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THE IMPACT OF SOCIAL ELIGIBILITY AND PENSION WEALTH ON RETIREMENT

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

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Abstract

I investigate a Danish policy reform that postpones social security eligibility tied to an increase in life expectancy. The reform creates sharp discontinuities based on exact birth dates, allowing for 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 postponing social security eligibility. The effect is strongest among individuals with low pension wealth. This pattern is consistent across multiple retirement age thresholds and cohorts, including both individuals who have already retired and in expectation for younger cohorts who are not yet retired. This research offers new insights into the impacts of life expectancy-based adjustments to social security eligibility. Welfare assessments show overall gains, but also that welfare effects are unequally distributed. Individuals with low pension wealth show the largest increases in labor supply, but also face the largest personal costs in terms of foregone consumption smoothing.

Keywords: retirement, social security, labor supply

JEL Codes: J26, H55

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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 involves postponing the eligibility age for social security benefits, thus incentivizing extended work life[1]. At the same time, the global shift towards defined contribution (DC) occupational pension plans has resulted in substantial pension wealth accumulation in many developed countries[2]. If leisure is considered a normal good, the rise in pension assets may lead to earlier retirement, potentially weakening the effects of postponed social security eligibility[3]. Assessing the joint influence of pension wealth accumulation and postponed eligibility on retirement decisions is vital for understanding the overall labor supply response and the policy's distributive consequences. Securing fiscal sustainability may require many countries to extent social security eligibility much beyond what has currently been implemented. However, empirical examination of the implications of postponing social security eligibility ages remains sparse.

I examine how postponed social security eligibility across different levels of pension wealth in DC pension accounts impact retirement decisions in Denmark. Denmark is a front-runner due to its pioneering 2006 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, Denmark's robust DC pension framework that has mandated worker contributions to DC accounts since the early 1990s, leading to the accumulation of significant retirement wealth in DC accounts.

To establish the causal impact of the delayed eligibility age on retirement behavior, I employ 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.

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 including

[1]Some countries implement this postponement as a singular measure, yet it is plausible they will have to enforce further delays as life expectancy continues to climb 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.

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. This change reflects an 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 31 percentage points), when affected by the reform. In contrast, individuals with substantial pension wealth only increase labor force participation by 9 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.

I also investigate the broader societal ramifications of the reform. Employing the analytical framework from Kolsrud et al. (2024) I quantify the welfare effects of postponing social security eligibility, by weighting the societal benefits derived from augmented tax revenues against the consumption smoothing cost experienced by retired workers. The overall societal effect is positive, but the benefits are distributed unevenly. Individuals with low pension wealth postpone retirement the most and thereby providing significant societal benefits. However, these same individuals bear higher costs because they have a lower ability to smooth consumption.

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)).

In another strand of the retirement literature, Seibold (2021) highlighted the role of reference dependence in retirement decisions, suggesting that merely financial incentives cannot fully explain observed behaviors. Similarly, Lalive et al. (2023) demonstrated how claiming and retirement decisions are coupled in a non-optimal way, partly due to reference dependence. While I do not take a stance on the exact mechanisms behind the responses I measure, my work contributes by documenting the overall response and its implications.

Furthermore, my research adds a novel 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 reveals 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. This method insights into the long-term effects of a social security reform that ties the eligibility age to life expectancy. I demonstrate that even in expectation, the reform's impact is heterogeneous across levels of anticipated pension wealth.

Finally, I contribute to the literature on assessing the welfare implications of reforms, a field pioneered by Baily (1978) and extended by Chetty (2006). Building upon the work of Kolsrud et al. (2024), I calculate the welfare effects associated with postponing social security eligibility ages. Employing their theoretical framework, I derive labor supply elasticities and welfare effects that that uncover highly unequally distributed effects of postponing eligibility.

While I investigate how pension wealth influences retirement decisions, other research has probed the opposite relationship. Specifically, García-Miralles and Leganza (2021) examined the same Danish reform I discuss, emphasizing the effects of postponing early retirement ages (from 60 to 61) on voluntary pension savings, concluding that reduced

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

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 Giupponi (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.

2 Structure of the Danish Pension System

The Danish pension system operates on a system comprising three main components: social security benefits, occupational pensions, and individual supplementary pensions. This multi-pillar approach ensures a well-rounded retirement income for most individuals by combining public support with private savings[5].

2.1 Social Security

At the heart of the Danish pension system lies the state pension, funded through a pay-as-you-go mechanism. This universally applied flat-rate benefit is provided to all Danish nationals upon reaching the statutory eligibility age, irrespective of labor market trajectory[6]. Historically, this eligibility age was standardized at 65 for both genders.

The benefit structure is contingent on an individuals specific situation. Comprising a basic transfer and an earnings-dependent supplement, both components undergo means testing relative to earnings and occupational pension revenues. In 2023 the maximum annual social security benefit is approximately $25,160 for single-resident individuals and $18,570 for cohabiting counterparts. Adjustments are made annually to reflect shifts in average wages and living costs.

2.2 The Welfare Agreement and Its Implications

In 2006, an overwhelming parliamentary majority endorsed the Welfare Agreement. This reform pivoted from the standard 65-year eligibility age, aligning it with forecasts of life expectancy from 2024 onward. The reforms initial 2006 timeline underwent an expedited shift in 2011, manifesting its effects as early as 2019. Figure 1 and Table 1 depict the phasing in of the reform. Importantly, this reform exclusively addresses eligibility ages and does not affect other aspects of the retirement system, making it a compelling case for examination.

[5] See Chetty et al. (2014) for more details on the Danish pension system.

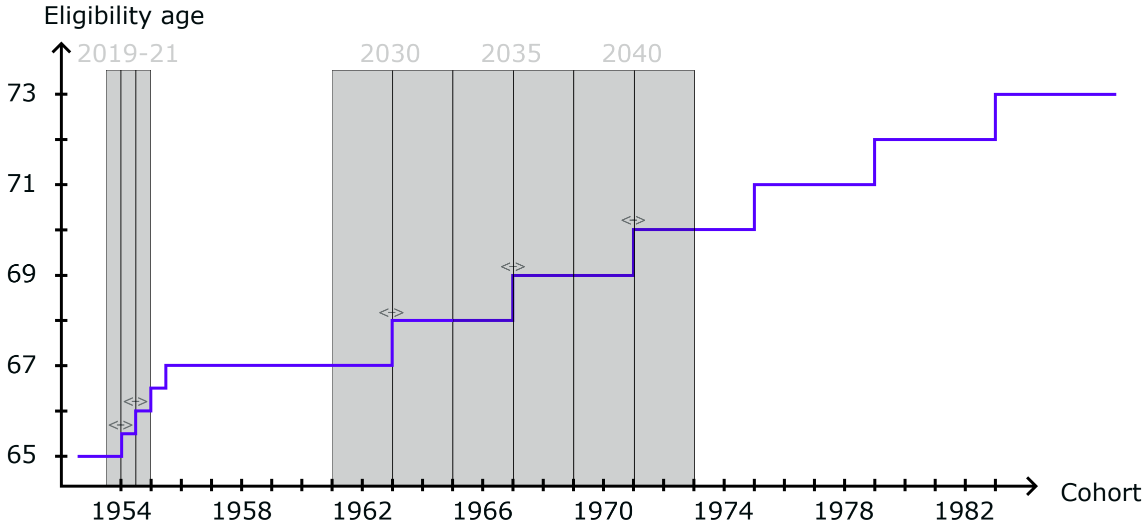
[6] The level of benefits is reduced proportionally for individuals who have lived less than 40 years in Denmark.

