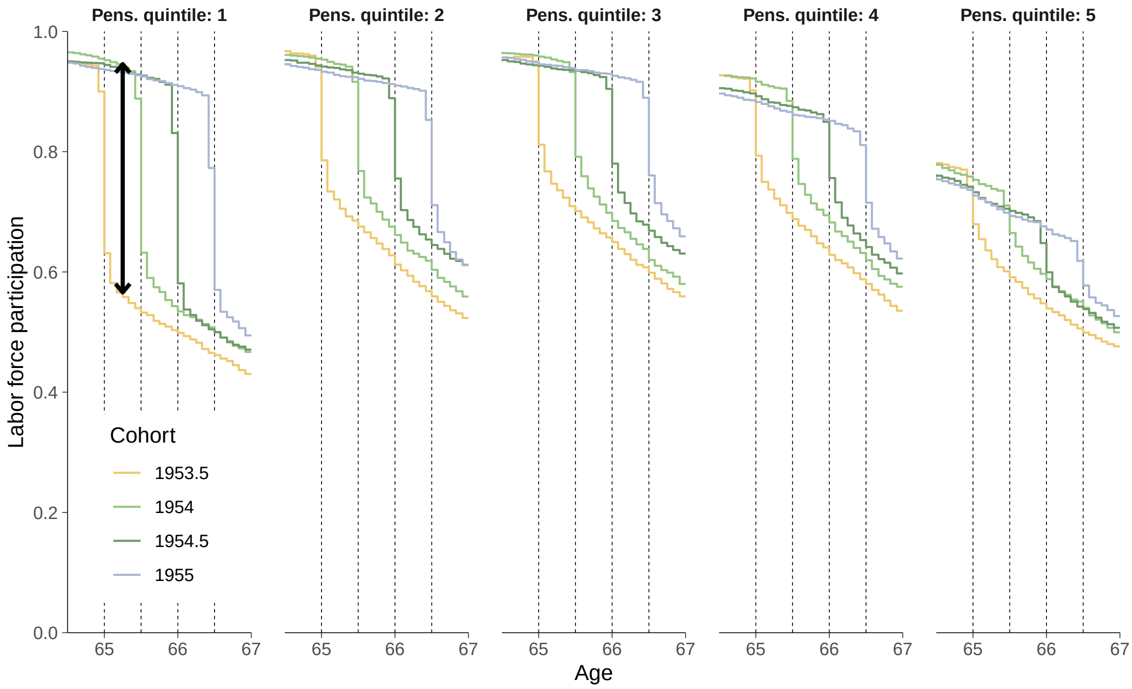
As a first-order check, Figure 2 plots labor force participation by cohort and pension wealth level for the administrative sample. There is a clear drop of between 10 and 30 percentage points in participation exactly at the statutory eligibility age. The figure shows that individuals respond notably to the reform, and that patterns across different cohorts (within each panel) are similar, suggesting that the cohorts are comparable. However, individuals with low pension wealth (Panel 1) exhibit a much larger response than those with high pension wealth (Panel 5). This descriptive result supports the hypothesis that individuals with higher pension wealth are less likely to retire exactly upon reaching eligibility. It is also notable that high-wealth individuals exhibit lower labor force participation already at age 65, indicating that a larger share retire before becoming eligible for social security. This again highlights the greater flexibility afforded by higher pension wealth. Figure 11 in Appendix B shows the full labor supply pattern from age 60 onwards, illustrating smooth transitions out of the labor force.

3.2 Survey Data

The survey instrument used in the analysis is part of the newly developed *Copenhagen Life Panel* [9] (CLP) which is an online panel survey implemented in Denmark. From the

[9] The *Copenhagen Life Panel* is an ongoing survey that was initiated in 2020 and is issued every year in January. See Caplin et al. (2023) on how income expectations can inform how we think about subjective earnings risk.

Figure 2: *Labor supply patterns, by pension wealth and cohort, administrative data*



Note: The five panels show transitions into retirement for individuals in the administrative data, grouped by pension wealth quintile. Within each panel, color denotes half-year cohorts. Vertical dashed lines indicate statutory eligibility ages. The arrow in Panel 1 highlights the difference in labor force participation at age 65 years and 3 months between the 1953.5 and 1954.0 cohorts—precisely when eligibility for social security benefits diverged.

administrative data we randomly select a group of individuals between 20 and 70 and invite them to participate in an online survey using unique personal id's. Invitations to participate are sent out using an official email account, called *e-boks*, which all Danes are equipped with.

After completing the survey, we link answers and id's back to the administrative data for all individuals who are invited to the survey. These data include standard demographic information, such as age, gender, education, earnings, hours worked, real estate, liquid wealth and pension wealth.

The questions in the CLP are all focused on expectations, and in this study I elicit the expected retirement responses of cohorts yet to experience the effect of the 2006 social security eligibility age reform. I consider only cohorts born from 1961 to 1972, with eligibility ages from 67 to 70 and I split them into brackets by eligibility age. From the invited 18,796 individuals across selected cohorts, 5,006 responded. The analytical framework and sample selection mirrors that of the administrative data, in the sense that I only consider those who expect to be employed at age 64, exclude self-employed and non-Danes. The cohorts considered in the analysis are indicated in Table 1, and the rightmost column reports the number of individuals in each comparison by treatment status. The numbers are an order of magnitude smaller than for the administrative sample, even when using a wider bandwidth. Figure 1 shows the compared cohorts across each discontinuity.

3.2.1 Survey Instrument

The survey contains various questions on expected future outcomes, and I focus on four retirement-related questions. These questions are structured to elicit both specific estimates and probability distributions, employing the "balls-in-bins" method. For details on the survey questions and the elicitation methodology, see Appendix E. I compare survey respondents and non-respondents on observables in Appendix E.1 and find that respondents are better educated, have higher employment rates, earn more, and have higher wealth. The relatively minor selection bias in the survey is not critical for the analysis, only if it was correlated with eligibility age brackets.

For each respondent, I collect data on the expected retirement age[10], and use it to create a time series dummy for labor supply. I then stratify individuals into pension wealth quintiles by their anticipated annual retirement income for each eligibility age bracket[11]. Figure 3 plots retirement transitions by cohort and pension wealth levels for the survey sample. The patterns are similar to Figure 2: consistent effect across cohorts (within panel) and decreasing effect in pension wealth (across panels). The results are more noisy due to the smaller sample size but the magnitudes are roughly comparable. The fact that expected retirement behavior as reported in the survey is so similar to the observed behavior in the administrative data indicates that the reform response will be consistent going forward, even for very high social security eligibility ages. This is true both for the size of the response, but also for the uneven pattern across pension wealth.

4 Identification Strategy

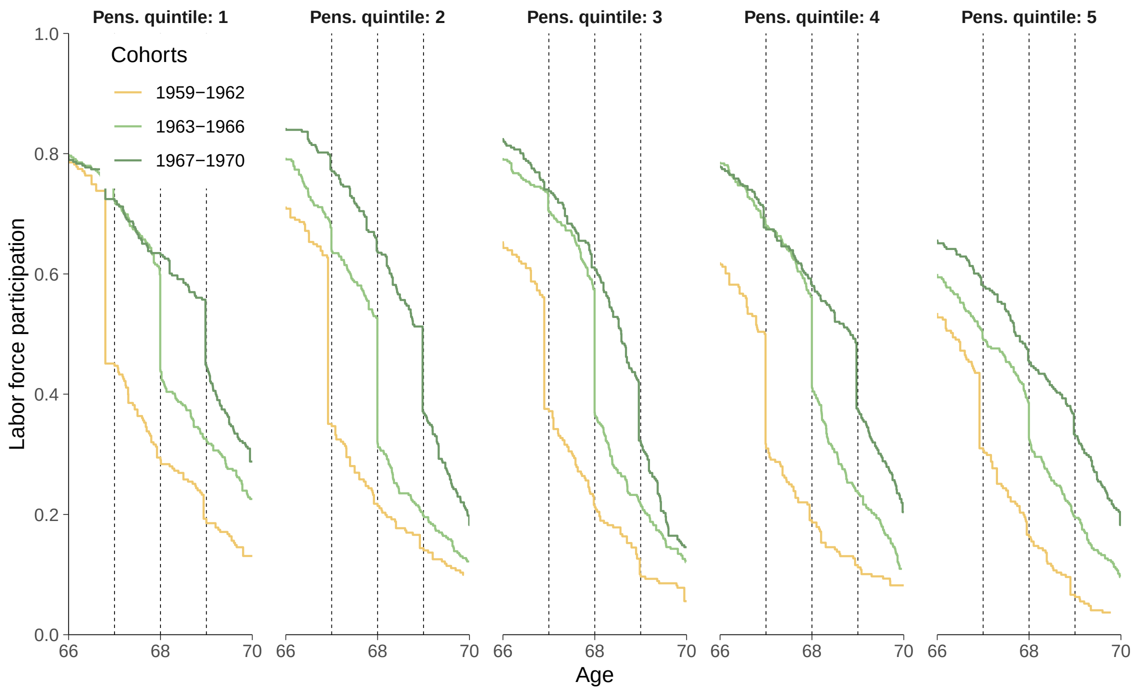
I use a regression discontinuity (RD) design as identification strategy as it overcomes selection biases and establishes causal effects in the presence of sharp discontinuities. It allows me to estimate the causal effect of a delay in social security eligibility age on the retirement decision.

The RD design relies on the assumption that individuals on either side of a sharp thresholdsuch as a date of birth cutoffare comparable, except for the treatment assignment

[10]For details, see Appendix E.2.

[11]The results are less clear when using current pension wealth to stratify, see Appendix D.3.

Figure 3: *Retirement patterns, by pension wealth and cohort, survey data*



Note: The five panels show expected labor force participation based on survey responses, grouped by pension wealth quintile. Within each panel, colors denote eligibility age brackets. Vertical dashed lines indicate the statutory eligibility ages corresponding to each bracket.

triggered by the discontinuity. In the context of retirement, these sharp discontinuities arise from age-related eligibility for social security, allowing RD to exploit this natural experiment to estimate the causal impact of postponing the eligibility age.

To evaluate retirement's treatment effects via RD, I estimate a regression model, where the dependent variable is a dummy variable taking the value 1 if the individual is working three months after the control group becomes eligible for social security. This model links labor supply on the extensive margin (employed versus retired) to the running variable (W_i), denoting proximity to the date of birth cutoff. The treatment dummy, D_i, indicates if the individual is born pre or post the date of birth cutoff. The model is:

$$y_i = \beta_0 + \beta_1 D_i + \beta_2 W_i + \beta_3 D_i W_i + \varepsilon_i \tag{1}$$

The coefficient of interest, β_1, captures the average treatment effect on labor supply resulting from delaying the eligibility age at the thresholds listed in Table 1. The model accounts for varying slopes on either side of the date of birth cutoff through β_2 and β_3, while ε_i represents the error term.

I analyze each date of birth cutoff in the reform, focusing on individuals born within six months of the cutoff in administrative data. For example, the control group for the initial date of birth cutoff includes individuals born in the latter half of 1953 (cohort 1953.5), while the treatment group consists of those born in early 1954 (cohort 1954.0). At age 65, the control group qualifies for social security, whereas the treatment group faces a six-month delay. I assess labor force participation at age 65 and 3 months, the midpoint of the differential eligibility period.

Beyond labor supply effects, I also estimate the impact on retirement age. In this specification, I restrict the sample to the age range available for the younger cohort to avoid downward bias from including data from the older cohort at more advanced ages.

When I analyze the survey data, I include individuals born within a two-year range on either side of the date of birth cutoff to increase statistical power. Appendix D.4 presents the primary outcome utilizing merely one cohort on each side of the cutoff. The findings are qualitatively consistent, but the effects are estimated with less precision.

In both the administrative and survey data analyses, I pool data across cutoffs and align observations to improve statistical power.

To validate the RD approach, I compare covariates between control and treatment groups across date of birth cutoffs. The results for the administrative data, detailed in Appendix F, show no difference, reinforcing the robustness of the identification strategy and the reliability of the estimated treatment effects.

5 Regression Discontinuity Results

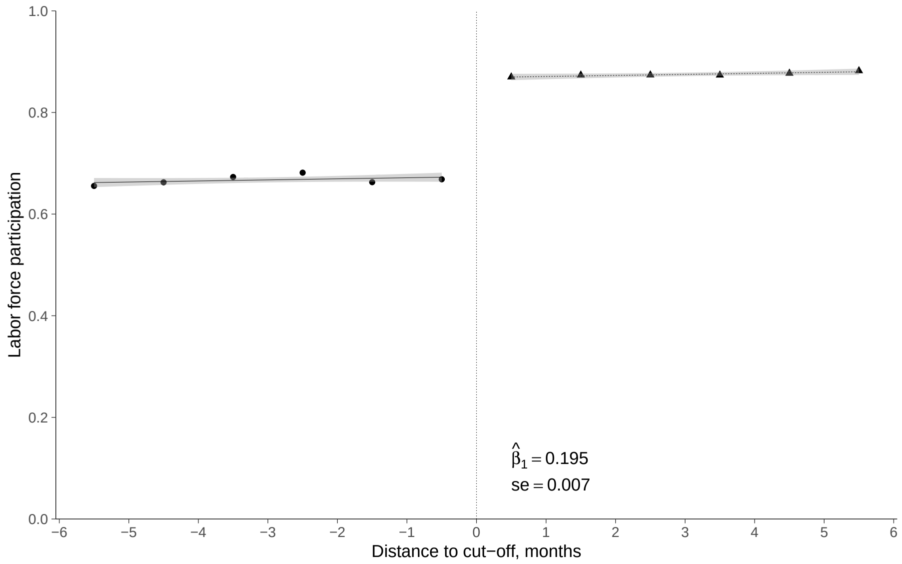
I use a regression discontinuity (RD) design to measure the causal effect of delaying the social security eligibility age of one cohort compared to the previous cohort. This effect is evaluated across both administrative and survey datasets with varying levels of pre-retirement pension wealth.