Table 1: *Reform details*

	Birth dates	Eligibility age	Starting year	# of C/T
Admin	-31 December 1953	65.0		
Admin	1 January 1954-	65.5	2019	9,811/10,732
Admin	1 July 1954-	66.0	2020	10,732/9,888
	1 January 1955-	66.5	2021	
Survey	1 July 1955-	67.0	2022	
Survey	1 January 1963-	68.0	2030	824/814
Survey	1 January 1967-	69.0	2035	817/760
Survey	1 January 1971-	70.0*	2040	715/679
	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 (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) split on control and treatment groups. Older cohorts (1953.5, 1954.0, 1954.5) are used in the administrative analysis and younger cohorts (1961-1972) are used in the survey analysis.

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



Note: The blue line shows the effect of the reform where the steps show the eligibility ages and the shaded bars indicate cohorts who are compared in the analysis. The figure clearly shows how the reform was phased in steeply in 2019 and then more gradual from 2030 (the grey numbers above the bars indicate when a given change takes place).

2.3 Occupational Pensions

Occupational pensions, introduced progressively from the early 1990s, have since cemented their role as the secondary pillar, safeguarding income continuity in retirement. Originating from employer initiatives and frequently sculpted through collective bargaining, these funds impose stringent penalties for premature withdrawals. The dominant model in Denmark is the defined contribution (DC) paradigm, requiring dual contributions from employers and employees. These tax-deductible contributions combine to offer retirement compensations, either as a lump sum or in annuity formats. Even though savings distributions exhibit disparities across cohorts, unsurprisingly higher earners typically amass more substantial pension wealth. Withdrawals can begin no earlier than five years prior to social security eligibility and is subject to standard income tax.

2.4 Individual Supplementary Pensions

Supplementing the dual pillars of state and occupational pensions, Danes can optionally invest in individual supplementary pensions. This personal initiative permits augmenting occupational retirement savings by diversifying investments across instruments like mutual funds or life insurance vehicles. Despite the variability in contribution magnitudes, the significance of state and occupational pensions in retirement strategies remains undiminished.

3 Data

I investigate behavioral responses to variations in the statutory eligibility age for social security benefits using both administrative records and survey responses. The administrative data offers a retrospective view by examining the reactions of already impacted cohorts. In contrast, the survey data provides a prospective angle, forecasting responses from yet-to-be-affected cohorts. This dual approach ensures a holistic examination of responses to changes in the social security eligibility age.

3.1 Administrative Data

I utilize two detailed administrative datasets from the Danish population. The first dataset, covering the period 1987 to 2021, comprises variables such as demographics, family ties, education, earnings, and savings. The second, a monthly record from 2008 to 2021, contains detailed employment information and comprehensive public transfer data, including social security payments.

For the analysis, I segment individuals into half-year cohorts (1953.5, 1954.0, 1954.5) 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 and 1954.0 vs 1954.5). 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 three narrow shaded bars around 1954 and the small arrows across the bars indicate a comparison. It is not yet possible to include cohorts 1955.0 and 1955.5 as their retirement is not yet observed.

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[7], 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. Refer to Appendix A for more details.

I define retirement as the month where an individual is no longer employed, has no earnings, and starts receiving pensions from either social security or income from occupational or private pension accounts. Supplementary analyses in Appendix C.1 confirm the robustness of this definition.

I stratify individuals into quintiles based on accumulated pension wealth to gauge differential responses to the reform. I use data from a year before potential withdrawals to mitigate endogeneity. Figure 8 in Appendix B reports the distribution of pension wealth at age 59 for the selected cohorts and concerns about potential savings adjustments are addressed. Appendix C.2 compare different wealth types' impact on retirement decisions.

As a first order check Figure 2 graphically illustrates retirement transitions by cohort and pension wealth levels for the administrative sample. There is a distinct drop of between 10 and 30 percentage points in labor force participation exactly at the statutory eligibility age. The figure shows that people do respond strongly to the reform, and the patterns across different cohorts (within each panel) are similar, indicating that the cohorts are comparable. However, people with low pension wealth (panel 1) exhibit a much larger response than those with high pension wealth (panel 5). This initial result supports the hypothesis that high pension wealth individuals are less sensitive to changes to social security eligibility age.

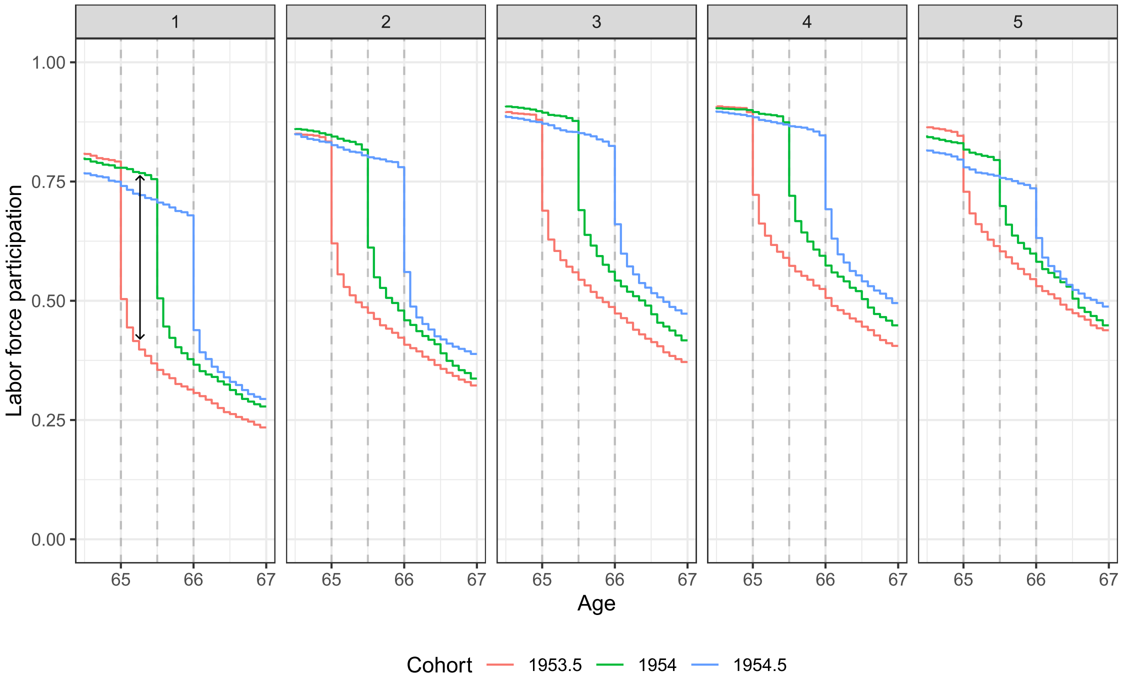
3.2 Survey Data

The survey instrument is part of the newly developed *Copenhagen Life Panel* [8] (CLP) which is an online panel survey implemented in Denmark. From the 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

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

[8]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: *Retirement patterns, by pension wealth and cohort, administrative data*



Note: The five panels show how people in the administrative data transition into retirement for different pension wealth levels. In each of the panels, the line type denotes a half-year cohort. Vertical dashed lines denote statutory eligibility ages. The arrow in Panel 1 shows the difference in labor force participation for low pension wealth workers cohorts 1953.5 and 1954.0 at age 65 years and 3 months, exactly when they differed in eligibility for social security benefits.

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 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. From the invited 18,796 individuals across selected cohorts, 5,006 responded. The analysis 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. 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 Appendix D. I compare survey respondents and non-respondents on observables in Appendix Appendix D.1 and find that respondents are better educated, have higher employment rates, earn more, and have higher wealth.

For each respondent I elicit the expected retirement age[9], and use it age to create a time series dummy where 1 indicates employment and 0 indicates retirement.

I use the anticipated annual retirement income to categorize individuals into pension wealth quintiles by eligibility age bracket[10]. Figure 3 graphically illustrates 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 the expected retirement behavior is so similar to the observed behavior in the administrative data indicates 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.

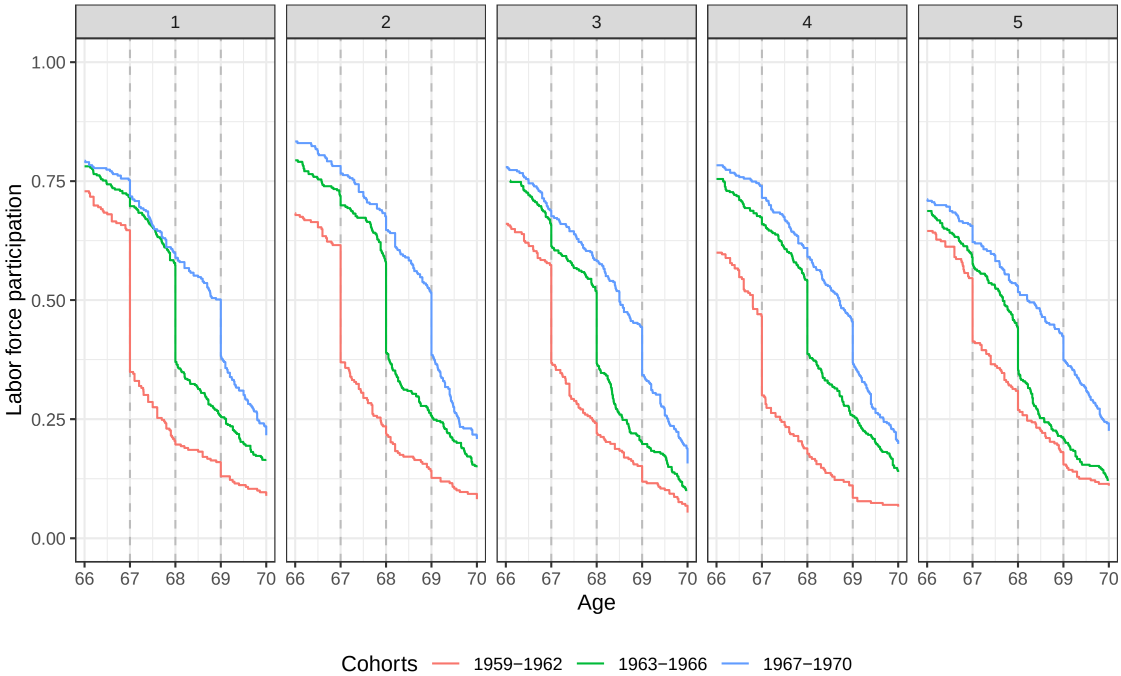
4 Identification Strategy

I employ Regression Discontinuity (RD) design, a potent identification strategy that identifies causal effects at sharp discontinuities, to quantify the impact of postponing social security eligibility age on retirement choices.

Fundamentally, RD assumes individuals on either side of a distinct threshold, like an age cutoff, differ only in the treatment initiated by the discontinuity[11]. Additionally, individuals must be unable to manipulate their position, thereby creating bunching on either side of the threshold. In the retirement context, the age-based social security benefit eligibility provides this discontinuity. Leveraging this natural experiment, I determine the causal consequence of extending the eligibility age.

[9] For details, see Appendix D.2.
[10] Appendix C.3 shows the main result using current pension wealth to stratify individuals.
[11] Refer to Appendix E for balancing checks across age cutoffs, I find no significant differences.

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



Note: The five panels show how people in the survey data expect to transition into retirement for different pension wealth levels. In each of the panels, the line type denotes a half-year cohort. Vertical dashed lines denote statutory eligibility ages.

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. This model links labor supply on the extensive margin (employed versus retired) to the running variable (W_i), denoting proximity to the age cutoff. The treatment dummy, D_i, indicates if the individual is born pre or post the age 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 variable of interest, β_1, denotes the average treatment effect the treated on labor supply decisions of postponing eligibility age at the thresholds listed in Table 1. The model accommodates varying slopes on either side of the age cutoff, defined by β_2 and β_3. ε_i represents the error term.

I analyze every age cutoff in the reform, focusing on those born within six months of the cutoff in administrative data. For instance, the control for the initial age cutoff comprises individuals born in the latter half of 1953 (cohort 1953.5), while the treatment group includes those born in early 1954 (cohort 1954.0). At 65, the control cohort qualifies for social security, but the treatment group waits an additional six months. I evaluate whether each individual works at age 65 and 3 months, e.i. the middle of the period where eligibility differs between the groups.

When I analyse the survey data, I include individuals born within a two-year range on either side of the age cutoff to increase statistical power. Appendix C.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 analysis of the administrative and survey data, I pool data across cutoffs and align the data to gain more statistical power.