5.1 Labor Supply Responses from Administrative Data

The following results use pooled data from multiple comparisons (see Table 1) where control and treatment groups are aligned by W_i, the distance to the age cutoff. Detailed RD results for each comparison are shown in Appendix H.

Figure 4 plots the treatment dummy $\hat{\beta}_1$ estimates, illustrating the pooled age cutoff scenario: older cohorts (control) are on the left, and younger cohorts (treatment) on the right. The y-axis shows the proportion not retired 3 months after the control group eligibility age, while the x-axis represents the distance from the birth date to the cutoff date in months. A distinct jump at the cutoff indicates that younger individuals have a 19.5 percentage point higher likelihood of working.

Figure 4: *RD results, pooled cutoffs, administrative data*



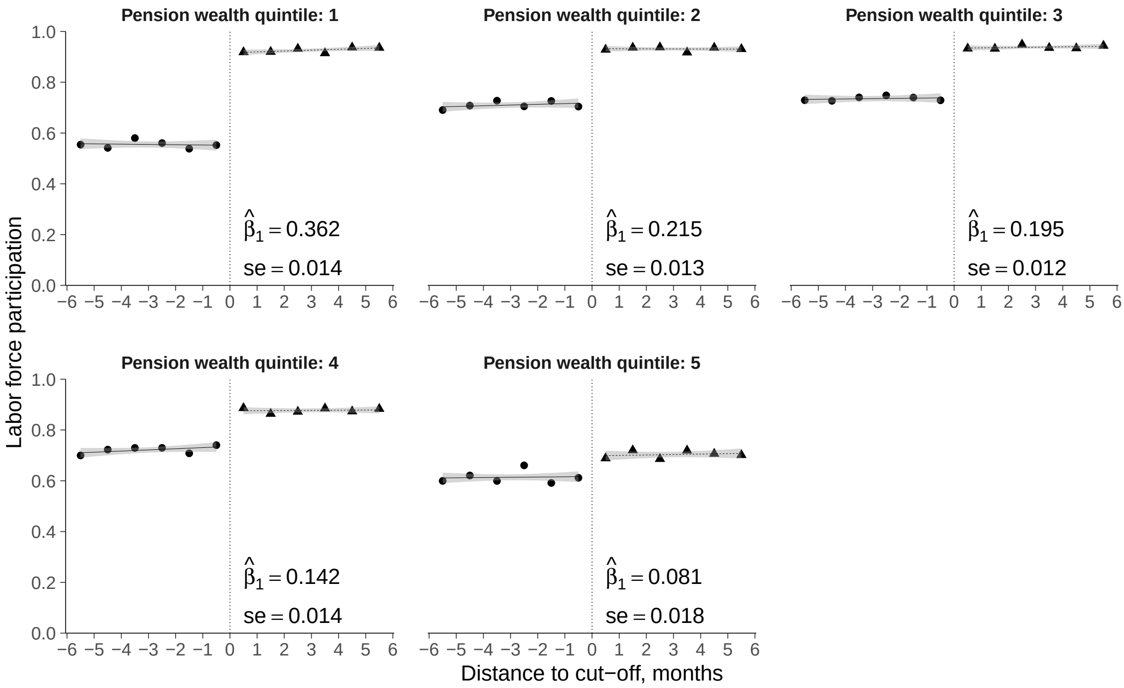
Note: This figure presents regression discontinuity (RD) estimates based on Equation 1 using pooled administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

The observed effect aligns with similar international findings. For instance, previous studies have reported comparable estimates: 13.6 pp in Behaghel and Blau (2012); 9.8 pp in Staubli and Zweimüller (2013); 7.2 pp in Cribb et al. (2016); elasticities around 0.24 in Manoli and Weber (2016); elasticity of 0.22 in Laun (2017); 20.9 pp in Rabate and Rochut (2020); 13.5 pp in Geyer and Welteke (2021); 21.2 pp in Rabaté et al. (2024). Given the reforms objective to prolong working duration, the observed increase in labor force participation is consistent and expected. The stable effects within each cohort indicate no systematic differences among individuals born within six months on either side of the cutoff.

Figure 5 breaks down the results by pension wealth quintile. The low-wealth group shows a substantial response (36.2 percentage points) compared to the high-wealth group (8.1 percentage points). This supports the hypothesis that individuals with significant pension wealth experience less impact from changes to social security eligibility age, as it constitutes a smaller proportion of their retirement income.

Figure 6a summarizes the results from the administrative analysis by plotting estimates of the treatment dummy $\hat{\beta}_1$ (effect sizes from Figure 5) alongside 95% confidence

Figure 5: *RD results, pooled cutoffs, by pension wealth, administrative data*



Note: This figure presents regression discontinuity (RD) estimates based on Equation 1 using pooled administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

intervals by pension wealth. The gradient is negative and monotonic and the estimates are significantly different from each other.

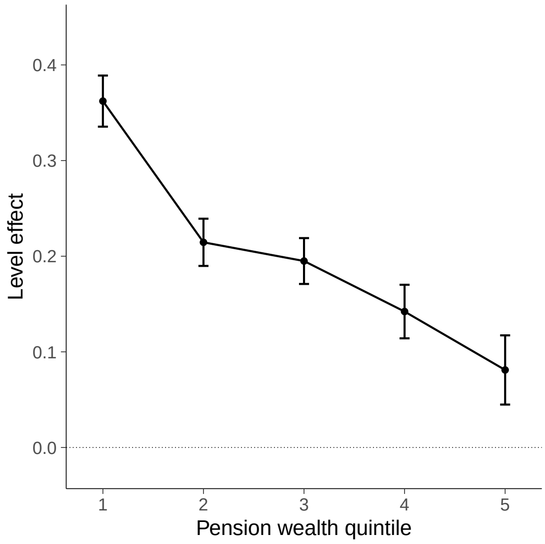
5.2 Retirement Age Responses from Administrative Data

As an alternative outcome measure, I analyze retirement age. One drawback of this approach is that I can only include individuals whom I observe retire at some point in the data. To ensure comparability across cohorts, the post-eligibility period must be consistent. Otherwise, the longer data availability for the control group (older cohort) would artificially increase their observed retirement age. The implication is that in the first comparison (1953.5 vs. 1954.0), post-eligibility period is 3.5 years; in the other two comparisons it is 2.5 years and 1.5 years.

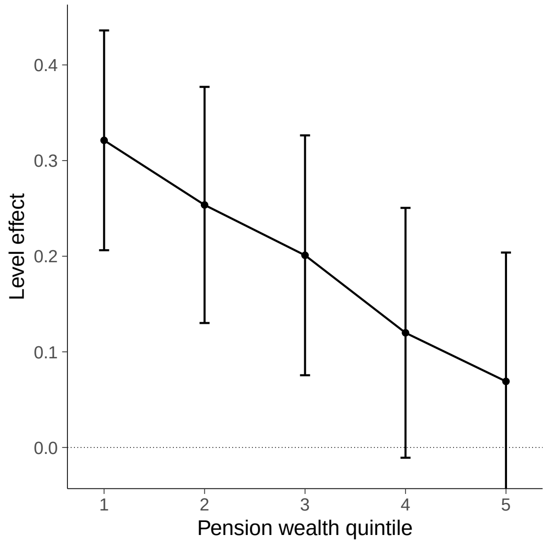
Pooling data across comparisons, I estimate that delaying social security eligibility by half a year increases the retirement age by 0.27 years (or 3.3 months), equivalent to a 55% pass-through rate. When I stratify by pension wealth a clear gradient in pension wealth is observed: those with low pension wealth delay by 0.37 years (4.5 months) and those

Figure 6: *Labor supply response to reform*

(a) *Summary of RD results, administrative data*



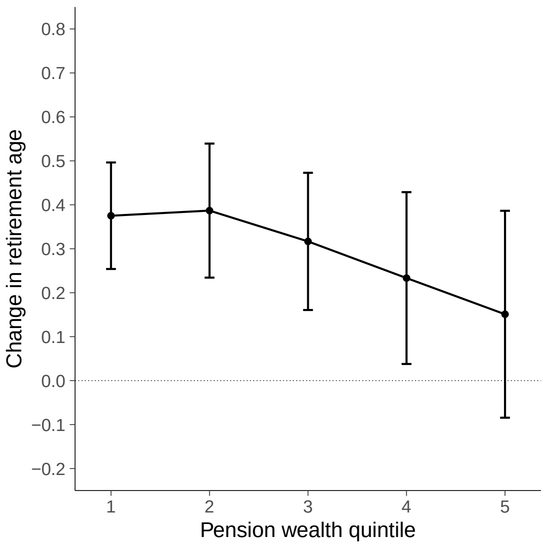
(b) *Summary of RD results, survey data*



Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using administrative data, Panel (a), and survey data, Panel (b). The x-axis indicates pension wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on labor force participation, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals.

with high pension wealth delay by 0.15 years (1.8 months). The results are summarized in Figure 7 and show the same gradient where low pension wealth individuals respond to a larger degree. Appendix I shows detailed RD results for the effect on retirement age.

Figure 7: *Retirement age response to reform*



Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using administrative data. The x-axis indicates pension wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on realized retirement age. Vertical bars represent 95% confidence intervals.

5.3 Survey results

In analyzing social security reforms, previous studies have primarily focused on post-retirement responses. As evidenced above, a delay in eligibility age induces later retirement, especially among low pension wealth individuals. In this section, I extend the analysis to predict future responses, factoring in retirement expectations of the not-yet-eligible.

Following the previous analysis, I pool the three available birth date cutoffs from the survey dataset[12]. Figure 6b presents the treatment dummy $\hat{\beta}_1$ estimates, segmented by pension wealth, and confirms administrative data insights: people expect to respond to the reform and there is a gradient in expected pension wealth. While these findings are less precise due to limited sample sizes, their magnitudes align closely. Individuals with low expected pension wealth respond by 32.1 percentage points and the high expected wealth individuals by 6.9 percentage points. Detailed RD outcomes for each cutoff are presented in Appendix J.

The survey data analysis indicates that the pattern observed in administrative data is likely to persist in forthcoming years. Expected responses to retirement ages close to 70 also show a decrease with pension wealth, suggesting that the distributional effects of the reform is likely to persist well into the future.

6 Fiscal Implications

As a back-of-the-envelope exercise, I calculate the net fiscal effect ϕ on the government budget of delaying the social security eligibility age by half a year. This calculation is motivated by the difficulty of disentangling crowd-out effects, where individuals may substitute into other welfare programs, thereby diminishing the reform's true impact. To address this issue, I compute the average annual net contributions of not being retired as the difference between total taxes paid, τ, and total transfers received, γ, both before and after retirement. The difference in net contributions, denoted by Σ, multiplied by the causal change in retirement age δR induced by the reform, represents the net fiscal effect, ϕ, per affected individual:

$$\phi = \left[\underbrace{(\tau^{\text{pre}} - \gamma^{\text{pre}}) - (\tau^{\text{post}} - \gamma^{\text{post}})}_{\Sigma=\text{Net contribution difference}} \right] \cdot \delta R \tag{2}$$

Specifically, I calculate the average annual taxes τ and transfers γ for individuals in

[12] I consider cohorts around cutoffs 1963, 1967, and 1971 as visualized by the shaded areas in Figure 1.

the main sample both before ($\tau^{\text{pre}} - \gamma^{\text{pre}}$) and after retirement ($\tau^{\text{post}} - \gamma^{\text{post}}$), excluding the year of retirement. I restrict the analysis to ages 58-68 to standardize comparisons across retirement ages. The analysis is performed for the entire sample and also stratified by pension wealth quintile. The results are presented in Table 2 and Figure 8.

Table 2: *Fiscal effect of a half-year delay in eligibility*

Group	*Pre-retirement* tax τ^{pre}	transfer γ^{pre}	*Post-retirement* tax τ^{post}	transfer γ^{post}	*Total effect* Σ	*Response* δR	*Scaled effect* ϕ
All	29k	3.2k	18.3k	13.5k	30.5k	0.27	8.4k
Q1	18.6k	5.5k	8.3k	23.8k	28.7k	0.37	10.6k
Q2	20.1k	3.6k	9.5k	21.5k	28.4k	0.39	11.1k
Q3	24.6k	3k	11.5k	19.1k	29.2k	0.32	9.4k
Q4	31.8k	2.4k	14.9k	16.2k	30.6k	0.23	7.1k
Q5	51.1k	1.7k	21.4k	12k	40k	0.15	6.1k

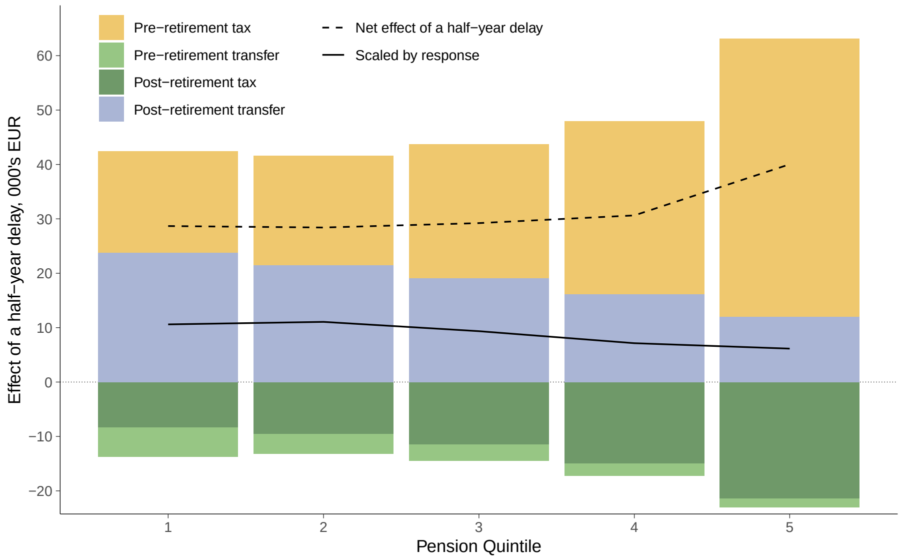
Note: The table summarizes the fiscal effects of retaining a worker in the workforce. The first row shows the components for the entire sample, the rest is stratified by pension wealth, measured at age 59.