5 Regression Discontinuity Results

Utilizing the Regression Discontinuity (RD) design, I quantify the causal effect of postponing the social security eligibility age compared to the previous cohort. I evaluate this effect across both administrative and survey datasets with varying pre-retirement pension wealth levels.

5.1 Retirement Patterns from Administrative Data

The results presented below are from pooled data across comparisons (refer to Table 1) where control and treatment groups are aligned by W_i, distance to the age cutoff. Detailed results for each comparison are shown in Appendix F.

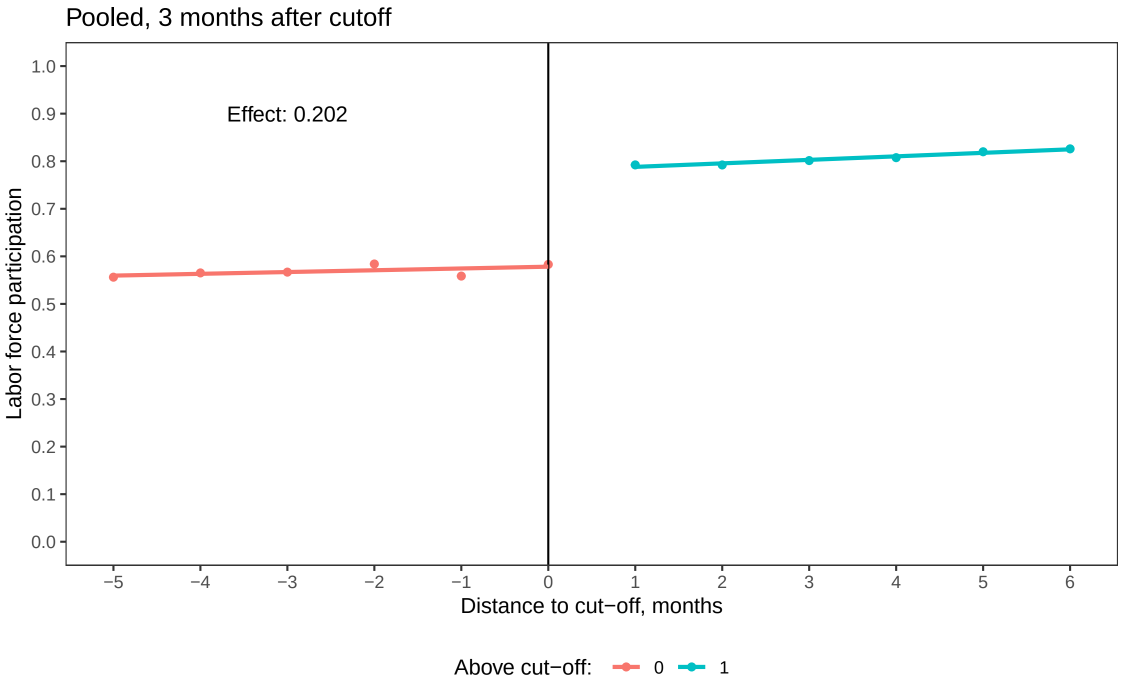
Figure 4 plots the treatment dummy $\hat{\beta}_1$ estimates and illustrates the results for the case where the age cutoffs are pooled: older cohorts (red: control) are on the left, and younger cohorts (blue: treatment) on the right. The y-axis depicts the proportion not retired 3 months after the eligibility age. The running variable is the distance from the birth date to cutoff date in months. A distinct jump at the cutoff reveals that younger individuals exhibit a 20.2 percentage point higher likelihood of working and this appears to be unrelated to the distance to the cutoff.

The observed pronounced effect aligns with prior findings in the literature[12]. Given the reform's intent to prolong working duration, its success is evident. The consistent effects within each cohort anticipate no systematic differences within individuals born six months on either side of the cutoff.

Figure 5 replicates the analysis by pension wealth quintile. The inference is unequivocal: the lesser-wealth group shows a pronounced response (30.9 percentage points) com-

[12] Conditions differ between countries and reforms, previous studies have found estimates in the vicinity, e.g. 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).

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

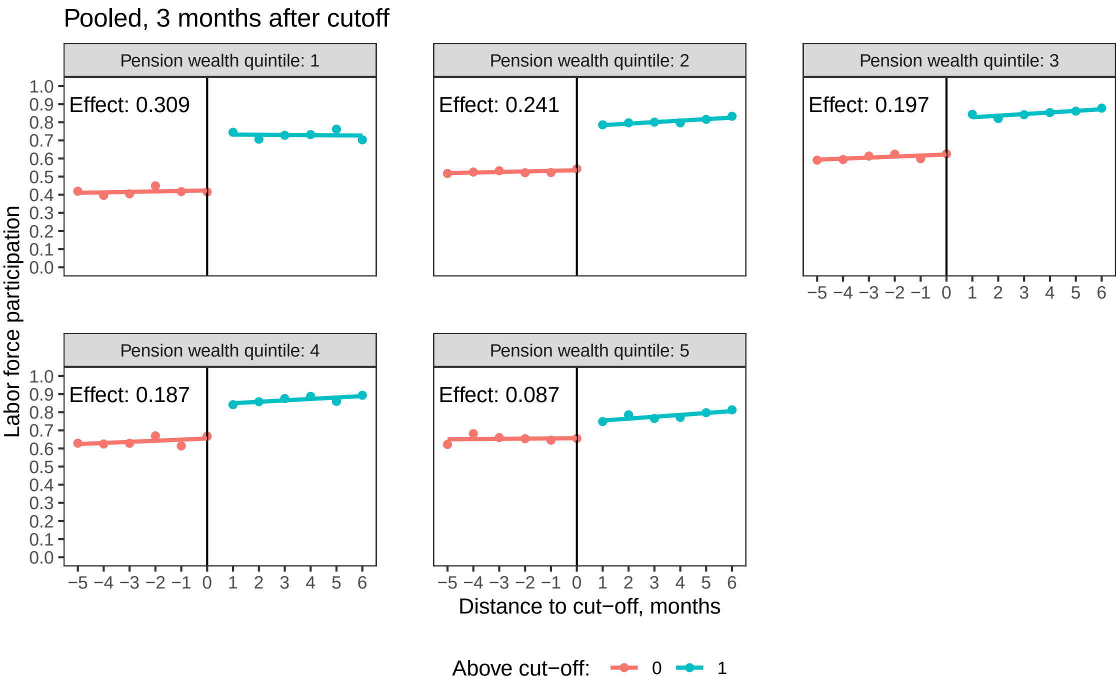


Note: The figure shows RD estimates from Equation 1 for the pooled administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

pared to the high-wealth group (8.7 percentage points). This resonates with the premise that those with substantial pension wealth face fewer constraints from changes to social security eligibility age, given its lesser contribution to their retirement income. This novel observation suggests the welfare effects of deferred social security eligibility age exhibit a non-uniform distribution, which I elaborate further on in Section 6.

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

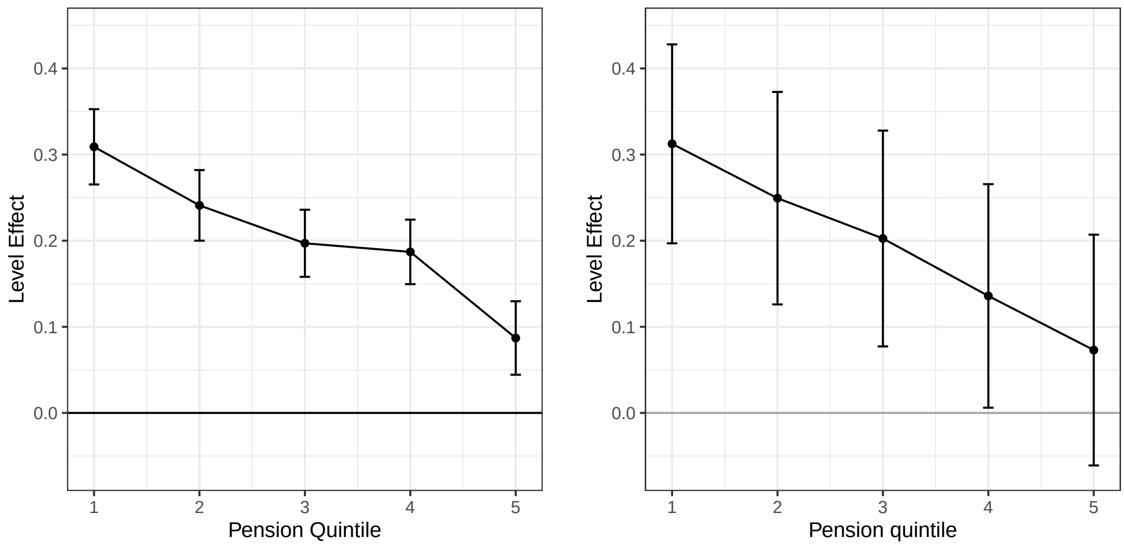
Figure 5: *RD results, pooled cutoffs, by pension wealth, administrative data*



Note: The figure shows RD estimates from Equation 1 for the pooled administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Figure 6: *Labor supply response to reform*

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



Note: The figure shows RD results from Equation 1 in the administrative data, Panel (a), and survey data, Panel (b).. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals.

5.2 Survey results

In analyzing social security reforms, the literature has traditionally considered post-retirement responses. As evidenced above, a delay in eligibility age spurs later retirement, most pronounced among low pension wealth individuals. Here, I expand the scope to anticipate future responses, factoring in retirement expectations of the not-yet-eligible.

Consistent with the previous analysis, I pool the three available age cutoffs from the survey dataset[13]. Figure 6b presents the treatment dummy $\hat{\beta}_1$ estimates, segmented by pension wealth, and confirms administrative data insights: individuals, particularly those with minimal pension wealth, react to the reform. While these findings are less precise due to limited sample sizes, their magnitudes align closely. The expected low pension wealth individuals respond by 31.2 percentage points and the high wealth individuals by 7.3 percentage points. The detailed outcomes of each cutoff is presented in Appendix G.

The survey data analysis suggests this pattern in administrative data will likely persist in forthcoming years. Expected responses to retirement ages even beyond 70 showcase a decrease with pension wealth growth. The next section delve into the welfare implications of reforms delaying social security benefit access.

6 Welfare Implications

Above, I document that when confronted with a reform raising the social security eligibility age, individuals adjust their retirement timing. Notably, those with low pension wealth delay retirement compared to those with high pension wealth.

The 2006 reform's overarching aim is to achieve fiscal sustainability by reducing social security expenses and have people work longer. By postponing access to social security benefits, it encourages later retirement, benefiting the government in two distinct ways: 1. direct savings from deferred benefit disbursements, and 2. enhanced tax revenues from prolonged working years. But workers, of course, may experience a cost associated with delayed access to social security benefits in terms of a utility loss as they have to work longer. In this section, I quantify the welfare implications of a simplified reform that contrasts the added tax revenue from the behavioral shifts with the individual costs. The simplified reform mirrors the actual reform without considering the direct savings on social benefits, and this allows me to precisely quantify the net welfare impact of the reform while maintaining a balanced government budget.

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

The welfare analysis is based on a stylized reform that redistributes resources within one cohort, transferring from those retiring earlier than a set benchmark to those retiring later. The analogy to the actual reform is for example the cohort born in 1954: those born in the first half of the year can claim benefits at age 65.5, but those born in the second half can only claim at age 66. We can think of the balanced budget of the simplified reform as the government transferring a lump-sum amount back to the entire cohort, corresponding to the savings on social security benefits. All else equals, such a reform motivates certain individuals to extend their working careers. This design lets me measure the increased tax revenue and balance this against the longer working durations which increase individual costs, while sidestepping potential inter-generational distributive concerns.

This analytical approach draws on the methodology outlined in Kolsrud et al. (2024), and it is rooted in the principles of social insurance theory as established by Baily (1978) and Chetty (2006). I compute total net welfare, ΔW, as the positive fiscal externality, FE, to the government (increased tax revenue) offset by individual consumption smoothing costs, CS, associated with retiring at a later age:

$$\Delta W = FE - CS \tag{2}$$

6.1 Fiscal Externality

Using the framework from Kolsrud et al. (2024)[14], I derive the fiscal externality (FE) as a function of average tax rate during employment and the labor supply elasticity[15]:

$$FE \approx \frac{T}{w}\varepsilon \tag{3}$$

The tax rate, $\frac{T}{w}$, for an individual with a gross income of w and tax liability of T, summarizes the societal benefit of having that individual in the workforce.

The labor supply elasticity, ε, captures the behavioral response to a delay in the access to social security benefits: how much more are people willing to work given that they can no longer receive the benefits? As in the analysis in Section 5, I exploit the age discontinuity in the access to the benefits created by the reform, this time contrasting the difference in labor supply elasticities across age cutoffs, where the control group is the older cohort with access to social security, and the treatment group is those without access.

[14]See Appendix H for derivations and a detailed discussion.

[15]I the main text I omit subscripts for legibility, see Appendix I for full derivation.

I estimate the labor supply elasticity, ε, following Laun (2017) by regressing labor supply (at the extensive margin), $P_{i,t}$, of individual i at time t on the net-of-participation tax rate $(1 - \tau_{i,t}^A)$, controlling for year fixed effects, δ_t, and month of birth, μ_a. $u_{i,t}$ is an error term:

$$P_{i,t} = \varepsilon \log(1 - \tau_{i,t}^A) + \delta_t + \mu_a + u_{i,t} \tag{4}$$

Above the age threshold the control group (who are now eligible for social security) will face a high net-of-participation tax rate as opposed the treatment group who will face the same low net-of-participation tax rate as below the threshold.