For individuals with low pension wealth (the row in Table 2 denoted "Q1"), average annual taxes paid before retirement are $\tau^{\text{pre}} = €18.6$k and transfers received are $\gamma^{\text{pre}} = €5.5$k. After retirement, these change to $\tau^{\text{post}} = €8.3$k and $\gamma^{\text{post}} = €23.8$k, respectively. This implies a total net contribution difference $\Sigma = €28.7$k, interpreted as the value of not being retired. Given an estimated retirement response of $\delta R = 0.37$, the scaled fiscal effect is $\phi = €10.6$k per individual. For individuals in the highest pension wealth group, the corresponding value is only $\phi = €6.1$k, indicating a differential fiscal impact by pension wealth.

Figure 8 shows that the total effect Σ (the dashed line) is relatively constant across pension wealth groups, except for the highest wealth quintile, where it increases by about one third. However, when scaled by the estimated response δR (shown in Figure 7), the pattern reverses. Individuals with low pension wealth exhibit a larger reform-induced fiscal effect ϕ, as depicted by the solid line.

This exercise illustrates that the reform has important distributional consequences. While its stated objective is to improve the government budget by encouraging later retirement, the burden of adjustment is not evenly distributed. Individuals with low pension wealth respond more strongly to the increase in eligibility age and therefore contribute disproportionately more to the fiscal gains. This raises broader equity concerns, as the reform effectively relies more heavily on those with fewer resources to achieve its budgetary goals.

Figure 8: *Fiscal effect of a half-year delay in eligibility*



Note: Each of the five bars summarize the effects of retaining a worker in the workforce by pension wealth, measured at age 59. The yellow bars illustrate the average annual taxes paid, the light green the average transfers received pre-retirement. Likewise, the dark green and light blue bars the post-retirement counterpart. The dashed line shows the net effect of the four components and the solid line is the net effect scaled by the response induced by a half-year delay in social security eligibility age. The numbers used in the figure can be found in Table 2.

7 Conclusion

In this study, I examine the impact of a Danish reform that raised the age threshold for social security eligibility on retirement decisions. Similar reforms are increasingly adopted across various countries to ensure the long-term fiscal sustainability of pension systems. Using a regression discontinuity design at specific age thresholds, I provide causal evidence that postponing the eligibility age leads individuals to delay retirement.

The analysis shows that individuals with low or no pension wealth tend to retire later compared to those with higher levels of pension wealth. This result aligns with conventional economic theories, which posit that greater pension wealth, holding other factors constant, reduces the incentive to extend one's working life.

Furthermore, I find that the estimated effects of the reform on cohorts already affected are comparable to the expected effects among younger cohorts who have not yet reached eligibility age. In a novel survey, participants report their projected retirement age under various hypothetical scenarios. The results indicate that future changes in eligibility age are likely to elicit similar responses, with individuals with lower pension wealth displaying stronger labor supply responses.

Lastly, I evaluate the fiscal implications of postponing social security eligibility across different pension wealth groups. The findings suggest that individuals with low pension wealth make the most significant fiscal contributions, even though the effect of them staying in the workforce is smaller as they exhibit stronger behavioral responses to the reform.

References

Artmann, E., N. Fuchs-Schündeln, and G. Giupponi (2023): "Forward-looking labor supply responses to changes in pension wealth: Evidence from germany," .

Becker, S., H. Buslei, J. Geyer, and P. Haan (2022): "The effect of pension wealth on employment," .

Behaghel, L. and D. M. Blau (2012): "Framing Social Security Reform: Behavioral Responses to Changes in the Full Retirement Age," American Economic Journal: Economic Policy, 4, 41–67.

Beshears, J., J. J. Choi, D. Laibson, and B. C. Madrian (2014): "Who uses the Roth 401 (k), and how do they use it?" Tech. rep., National Bureau of Economic Research.

Börsch-Supan, A. H. and C. Coile (2018): "Social Security Programs and Retirement around the World: Reforms and Retirement Incentives–Introduction and Summary," Tech. rep., National Bureau of Economic Research.

——— (2023): "The Effects of Reforms on Retirement Behavior: Introduction and Summary," Tech. rep., National Bureau of Economic Research.

Brown, J. R., C. C. Coile, and S. J. Weisbenner (2010): "The effect of inheritance receipt on retirement," The Review of Economics and Statistics, 92, 425–434.

Caplin, A., V. Gregory, E. Lee, S. Leth-Petersen, and J. Sæverud (2023): "Subjective Earnings Risk," Tech. rep., National Bureau of Economic Research.

Caplin, A., E. Lee, S. Leth-Petersen, and J. Sæverud (2022): "Communicating Social Security Reform," Tech. rep., National Bureau of Economic Research.

Chetty, R., J. N. Friedman, S. Leth-Petersen, T. H. Nielsen, and T. Olsen (2014): "Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark," The Quarterly Journal of Economics, 129, 1141–1219.

Choi, J. J., D. Laibson, B. C. Madrian, and A. Metrick (2002): "Defined contribution pensions: Plan rules, participant choices, and the path of least resistance," Tax Policy and the Economy, 16, 67–113.

Clark, R. L. and O. S. Mitchell (2002): "Reinventing retirement: New pathways, new arrangements, new meanings," The American Economic Review, 92, 247–252.

Cribb, J., C. Emmerson, and G. Tetlow (2016): "Signals matter? Large retirement responses to limited financial incentives," Labour Economics, 42, 203–212.

Delavande, A. and S. Rohwedder (2008): "Eliciting Subjective Probabilities in Internet Surveys," Public Opinion Quarterly, 72, 866–891.

Diamond, P. A. (2009): "Adverse selection, retirement, and annuity markets," Econometrica, 77, 857–887.

Etgeton, S., B. Fischer, and H. Ye (2023): "The effect of increasing retirement age on households savings and consumption expenditure," Journal of Public Economics, 221, 104845.

García-Miralles, E. and J. M. Leganza (2024): "Public pensions and private savings," American Economic Journal: Economic Policy, 16, 366–405.

Geyer, J. and C. Welteke (2021): "Closing Routes to Retirement for Women How Do They Respond?" Journal of Human Resources, 56, 311–341.

Gruber, J. and D. A. Wise (1999): "Social security and retirement around the world," The Journal of Political Economy, 107, S72–S91.

Lalive, R., A. Magesan, and S. Staubli (2017): "Raising the full retirement age: Defaults vs incentives," NBER Working Paper orrc17–12.

——— (2023): "How Social Security Reform Affects Retirement and Pension Claiming," American Economic Journal: Economic Policy, 15, 115–50.

Laun, L. (2017): "The effect of age-targeted tax credits on labor force participation of older workers," Journal of Public Economics, 152, 102–118.

Lee, R. D. and A. Mason (2011): Population aging and the generational economy: A global perspective, Edward Elgar Publishing.

Manoli, D. S. and A. Weber (2016): "The Effects of the Early Retirement Age on Retirement Decisions," Working Paper 22561, National Bureau of Economic Research.

Mastrobuoni, G. (2009): "Labor supply effects of the recent social security benefit cuts: Empirical estimates using cohort discontinuities," Journal of Public Economics, 93, 1224–1233.

OECD (2021): Pensions at a Glance 2021: OECD and G20 Indicators, Organisation for Economic Co-operation and Development OECD.

Poterba, J. M., S. F. Venti, and D. A. Wise (2011): "The composition and drawdown of wealth in retirement," Journal of Economic Perspectives, 25, 95–118.

Rabaté, S., E. Jongen, and T. Atav (2024): "Increasing the Retirement Age: Policy Effects and Underlying Mechanisms," American Economic Journal: Economic Policy.

Rabate, S. and J. Rochut (2020): "Employment and substitution effects of raising the statutory retirement age in France," Journal of Pension Economics & Finance, 19, 293–308.

Saez, E. and G. Zucman (2016): "Wealth inequality in the United States since 1913: Evidence from capitalized income tax data," The Quarterly Journal of Economics, 131, 519–578.

Seibold, A. (2021): "Reference Points for Retirement Behavior: Evidence from German Pension Discontinuities," American Economic Review, 111, 1126–65.

Skinner, J. (2007): "Are you sure you're saving enough for retirement?" The Journal of Economic Perspectives, 21, 59–80.

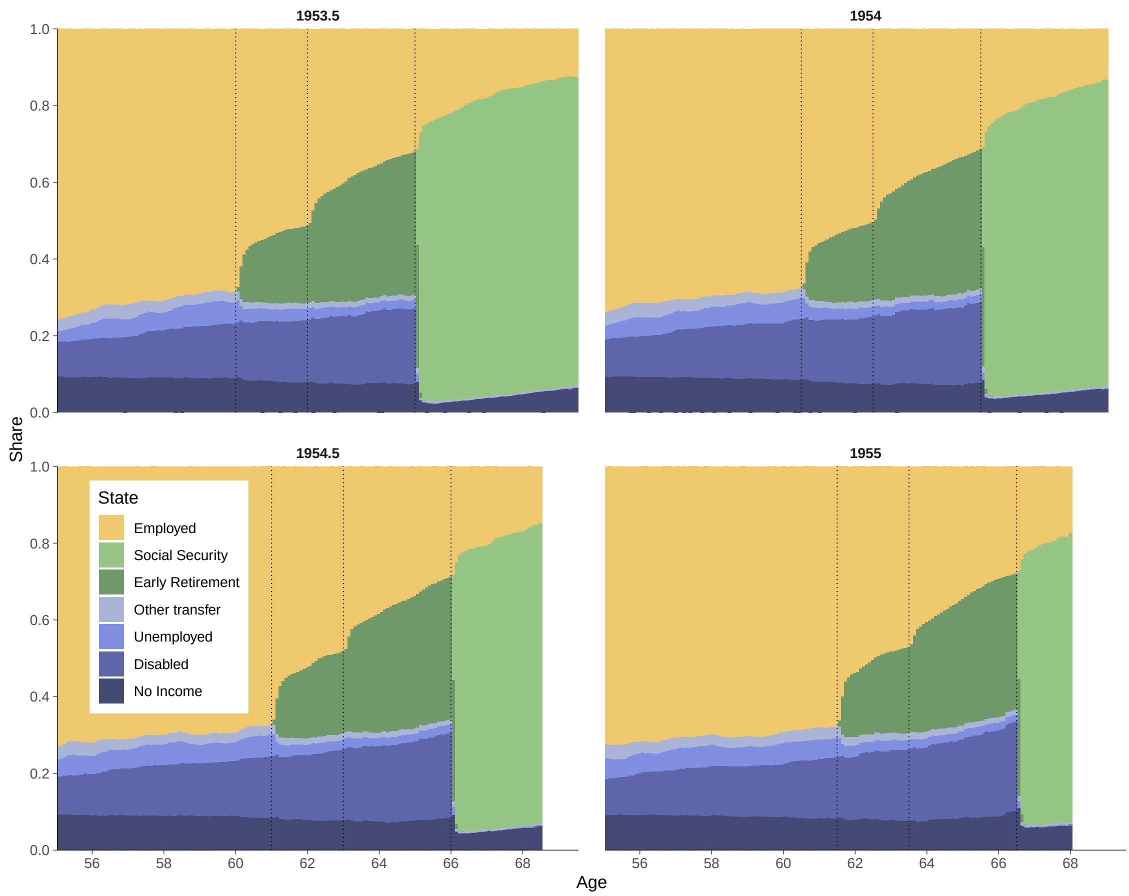
Staubli, S. and J. Zweimüller (2013): "Does raising the early retirement age increase employment of older workers?" Journal of Public Economics, 108, 17–32.

Appendix A Administrative Data Sample Selection

I focus on "regular" employees nearing retirement, excluding non-employees at age 59, predominant self-employees (defined herein as earning a larger portion of income from self-employment as opposed to wages) at age 59, early retirees, and those with prior disability benefits. The sample is limited to native Danes due to the immigrant population's small size and heterogeneity.