To address potential endogeneity, I use an IV approach and instrument $\log(1 - \tau_{i,t}^A)$ using a binary variable which assumes a value of 1 post the age threshold for the control group and 0 otherwise. Under the assumption of parallel trends i.e., I assume identical behavior between the two cohorts in the absence of the reform the instrument should correlate with $\log(1 - \tau_{i,t}^A)$, but not with the error term, u_{it}, in Equation 4. Figure 28 in Appendix J displays mean net-of-participation tax rates, segmented by cohort and age relative to the social security eligibility benchmark. Above-threshold control group cohorts gain noticeably less from working as their net-of-participation tax rate is much higher, confirming the relevance of our instrument. Under the assumption that nothing else systematically influences the labor supply of these cohorts during the study's timeframe, the instrument is valid. This is likely as there were no concurrent reforms affecting social security rules.

The individual net-of-participation tax rate is given by:

$$(1 - \tau_{i,t}^A) = \frac{d_i - d_{i,0}}{w_i} \tag{5}$$

where d_i is after tax income when working, $d_{i,0}$ is after tax income when working, and w_i is gross income when working.

I observe individuals exclusively in either employed or unemployed states. However, for a comprehensive tax rate calculation, counterfactuals for both states are essential. Using hedonic regressions[16] on senior cohorts, I estimate income and corresponding tax rates both for employment and unemployment across individuals close to the age cutoff, as outlined in Equation 5. The underlying assumption is that working and non-working individuals with similar observable characteristics are comparable.

For hedonic regressions, I employ a sample comprising older cohorts (1948-53), dividing

[16]The regressions are explained in Appendix J.

them by retirement age above and below age 65, denoted employed and retired. I condition on them being employed at age 63 and not on disability, early retirement or self-employed. I record their incomes at age 65, the age of universal social security eligibility for those cohorts. For the employed, I regress both gross earnings, denoted as w, and disposable income, represented by d, on gender, education across five tiers, average earnings at age 60-63, four-digit occupation codes, three-digit industry classifications, pension wealth, and other forms of wealth these latter three variables being recorded at age 59. For the retired, I regress their disposable income on the same set of variables, in both the control and treated case, denoted d_0^C and d_0^T. The superscript C indicates estimates for the control group above the age threshold who are eligible for social security and retired. For the treated and retired I impute their disposable income, d_0^T, by subtracting a fraction from their observed disposable income corresponding to social security out of gross income for the unemployed at age 65.

I use these estimates to predict both employed and retired outcomes of all individuals in the analysis sample (cohorts 1953.5, 1954.0, and 1954.5) for each comparison, i.e. in the first comparison cohort 1953.5 is the control group and 1954.0 is the treatment group. These regression exercises enable me to predict counterfactual incomes for all individuals in cohorts adjacent to the cutoffs, considering both their eligibility statuses for social security (as control or treatment groups) which varies differentially across the age cutoff, allowing me to compute 5 and estimate 4[17].

6.2 Consumption Smoothing Cost

The consumption smoothing cost captures the consumption decrease individuals encounter when retiring early rather than late. This decline in consumption around retirement was first described by Banks et al. (1998) and the size of this consumption drop is potentially influenced by the retirement age. I use this individual consumption drop variability to determine the costs linked to consumption smoothing.

Transferring resources from early to late retirees introduces a cost associated with missed opportunities for consumption smoothing. This cost relies on the social marginal utility of consumption, measured by the drop in consumption around retirement for early versus late retirees. I consider other individual costs linked to behavioral shifts to be of second-order significance, based on the envelope theorem, and thus disregard them in this analysis.

[17] Auxiliary results are presented in Appendix I: Figures 28 and 29 show the estimated net-of-participation tax rates and elasticities respectively.

I leverage consumption data to approximate the concept of social marginal utility (SMU) of consumption. Following the sufficient statistics method presented in Kolsrud et al. (2024), I view individual utility through the lens of two choice variables: consumption and a reduced form variable that encapsulates all alternative choices and individual traits influencing utility. Within this framework, the SMU of consumption can be inferred using a first-order Taylor series centered on consumption drops around retirement. There are several underlying assumptions in this approach, and two of the critical ones are that 1. workers differ only by retirement age, and 2. the SMU of consumption of workers is comparable across retirement ages[18].

In calculating consumption smoothing costs of the stylized reform I consider the changes in consumption when transferring one dollar from those retiring early (before the normal retirement age 65, denoted 65^{-}) to those retiring late (denoted 66^{+}). To arrive at a monetary measure of the consumption cost, I consider the difference in SMU between early and late retirees, divided by the SMU of those retiring at the normal retirement age 65:

$$CS = \frac{SMU_{65^-} - SMU_{66^+}}{SMU_{65}} \approx \frac{\gamma \Delta \bar{c}_{65^-} - \gamma \Delta \bar{c}_{66^+}}{1 + \gamma \Delta \bar{c}_{65}} \tag{6}$$

where $\Delta \bar{c}_r = \frac{\bar{c}_{r,pre} - \bar{c}_{r,post}}{\bar{c}_{r,pre}}$ is the average drop in consumption around retirement for people who people who retire at age r, and γ is risk aversion. If CS is positive there is a consumption smoothing cost associated with retiring late rather than early.

6.3 Implementation and Results

In this section, I analyze two distinct sets of cohorts from the registry. First, I employ the same cohorts (1953.5 to 1954.5) as in Section 5 to quantify the behavioral reactions to the reform and consequently estimate the labor supply elasticities. For estimating both income and tax around retirement, as well as the consumption decline post-retirement, I turn to another set of cohorts from 1948 to 1953. These older cohorts permit me to monitor their financial behaviors both before and after retirement and in turn predict outcomes of the younger cohorts in question. Including a broader range of cohorts enhances the precision of my estimates. Applying consistent sample restrictions, I ensure that these older cohorts are employed at age 59, do not claim disability benefits, and never opt for early retirement benefits. Further, I segment them into quintiles based on their pension wealth prior to retirement.

[18] In Appendix H I go through the derivations and discuss the assumptions.

In the administrative dataset, a direct measure of consumption is absent. However, drawing on Browning and Leth-Petersen (2003), I impute annual consumption as the difference between disposable income and the change in wealth stock. For a significant portion of the population, this serves as a reliable approximation of actual expenditures, as shown by Abildgren et al. (2020). It is essential to recognize the conventional caution: expenditures do not invariably equate to consumption.

I derive a fiscal externality of $FE = 0.31$ from Equation 3, indicating that for every dollar transferred from those retiring early to those retiring late, the government accrues an additional 31 cents in tax revenue. From Equation 6, I compute the consumption smoothing cost at $CS = 0.13$, which indicates that early retirees assign a higher social marginal value to consumption compared to their late-retiring counterparts. The resulting overall net welfare benefit, following Equation 2, from the stylized reform stands at $W = 0.18$. This means that for every dollar transferred from early to late retirees, society benefits by 18 cents. It is important to note that this number is not inherently positive, since the stylized reform is budget-neutral and does not factor in savings from reduced benefit disbursements.

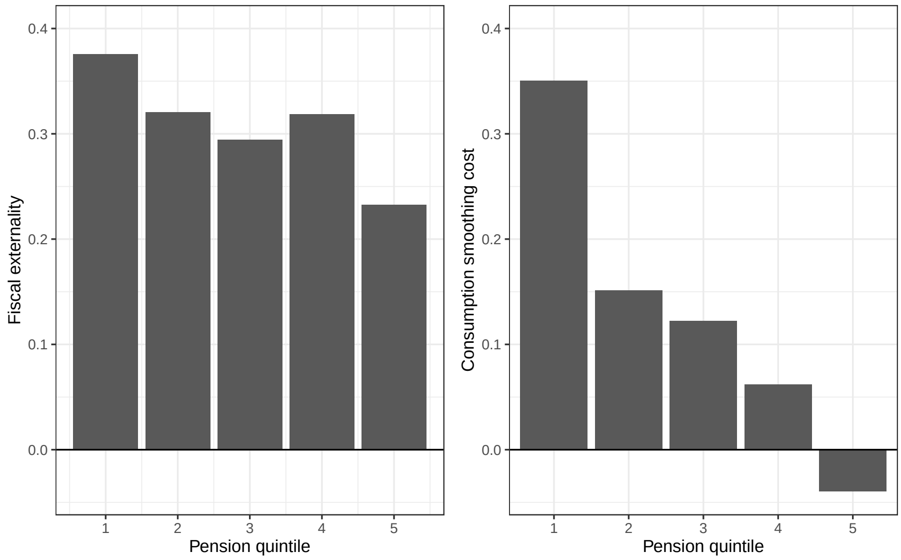
6.4 Welfare Effects Across Pension Wealth

I conduct welfare calculations separately across the five distinct pension wealth quintiles, presenting the outcomes in Figure 7. Panel (a) illustrates fiscal externalities, while Panel (b) reports consumption smoothing costs. Both metrics diminish with increasing pension wealth, with notable distributive implications.

When individuals with lower pension wealth have higher fiscal externalities, it means that their behavioral adjustments in response to the reform yield higher societal benefits than their counterparts with higher pension wealth. Note that this holds even with their reduced tax rates. The higher consumption smoothing cost for those in the lower pension wealth bracket implies a more significant consumption reduction retiring later, a contrast sharper than observed in individuals with higher pension wealth. Simply put, the more pension wealth, the lesser the reform's influence on consumption patterns.

Synthesizing these insights, it becomes evident that the behaviors of individuals with low pension wealth play a key role for the overall societal gains. However, they simultaneously incur a steeper personal cost. This pattern suggests that the reform intensifies inequality, in addition to its core objective of ensuring fiscal sustainability.

Figure 7: *Welfare effects by pension wealth quintiles*



Note: The left panel shows fiscal externality as per Equation 3 by pension wealth quintile; The right panel shows consumption smoothing cost as per Equation 6 by pension wealth quintile.

7 Conclusion

In this study, I explore the impact of a Danish reform, which raised the age threshold for social security eligibility, on retirement decisions. Such reforms are commonly implemented across various nations aiming for the long term fiscal sustainability of their pension systems. Through a regression discontinuity design over specific age thresholds, I present causal findings indicating that postponing the eligibility age leads individuals to retire later.

I document that individuals with low or no pension wealth retire later than people with high levels of pension wealth. This finding is consistent with conventional economic theories suggesting that greater pension wealth, all other factors being constant, would reduce the incentive to work longer.

Additionally, I find that the estimated effects of the reform for cohorts already affected by the policy are similar to the expected effects among younger cohorts who have not yet reached their eligibility age. In a novel survey, participants disclose their projected retirement age in various hypothetical contexts. The data implies that future shifts in the eligibility age will likely generate similar reactions as for the cohorts already affected, with individuals possessing lower pension wealth exhibiting more significant labor supply responses.

Finally, using the model from Kolsrud et al. (2024), I assess the overall welfare consequences of of postponing social security eligibility across different pension wealth brackets. Comparing the beneficial fiscal externality due to enhanced tax revenues with the consumption smoothing costs borne by individuals, I find that individuals with low pension wealth both are responsible for a large share of the the societal benefits but bear a heftier individual burden. In other words, postponing social security eligibility increases economic inequality.

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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. Table 2 shows how, for the four half-year cohorts in question (1953.5, 1954.0, 1954.5) this reduces the sample size from 95,000 to 30,000.

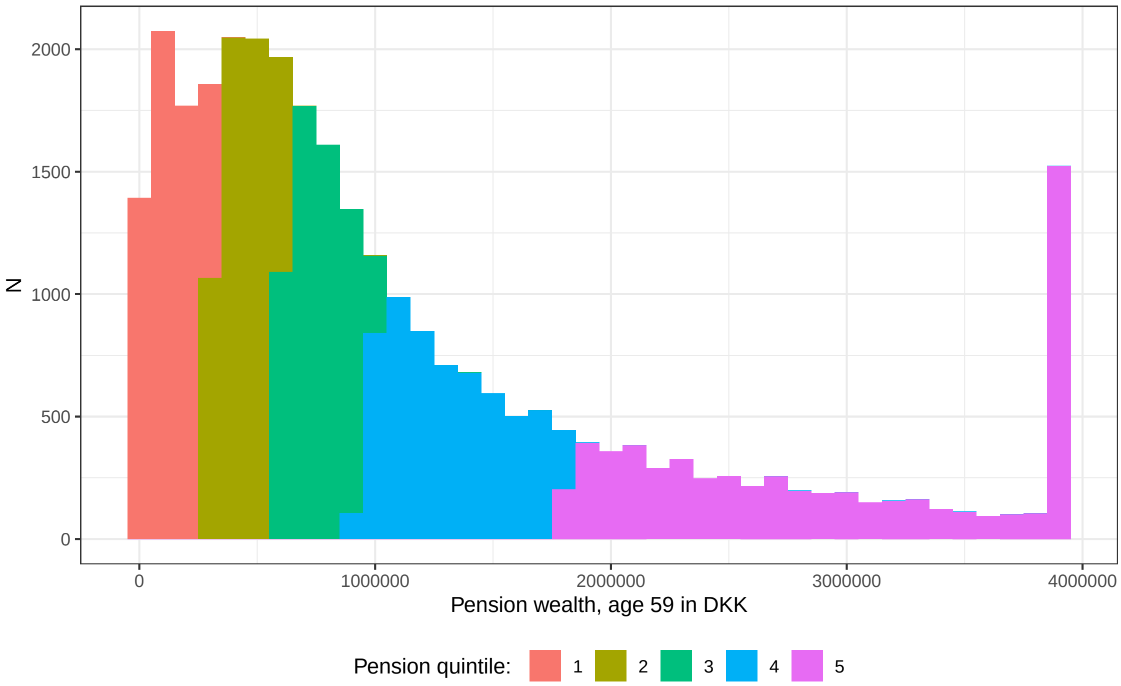
Table 2: *Sample size*

Sample criteria	N
Population	94,966
Native Danes	88,301
Working age 59	68,133
Not disabled	67,853
Not self-employed	66,625
Not on early retirement	30,431

Appendix B 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 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 8 reports the distribution for cohorts 1953.5, 1954.0, and 1954.5.