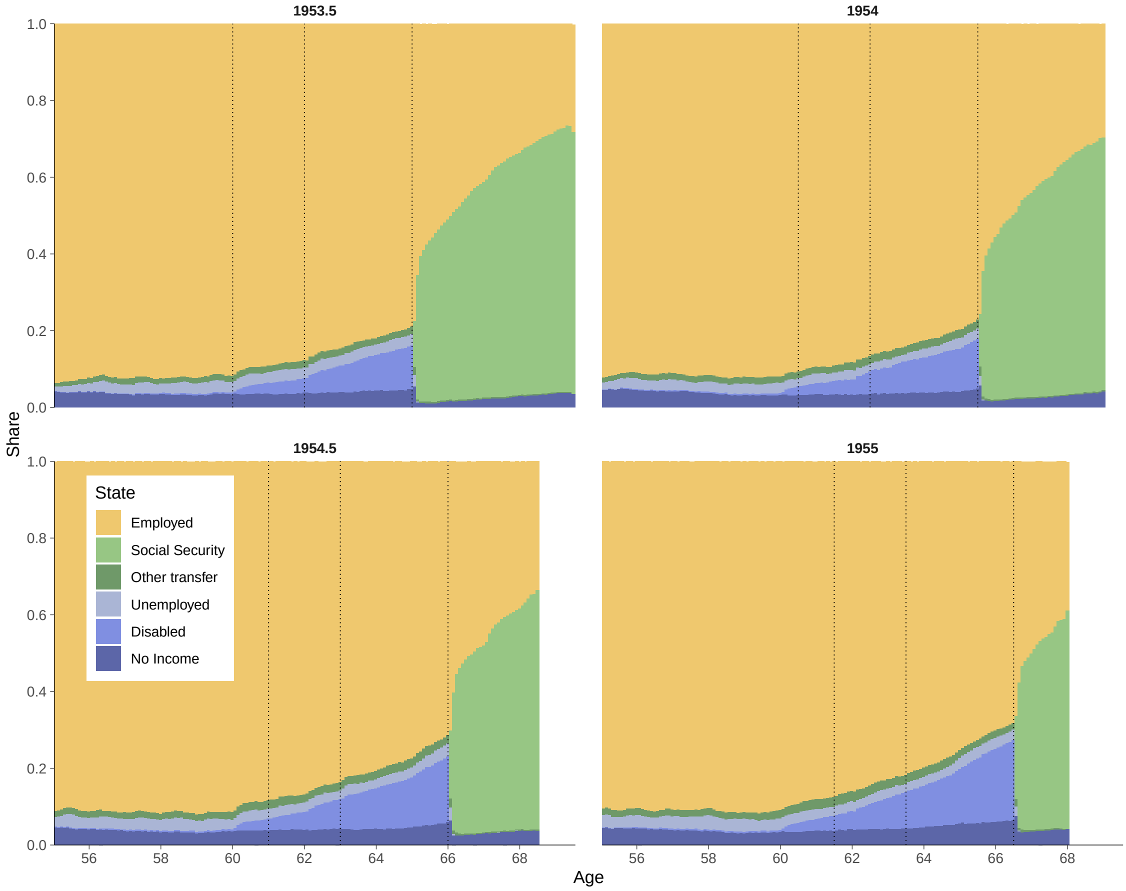
Figure 9 shows, by cohort, how the shares of different labor market statuses evolve over time. Increases in both early retirement and social security are governed to a large degree by the statutory eligibility ages indicated by dashed lines. Figure 10 shows the equivalent but only for the sample of "regular" employees. Table 3 shows how, for the four half-year cohorts in question (1953.5, 1954.0, 1954.5, 1955.0) this reduces the sample size from 167,000 to 58,000.

Figure 9: *Shares of the population*



Note: Each panel in the figure depicts how a cohort stratified by labor market status evolves over time. Each individual is classified in a given month by their main source of income.

Figure 10: *Shares of the sample*



Note: Each panel in the figure depicts how a cohort stratified by labor market status evolves over time. Each individual is classified in a given month by their main source of income.

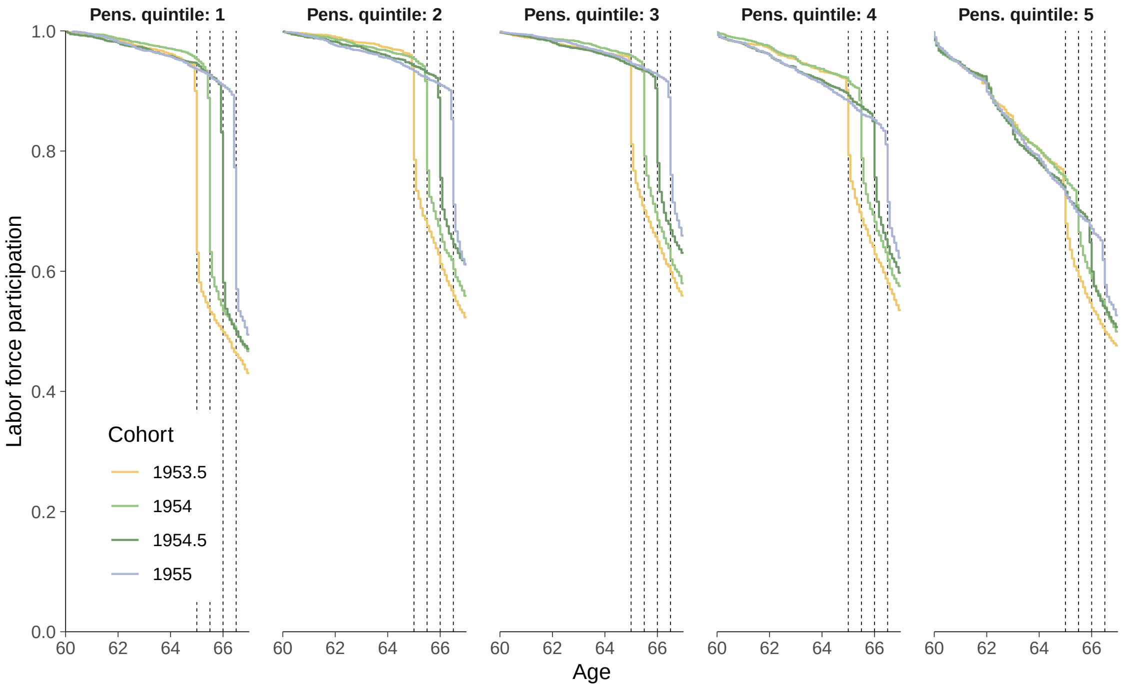
Table 3: *Sample size*

Sample criteria	Number of observations
Population	166,753
Native Danes	154,408
Working age 59	123,347
Not disabled	121,125
Not self-employed	117,907
Not on early retirement	57,303

Appendix B Labor supply patterns

Figure 11 shows for each pension wealth quintile the full labor supply patterns by cohort since age 60. Figure 2 in the main text shows the same, but only from age 64.5.

Figure 11: *Labor supply patterns, by pension wealth and cohort, administrative data*

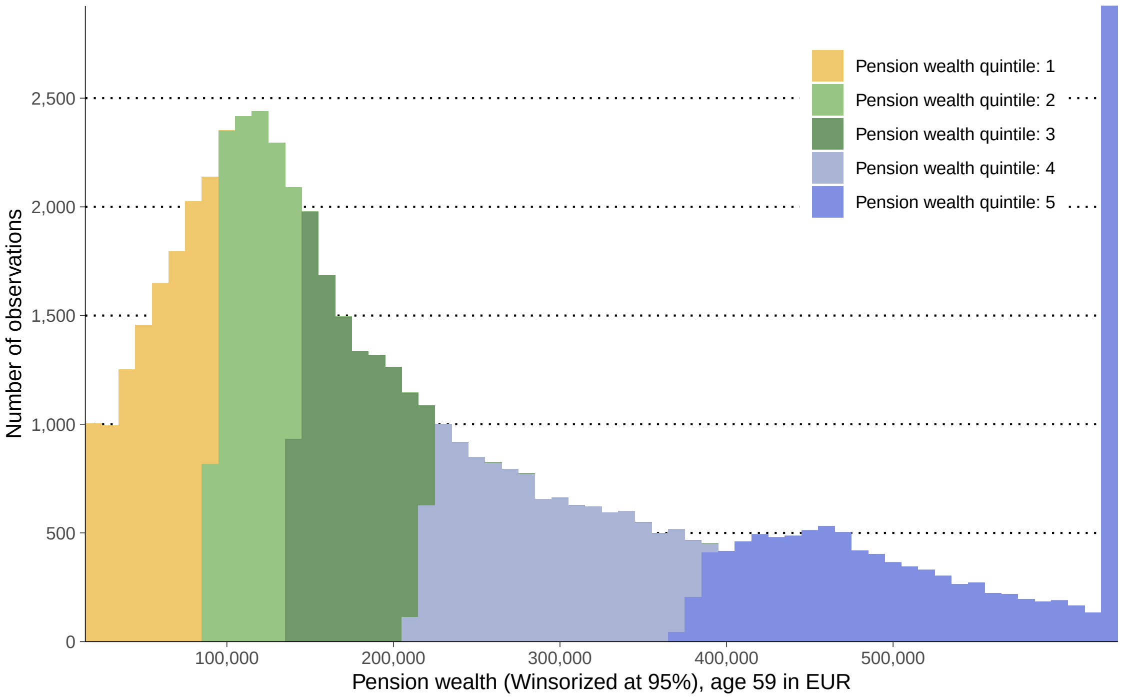


Note: The five panels show transitions into retirement for individuals in the administrative data, grouped by pension wealth quintile. Within each panel, color denotes half-year cohorts. Vertical dashed lines indicate statutory eligibility ages.

Appendix C Pension Wealth

I use accumulated pension wealth to stratify individuals into quintiles by cohort. In the main analysis, in order to avoid endogeneity, I measure the stock of pension wealth one year prior to when individuals can start to withdraw from their pension accounts (which happens at social security eligibility age minus five years). The wealth is measured at age 59 corresponding to end of year 2012 for cohort 1953.5. In the Danish administrative data the stock of pension wealth is readily available at the annual level, but only since 2014, meaning that for the years prior to 2014 I use the annual flows in and out of pension accounts to impute pension wealth. Figure 12 reports the distribution for cohorts 1953.5, 1954.0, 1954.5, and 1955.0.

Figure 12: *Distribution of pension wealth, age 59*



Note: Pension wealth by pension wealth quintile at age 59 in EUR. Cohorts 1953.5, 1954.0, 1954.5, and 1955.0. Winsorized at 1% and 95%.

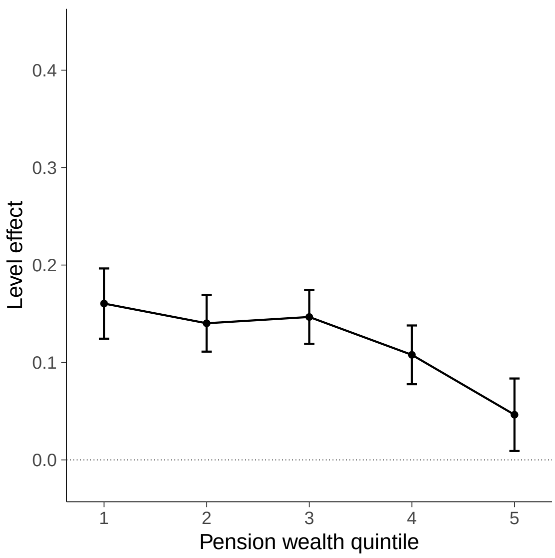
Appendix D Robustness Analysis

In this appendix I present results akin to the two main figures in the main text: Figure 6a for administrative data and Figure 6b for survey data.

Appendix D.1 Alternative Outcome Variable, Administrative data

In the main analysis of administrative data I define the retired state as individuals who are both no longer employed (for the remainder of the dataset) and are receiving pension payouts. In Figure 13 I have estimated the main results using non-employment 3 months after reaching the cutoff (not conditioned on future non-employment and not conditioned on receiving pension payouts) as the main outcome instead. The effect is similar to the main results, except for the low pension wealth group. This group has a lower labor market participation rate if only contingent on non-employment, as they shift in and out of the labor market pre-retirement. Compared to the main specification (the line to the left in Figure 5, Panel 1) where 77% of the low wealth individuals are not retired yet, the same number is 70% for the current specification. At the same time this group has less access to withdrawal of occupational pension before the statutory age, the amount of retirees is constant across the main specification and the current one.

Figure 13: *Summary of RD results, administrative data, non-employment*

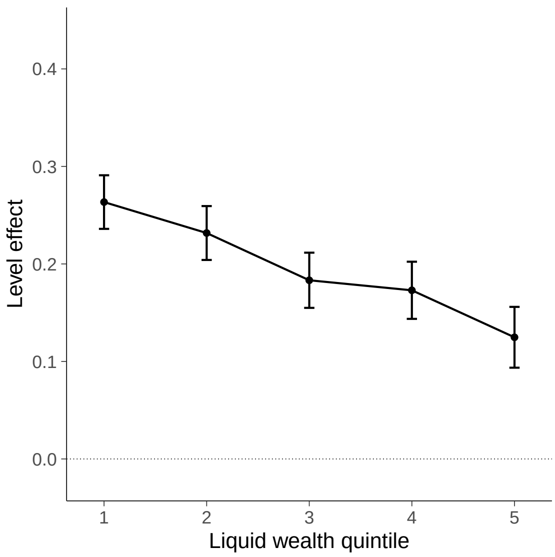


Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using administrative data. The x-axis indicates pension wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on employment status, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals.

Appendix D.2 Alternative Wealth Definitions, Administrative data

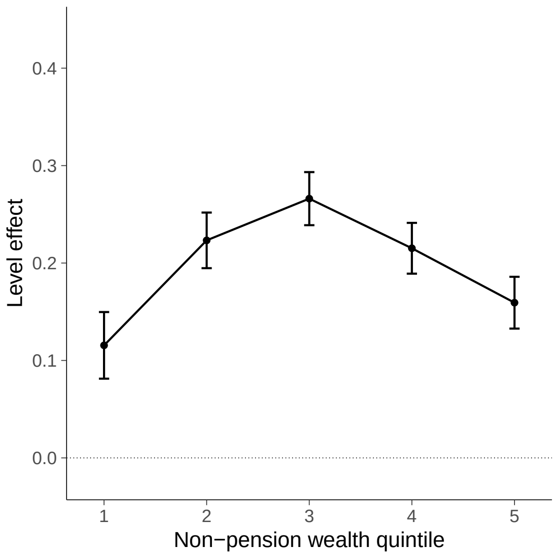
Most pension accounts are paid out as either fixed-term or indefinite annuities and as such pension wealth can be considered an income stream in retirement rather than an asset that can be accessed at will. This sets pension wealth apart from other types of wealth, e.g. liquid wealth and real estate. Figures 14 and 15 present the main results but using either liquid wealth or total wealth (including real estate) to divide individuals into wealth quintiles and neither has nearly the same clear gradient on the retirement decision as pension wealth. This confirms that pension wealth is the most appropriate wealth measure to stratify on. As for pension wealth, quintiles for liquid and total wealth is determined at age 59.

Figure 14: *Summary of RD results, administrative data, stratified by liquid wealth*



Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using administrative data. The x-axis indicates liquid wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on labor force participation, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals.

Figure 15: *Summary of RD results, administrative data, stratified by total wealth*

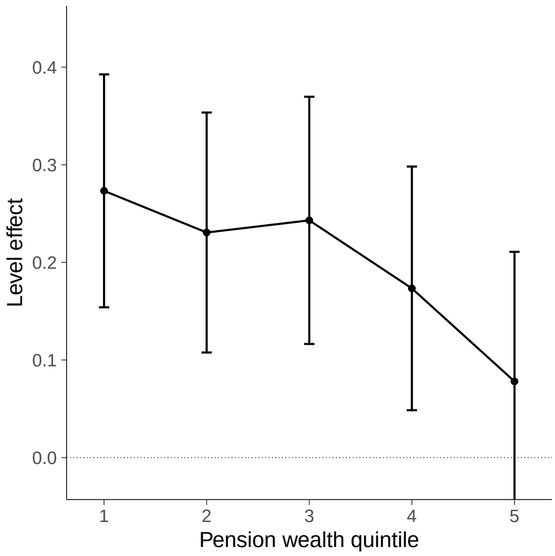


Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using administrative data. The x-axis indicates total wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on labor force participation, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals.

Appendix D.3 Alternative Pension Measure, Survey data

In the main analysis, survey respondents are stratified by their expected income stream from pension savings in retirement. If I instead stratify by actual pension wealth at the time of the survey, the result is less clear, as shown in Figure 16.

Figure 16: *Summary of RD results, survey data, stratified by current pension wealth*

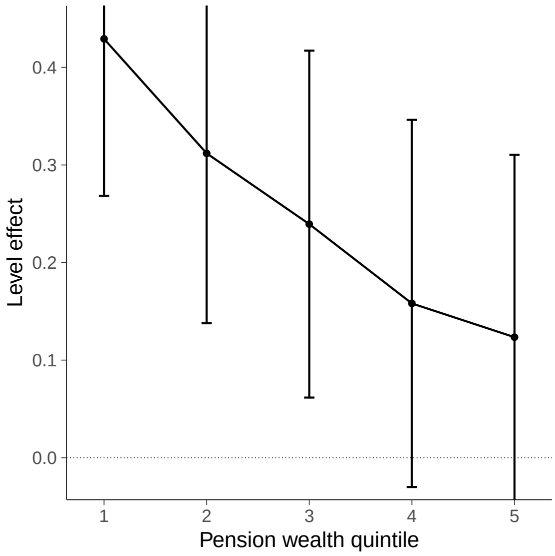


Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using survey data. The x-axis indicates pension wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on labor force participation, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals. In this figure wealth quintile as determined using current pension wealth measured in 2020.

Appendix D.4 Smaller Bandwidth, Survey data

In the main analysis, I use two full-year cohorts on either side of the cutoff in the survey analysis. If I use only one cohort instead of two on either side the result is more noisy, but consistently downward sloping in pension wealth, as shown in Figure 17.

Figure 17: *Summary of RD results, survey data, only one cohort instead of two on either side*



Note: This figure summarizes regression discontinuity (RD) estimates from Equation 1 using survey data using only one cohort on either side of the cutoff. The x-axis indicates pension wealth quintiles, and the y-axis plots the estimated treatment effects ($\hat{\beta}_1$) on labor force participation, measured three months after the eligibility threshold. Vertical bars represent 95% confidence intervals.

Appendix E Copenhagen Life Panel Survey

I use answers to the Copenhagen Life Panel survey[13] fielded in January 2021.

A random subset of 100,000 individuals from the population aged 20-70 was drawn from the administrative registers and invited to participate using an official email account, called e-boks, which all Danes are equipped with. As in the analysis of the administrative data above I contrast cohorts born on either side of an age cutoff, but here I use entire cohorts and the individuals I consider in the analysis are from the following cohorts:

- Control born in 1962, eligible at age 67 vs. Treated born in 1963, eligible at age 68
- Control born in 1966, eligible at age 68 vs. Treated born in 1967, eligible at age 69
- Control born in 1970, eligible at age 69 vs. Treated born in 1971, eligible at age 70

In total 23,802 people belonging to the selected cohorts were invited, and out of these, 5,006 answered the survey.

I compare the retirement decision of the cohorts pairwise in the regression discontinuity analysis, as they are affected differentially by the reform. I refer to Table 1 for sample sizes used at each cutoff and the shaded areas in Figure 1 show exactly which cohorts are contrasted.

The Copenhagen Life Panel survey contains numerous questions about expected future outcomes both at the individual and aggregate level[14]. In this paper I use four questions pertaining to retirement that allows me to conduct an analysis in expectations that match the analysis done on administrative data. The four questions are as follows:

1. $\mathbb{E}[SS]$: *At what age do you anticipate first being eligible for social security?*
2. $\mathbb{E}[R_{65}]$: *Suppose that you first become eligible for social security at the age of 65. At what age do you expect to retire?*
3. $\mathbb{E}[R_{TA}]$: *Suppose that you first become eligible for social security at the age of [Table age]. At what age do you expect to retire?*
4. $\mathbb{E}[Y]$: *Suppose you were to retire at the age of [Table age] and suppose your future pension savings evolve as you expect. If you were to convert all your future pension*

[13] Two other papers use data from the Copenhagen Life Panel: Caplin et al. (2023) on how income expectations can inform how we think about subjective earnings risk, and Caplin et al. (2022) on how young people are unaware of their statutory eligibility age.

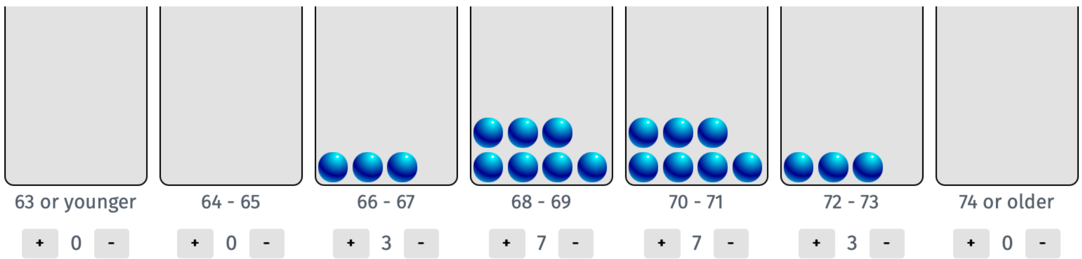
[14] For validation exercises see Caplin et al. (2022) that use the same survey wave.

savings into a steady income stream (life-long pension) at the age of [Table age]. How much annual income do you believe it would provide?

In the third question, eliciting $\mathbb{E}[R_{TA}]$, I show the respondents the eligibility age from Table 1 based on their cohort.

In each of the questions I elicit a probability distribution instead of just a point estimate using the "balls-in-bins" method proposed by Delavande and Rohwedder (2008). For the first three questions, seven bins are pre-specified as shown in Figure 18 wherein the respondents place 20 balls, each representing a 5% probability. In the last question, the respondents first specify a minimum and a maximum limit for their annual income followed by seven equally spaced bins representing the entire interval between stated minimum and maximum. They are then prompted to place the 20 balls in the bins. The respondents have been thoroughly instructed and have completed several iterations of placing balls in bins before encountering the retirement questions.

Figure 18: *"Balls-in-bins"*



Note: The graphical user interface where the respondents place 20 balls in seven bins to reflect their subjective beliefs.

In correspondence with the analysis of the administrative data I use the binary retirement status measured right after eligibility for the control cohort as outcome variable. This means that for the first pair of cohorts (where cohorts 1962 and 1963 are compared) the outcome of interest is whether or not an individual expects to be retired at age 67 and 3 months, precisely when the older cohort is eligible and the younger cohort is not yet eligible.

Appendix E.1 Survey Data Balance Table

Table 4: *Balance table, survey data*

	Respondent	Non-respondent	Difference	p-value
N	5001	18780		
Female	0.5	0.494	0.006	0.427
	(0.5)	(0.5)	(0.008)	
Age	54.766	54.517	0.25	0
	(3.411)	(3.447)	(0.054)	
College	0.51	0.331	0.179	0
	(0.5)	(0.471)	(0.008)	
Employed	0.922	0.789	0.133	0
	(0.269)	(0.408)	(0.005)	
Earnings, EUR	76,681	58,707	17,974	0
	(54,793)	(58,630)	(885)	
Total net wealth, EUR	384,412	312,699	71,714	0
	(427,740)	(562,590)	(7,310)	
Pension wealth, EUR	356,251	270,477	85,774	0
	(291,782)	(273,269)	(4,583)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1961-1972. Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree, *Employed* is a dummy for earning more than €7,500 in 2020. All values are in EUR and recorded end of year 2020.

Appendix E.2 Expected Retirement

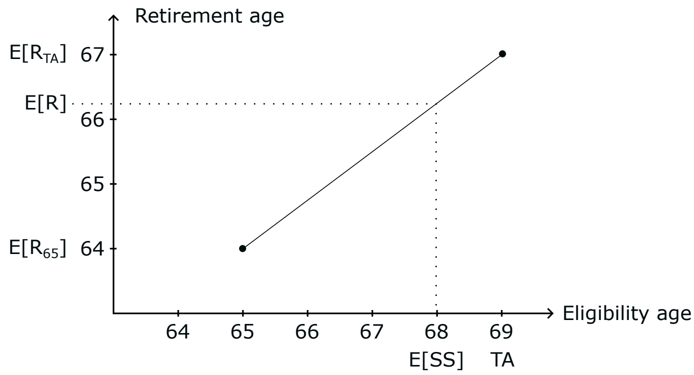
I derive the expected retirement age, $\mathbb{E}[R]$, using the expected social security eligibility age to interpolate between two hypothetical retirement scenarios. The metric of interest, the expected retirement age, $\mathbb{E}[R]$, is not directly elicited in the survey. In line with Caplin et al. (2022) I compute $\mathbb{E}[R]$ by using use expected social security eligibility age to interpolate between the expected retirement age in each of two hypothetical scenarios: being eligible at age 65 (which was the universal age for decades prior to the reform) or at the "Table Age" (TA) specified in Table 1 (which in practice constitutes an upper bound).

The interpolation of expected retirement is calculated as:

$$\mathbb{E}[R] = \frac{\mathbb{E}[R_{65}](TA - \mathbb{E}[SS]) + \mathbb{E}[R_{TA}](\mathbb{E}[SS] - 65)}{TA - 65}$$

and can be visualized with an example where

$\mathbb{E}[R_{65}]$	64
$\mathbb{E}[R_{TA}]$	69
$\mathbb{E}[SS]$	68
TA	69
E[R]	66.25



Appendix F RD Balancing Checks, administrative data

The tables below show for each of the three birth date cut-offs used in the administrative analysis that there are no significant differences across the age cut-offs (holding a college degree is significant statistically, but very small). This confirms that the standard RD assumption holds.

Table 5: *Balance table, admin data, control cohort 1953.5, treatment 1954.0*

	Control	Treatment	Difference	p-value
N	10979	11931		
Female	0.409	0.408	0	0.957
	(0.492)	(0.492)	(0.007)	
College	0.625	0.614	-0.012	0.067
	(0.484)	(0.487)	(0.006)	
Earnings, EUR	67,501	66,811	-690	0.296
	(51,468)	(48,203)	(661)	
Total net wealth, EUR	393,480	388,717	-4,763	0.492
	(460,696)	(585,488)	(6,933)	
Pension wealth, EUR	401,438	396,554	-4,884	0.324
	(376,912)	(371,739)	(4,952)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1953.5 (control) and 1954.0 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree, *Earnings* is earnings at age 59 in DKK, *Total wealth* includes real estate at age 59, *Pension savings* is total occupational and supplementary pension savings at age 59.

Table 6: *Balance table, admin data, control cohort 1954.0, treatment 1954.5*

	Control	Treatment	Difference	p-value
N	11931	10930		
Female	0.408	0.409	0.001	0.923
	(0.492)	(0.492)	(0.007)	
College	0.614	0.627	0.014	0.035
	(0.487)	(0.484)	(0.006)	
Earnings, EUR	66,811	66,603	-208	0.778
	(48,203)	(61,479)	(736)	
Total net wealth, EUR	388,717	384,748	-3,968	0.576
	(585,488)	(484,783)	(7,088)	
Pension wealth, EUR	396,554	397,277	723	0.889
	(371,739)	(408,030)	(5,179)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1954.0 (control) and 1954.5 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree, *Earnings* is earnings at age 59 in DKK, *Total wealth* includes real estate at age 59, *Pension savings* is total occupational and supplementary pension savings at age 59.

Table 7: *Balance table, admin data, control cohort 1954.5, treatment 1955.0*

	Control	Treatment	Difference	p-value
N	10930	12099		
Female	0.409	0.399	-0.011	0.105
	(0.492)	(0.49)	(0.006)	
College	0.627	0.614	-0.013	0.035
	(0.484)	(0.487)	(0.006)	
Earnings, EUR	66,603	67,956	1,353	0.099
	(61,479)	(62,552)	(819)	
Total net wealth, EUR	384,748	382,744	-2,005	0.767
	(484,783)	(543,153)	(6,774)	
Pension wealth, EUR	397,277	394,756	-2,521	0.656
	(408,030)	(449,364)	(5,650)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1954.5 (control) and 1955.0 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree, *Earnings* is earnings at age 59 in DKK, *Total wealth* includes real estate at age 59, *Pension savings* is total occupational and supplementary pension savings at age 59.

Appendix G RD Balancing Checks, survey data

The tables below show for each of the three birth date cut-offs used in the survey analysis that there are no significant differences across the age cut-offs. This confirms that the standard RD assumption holds.

Table 8: *Balance table, survey data, control cohorts 1961-1962, treatment 1963-1964*

	Control	Treatment	Difference	p-value
N	898	893		
Female	0.507	0.499	-0.007	0.759
	(0.5)	(0.5)	(0.024)	
College	0.529	0.543	0.014	0.548
	(0.499)	(0.498)	(0.024)	
Earnings, EUR	71,962	74,028	2,066	0.389
	(47,267)	(53,970)	(2,398)	
Total net wealth, EUR	479,456	446,907	-32,549	0.194
	(403,167)	(631,591)	(25,054)	
Pension wealth, EUR	451,110	415,567	-35,543	0.049
	(311,211)	(440,256)	(18,025)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1953.5 (control) and 1954.0 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree. All values are in EUR and recorded end of year 2020.

Table 9: *Balance table, survey data, control cohorts 1965-1966, treatment 1967-1968*

	Control	Treatment	Difference	p-value
N	887	821		
Female	0.487	0.499	0.012	0.61
	(0.5)	(0.5)	(0.024)	
College	0.507	0.442	-0.065	0.007
	(0.5)	(0.497)	(0.024)	
Earnings, EUR	76,856	79,623	2,767	0.37
	(49,285)	(74,564)	(3,084)	
Total net wealth, EUR	381,594	345,736	-35,857	0.05
	(382,928)	(371,600)	(18,262)	
Pension wealth, EUR	345,910	328,405	-17,505	0.128
	(244,285)	(230,559)	(11,490)	

Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1954.0 (control) and 1954.5 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree. All values are in EUR and recorded end of year 2020.

Table 10: *Balance table, survey data, control cohorts 1969-1070, treatment 1971-1972*

	Control	Treatment	Difference	p-value
N	773	729		
Female	0.519	0.491	-0.028	0.284
	(0.5)	(0.5)	(0.026)	
College	0.472	0.428	-0.044	0.085
	(0.5)	(0.495)	(0.026)	
Earnings, EUR	78,943	79,818	875	0.731
	(49,149)	(49,566)	(2,549)	
Total net wealth, EUR	321,793	304,166	-17,627	0.279
	(320,662)	(310,286)	(16,282)	
Pension wealth, EUR	302,230	267,966	-34,264	0
	(200,557)	(159,453)	(9,323)	

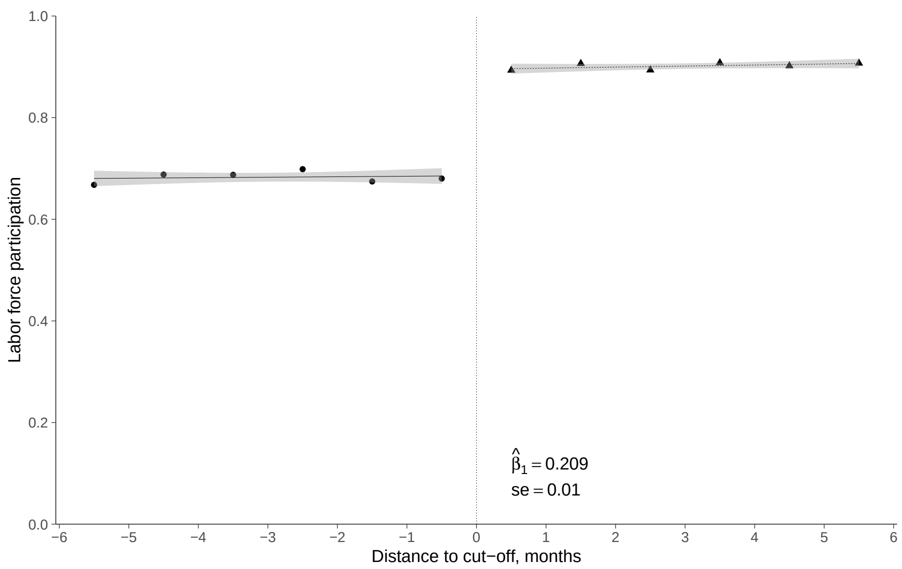
Note: The table reports the differences between respondents and non-respondents in the survey. Cohorts 1969-1970 (control) and 1971-1972 (treatment). Numbers in parentheses are standard deviations in columns 1 and 2, and standard errors from a two-sample t-test in column 3. Column 4 reports the p-value. *College* is a dummy for having obtained at least a Bachelor degree. All values are in EUR and recorded end of year 2020.

Appendix H Additional Admin Results

Figure 19 illustrates the results for the inaugural age cutoff: cohort 1953.5 on the left, and cohort 1954.0 on the right. The y-axis depicts the proportion not retired at age 65 and 3 months. The running variable signifies the interval in months from the birth date to January 1st, 1954. A distinct jump at the cutoff reveals that individuals from cohort 1954.0 exhibit a 21.1 percentage point higher likelihood to retire; this tendency remains uninfluenced by the distance to the cutoff. Figure 20 shows the RD estimates by pension wealth.

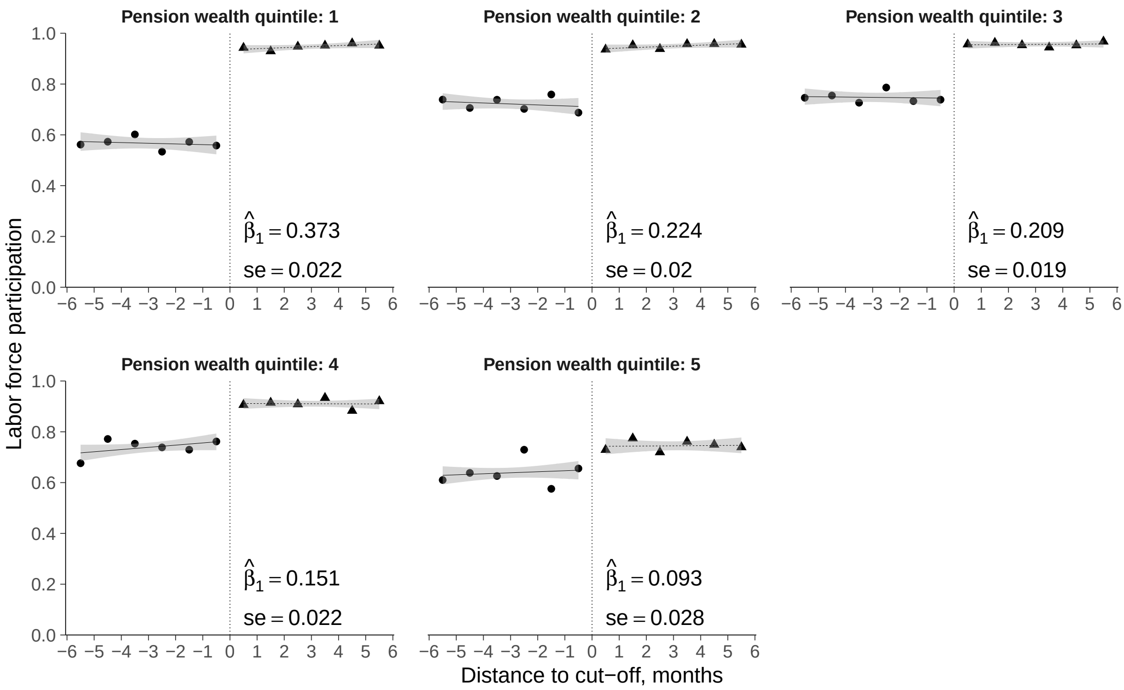
The analysis is repeated for the 2nd and 3rd birth date cutoffs centered on July 1st, 1954 and January 1st, 1955 respectively. Figures 21 - 24 - show the RD estimates both in the aggregate and by pension wealth.

Figure 19: *RD results, January 1st, 1954 cutoff, administrative data*



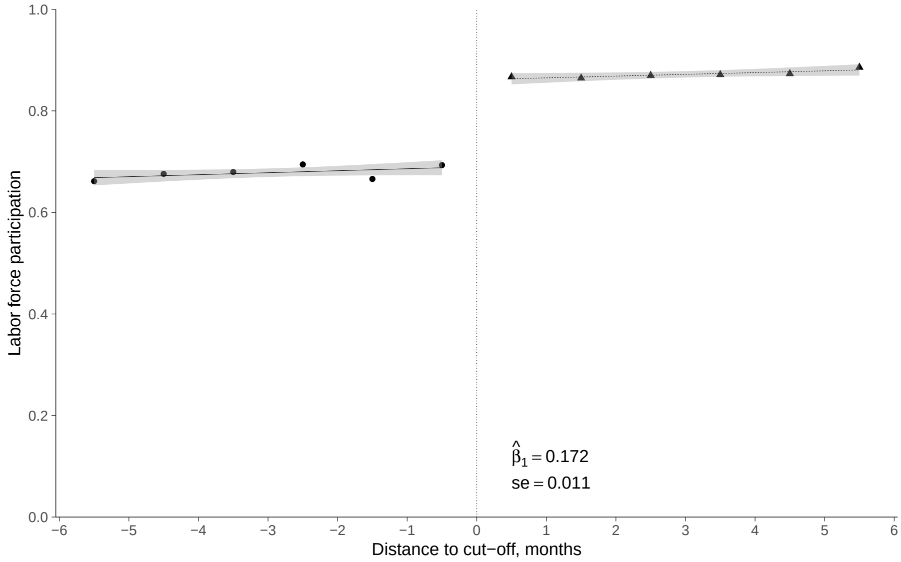
Note: Results for the cutoff at January 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1953.5 and 1954.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 20: *RD results, January 1st, 1954 cutoff, by pension wealth, administrative data*



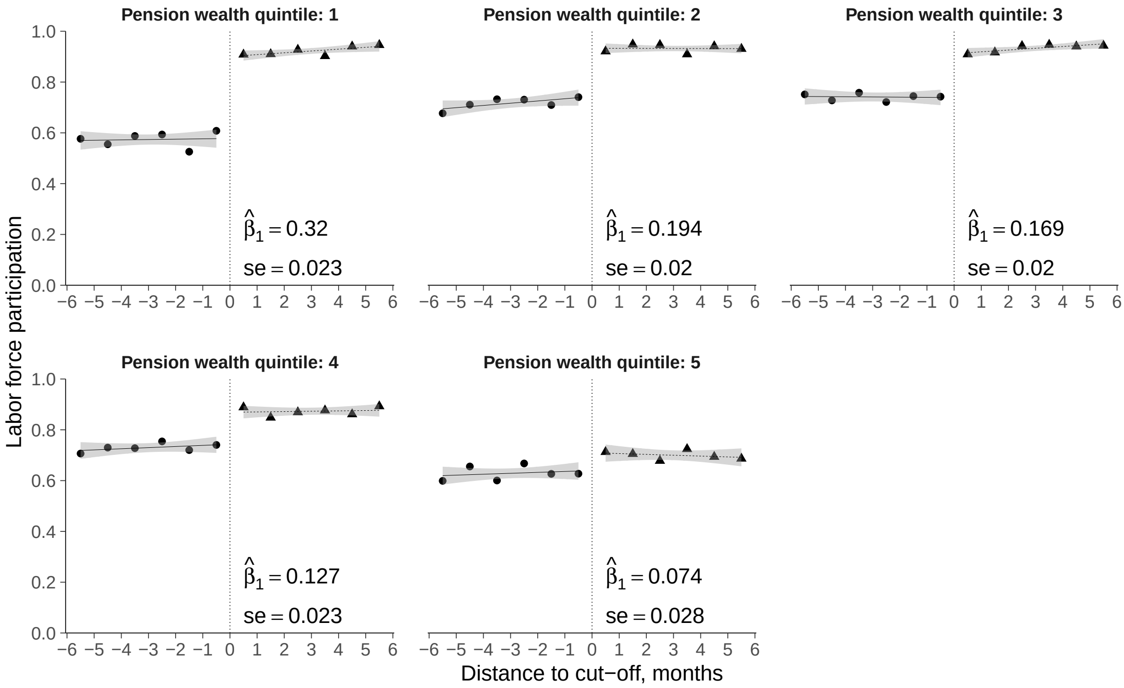
Note: Results for the cutoff at January 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1953.5 and 1954.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Figure 21: *RD results, July 1st, 1954 cutoff, administrative data*



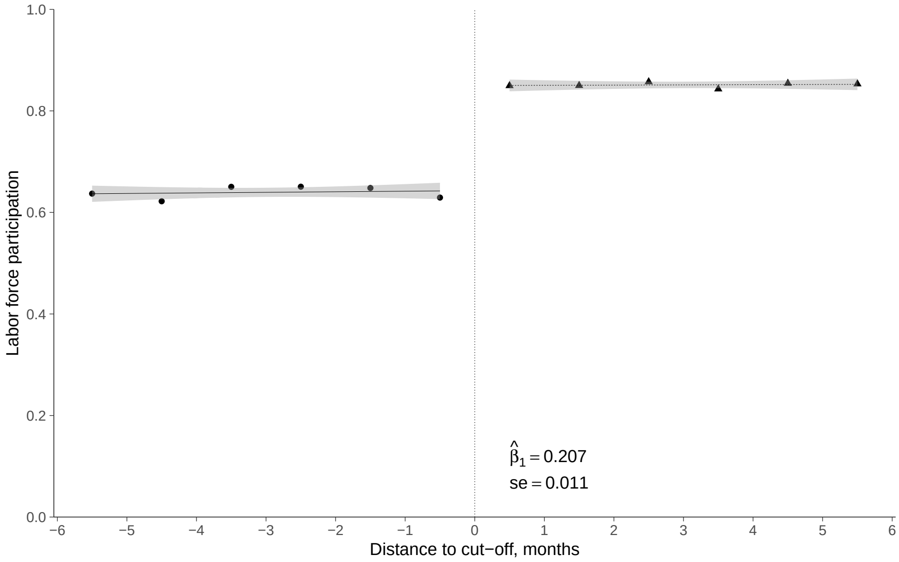
Note: Results for the cutoff at July 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1954.0 and 1954.5 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 22: *RD results, July 1st, 1954 cutoff, by pension wealth, administrative data*



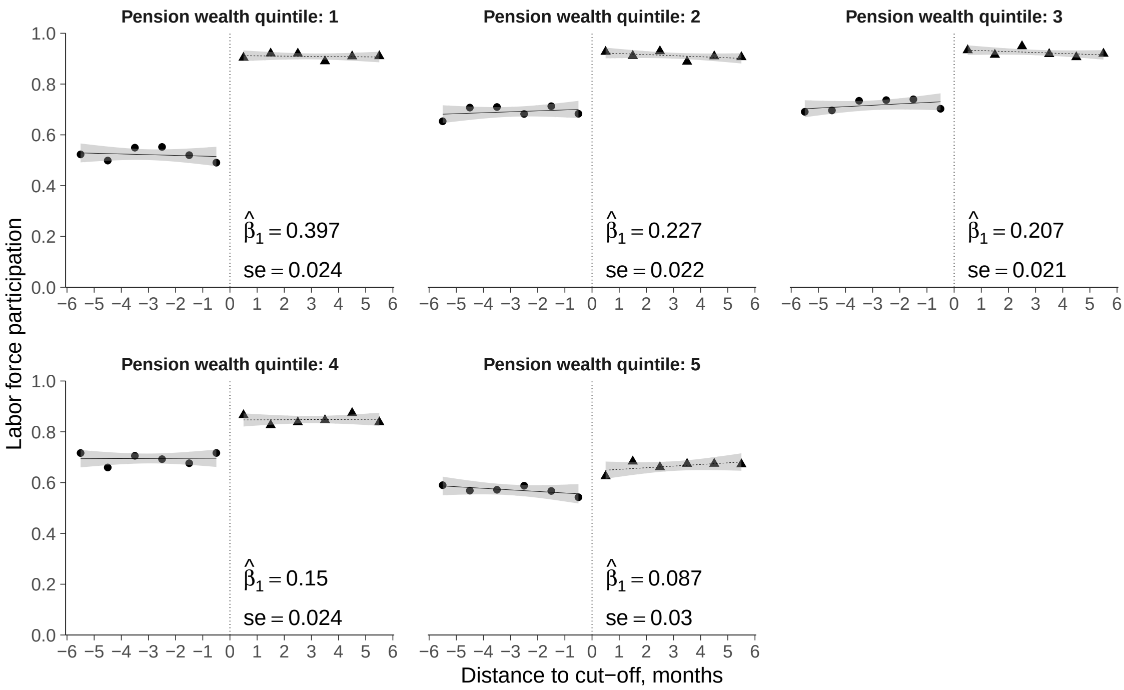
Note: Results for the cutoff at July 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1954.0 and 1954.5 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Figure 23: *RD results, January 1st, 1955 cutoff, administrative data*



Note: Results for the cutoff at January 1st, 1955. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1954.5 and 1955.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 24: *RD results, January 1st, 1955 cutoff, by pension wealth, administrative data*

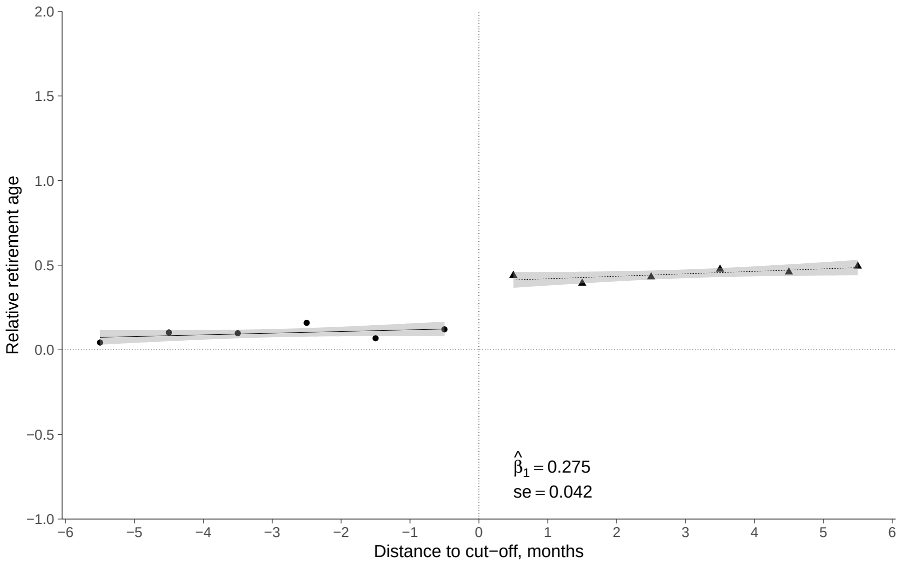


Note: Results for the cutoff at age January 1st, 1955. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1954.5 and 1955.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Appendix I Additional Admin Results, Retirement Age

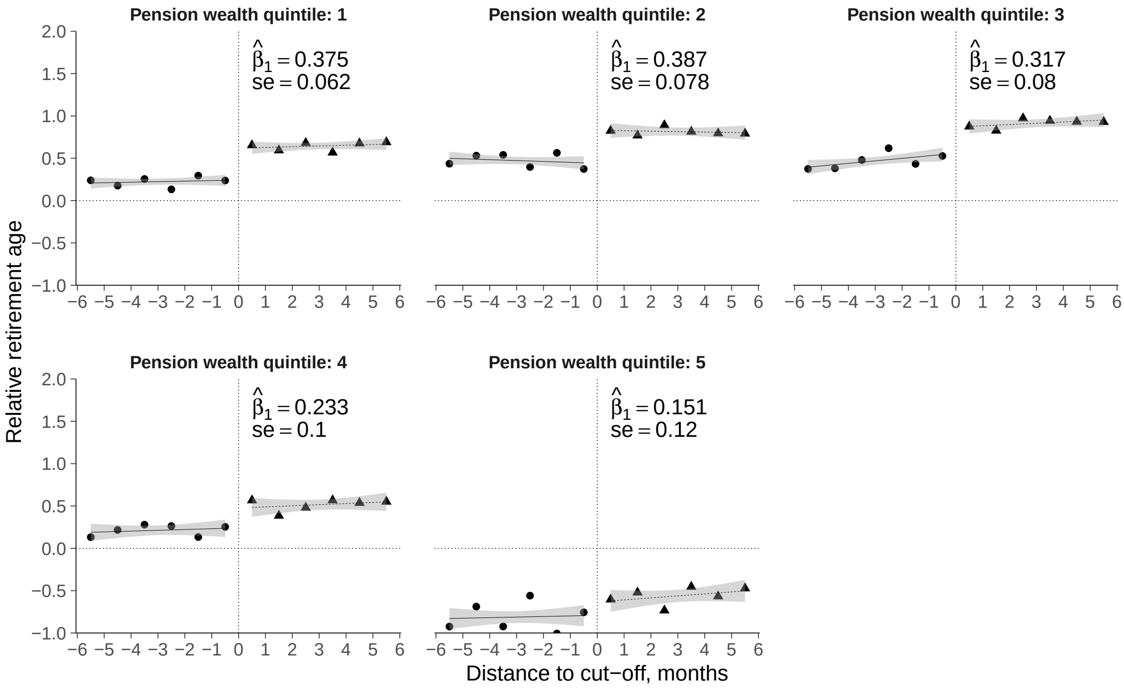
Figure 25 illustrates the effect of increasing eligibility age on retirement. Figure 26 shows the RD estimates by pension wealth.

Figure 25: *RD results, retirement age, administrative data*



Note: Results for the cutoff at January 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1953.5 and 1954.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 26: *RD results, retirement age, by pension wealth, administrative data*



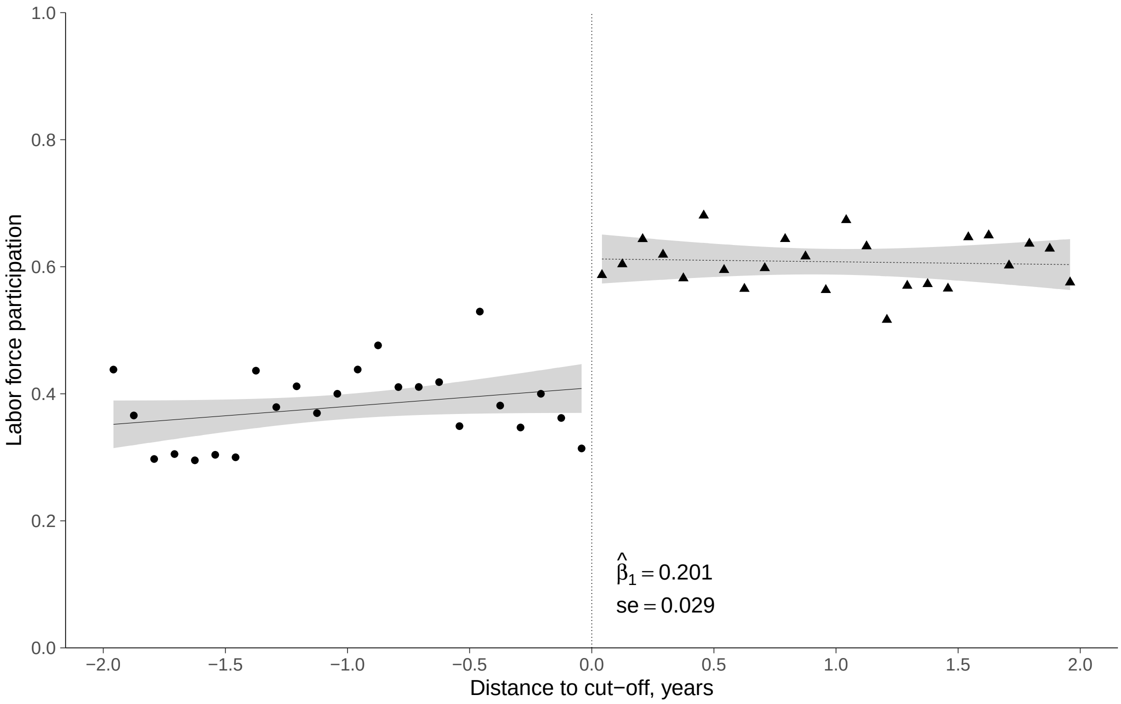
Note: Results for the cutoff at January 1st, 1954. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts 1953.5 and 1954.0 in the administrative data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Appendix J Additional Survey Results

Appendix J.1 Survey results, pooled

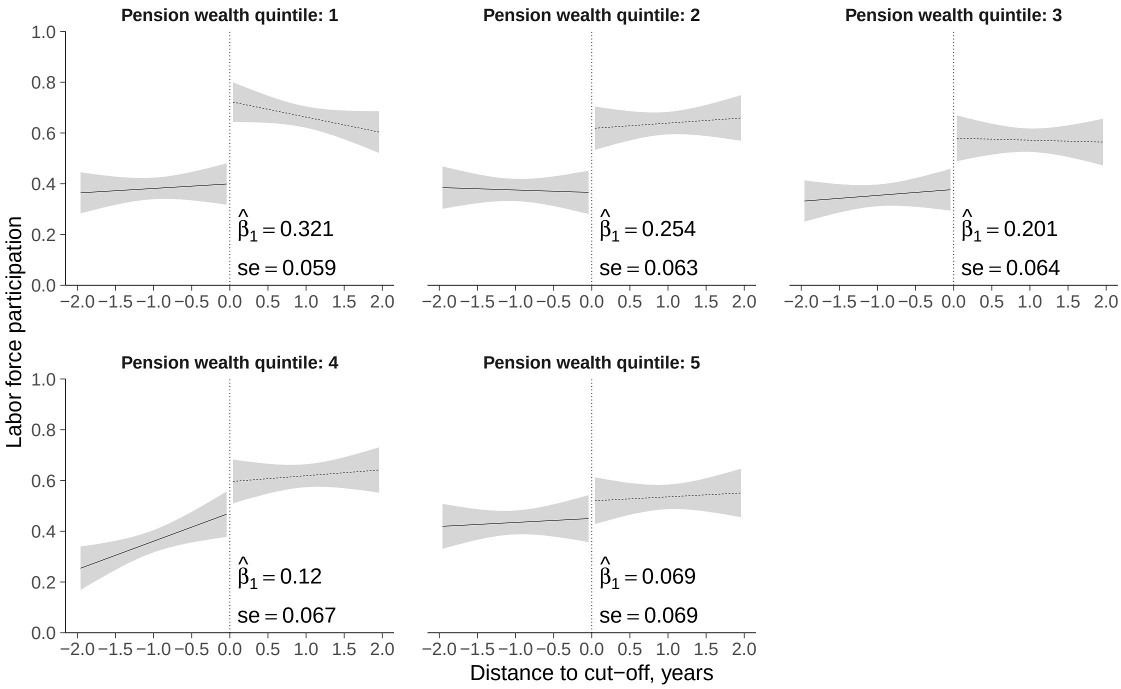
Figure 27 shows the pooled RD results for all three cutoffs and Figure 28 shows the pooled results by pension wealth quintile. The effect sizes correspond to Figure 6b in the main document.

Figure 27: *RD results, pooled cutoffs, survey data*



Note: This figure presents regression discontinuity (RD) estimates based on Equation 1 using pooled survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 28: *RD results, pooled cutoffs, by pension wealth, survey data*

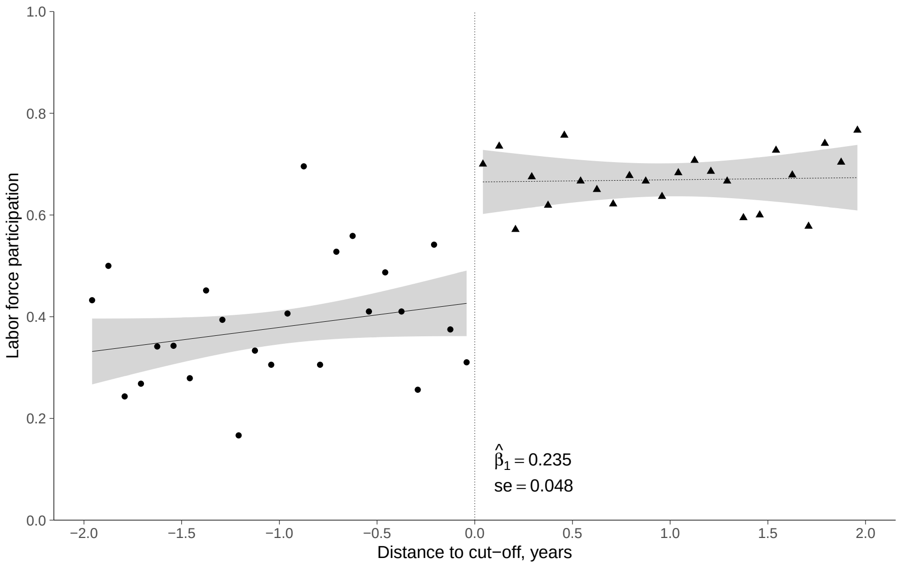


Note: This figure presents regression discontinuity (RD) estimates based on Equation 1 using pooled survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points are omitted for discretionary reasons. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Appendix J.2 Survey results, by birth date cutoff

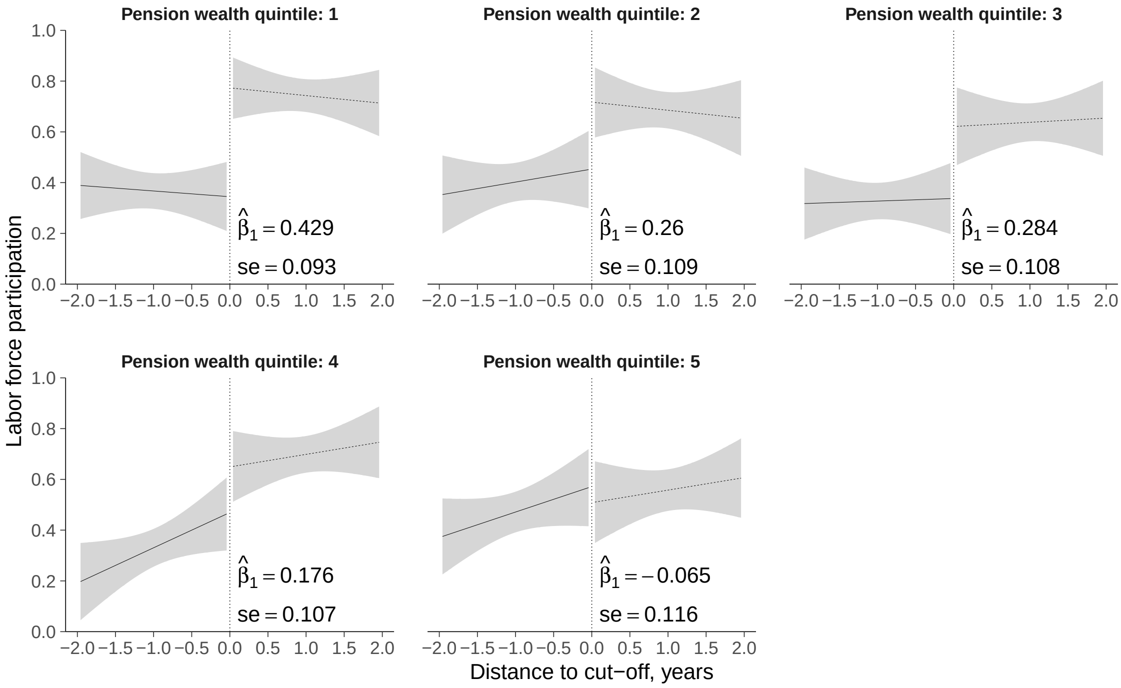
The following pages show the RD results for each birth date cutoff in the survey, both in the aggregate and by pension wealth. Results are quite noisy, but the pension wealth gradient is still present.

Figure 29: *RD results, January 1st, 1963 cutoff, survey data*



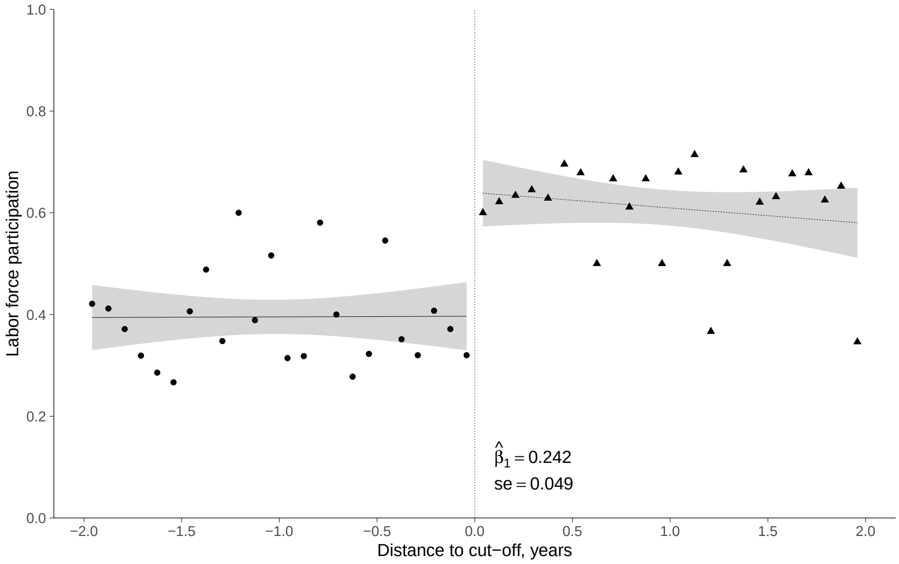
Note: Results for the cutoff at January 1st, 1963. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 30: *RD results, January 1st, 1963 cutoff, by pension wealth, survey data*



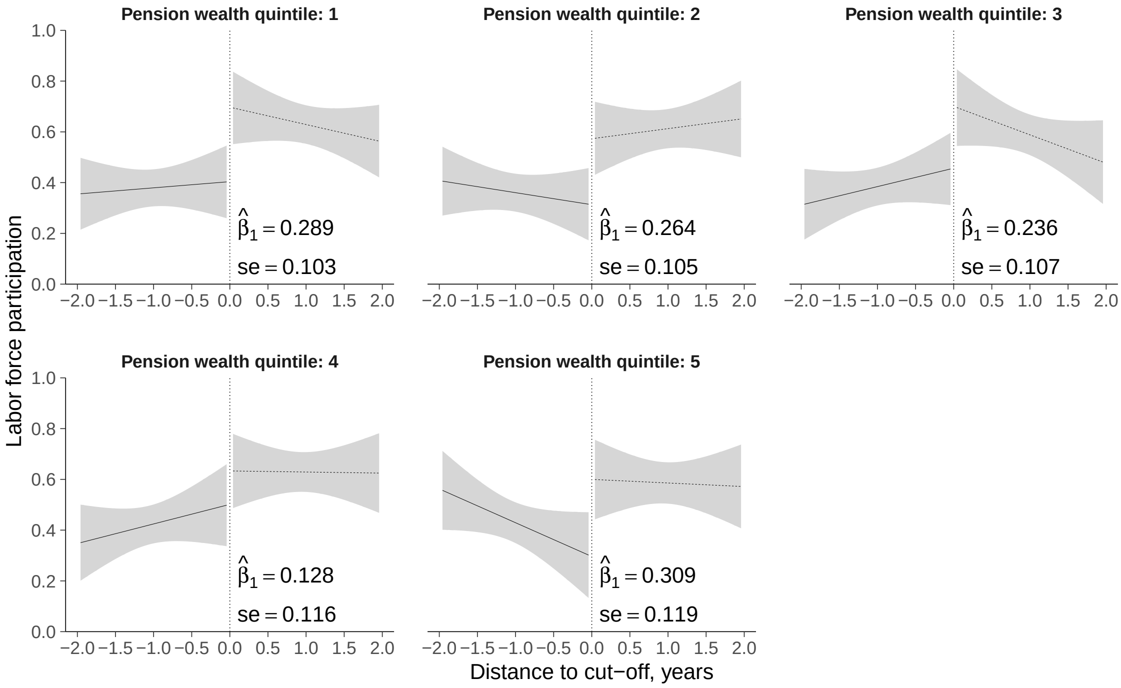
Note: Results for the cutoff at January 1st, 1963. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points are omitted for discretionary reasons. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Figure 31: *RD results, January 1st, 1967 cutoff, survey data*



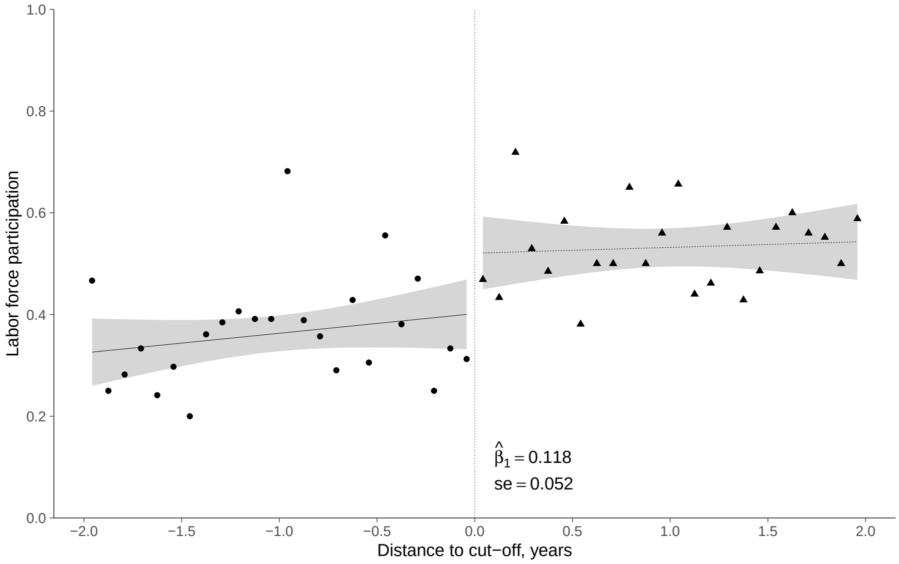
Note: Results for the cutoff at January 1st, 1967. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 32: *RD results, January 1st, 1967 cutoff, by pension wealth, survey data*



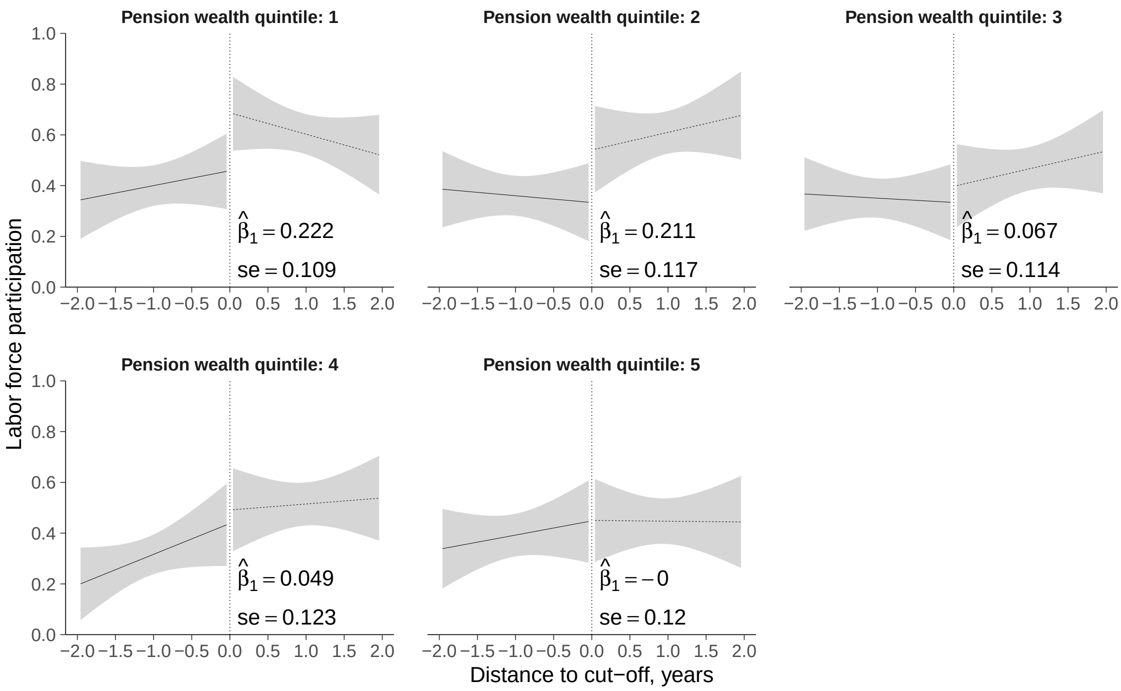
Note: Results for the cutoff at January 1st, 1967. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points are omitted for discretionary reasons. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.

Figure 33: *RD results, January 1st, 1971 cutoff, survey data*



Note: Results for the cutoff at January 1st, 1971. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points denote monthly averages: circles for the control group and triangles for the treatment group. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure.

Figure 34: *RD results, January 1st, 1971 cutoff, by pension wealth, survey data*



Note: Results for the cutoff at January 1st, 1971. This figure presents regression discontinuity (RD) estimates based on Equation 1 using two cohorts on either side of the cutoff in the survey data. The x-axis shows the distance to the social security eligibility cutoff in months; negative values correspond to the older cohort (control group) unaffected by the reform. The y-axis measures labor force participation, defined as the share of individuals not retired three months after the eligibility threshold. The lines represent linear fits with 95% confidence intervals estimated on individual-level data. Points are omitted for discretionary reasons. The estimated treatment effect, $\hat{\beta}_1$, and its standard error are reported in the figure. Each panel shows the estimates separately for different wealth quintiles.