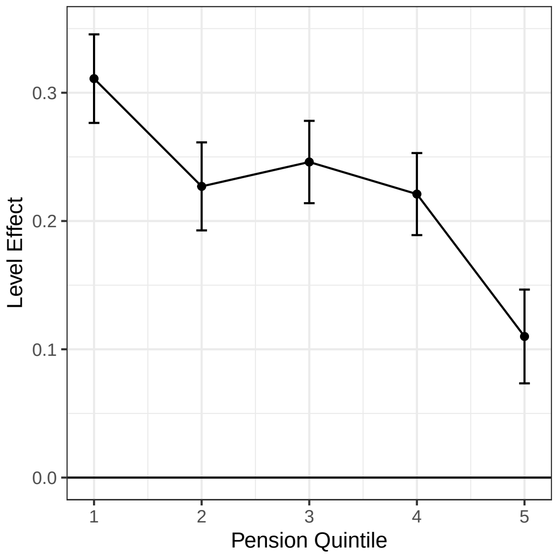
Figure 8: *Distribution of pension wealth, age 59*



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

One could argue that the reform in 2006 gave people amble time to adjust their savings behavior, as shown by García-Miralles and Leganza (2021). Importantly, they show that changes to savings are in liquid wealth and not in pension accounts. This is in line with the fact that most people are ”passive” savers, as documented by Chetty et al. (2014). I address this concern in two ways: Figure 9 shows the main result using imputed pre-reform pension wealth (2005-levels) to stratify the cohorts yielding qualitatively the same results as in Figure 4.

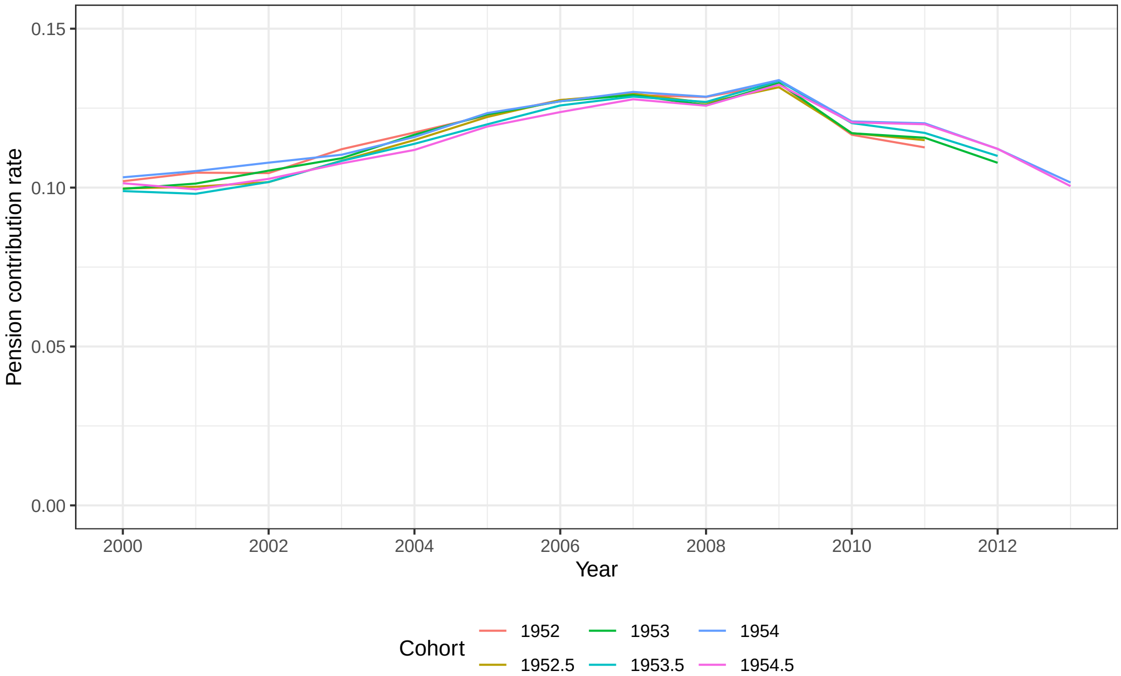
Figure 9: *Summary of RD results, administrative data, stratified by pre-reform wealth (2005)*



Note: The figure shows RD results from Equation 1 in the administrative data. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals. In this figure wealth quintile as determined using pension wealth in 2005 before the reform was announced.

Figure 10 shows that the historical contribution patterns from mid-40s to age 59 follow the same patterns across a range of cohorts (both cohorts unaffected by the reform and the considered cohorts 1953.5, 1954.0, and 1954.5). Annual fluctuations reflect both the general phase in of the system in the early 2000s and business cycle dynamics. Contribution rates are calculated as the annual cohort specific average over individual total pension savings divided by individual earnings.

Figure 10: *Contribution rates by cohort, 1952.0-1954.5*



Note: Cohort-year specific averages over individual pension contribution rates, measured as total pension savings divided by earnings.

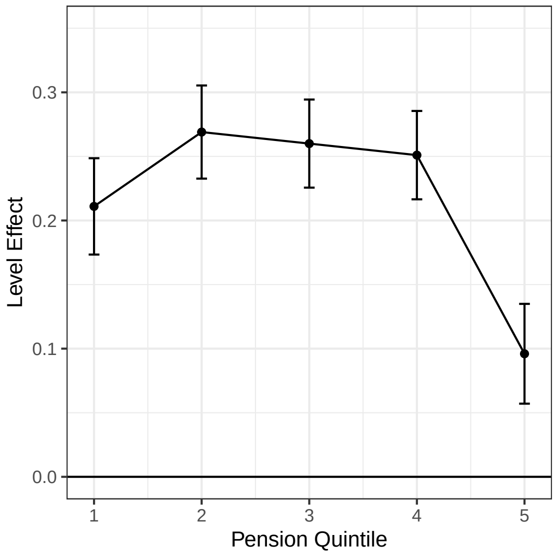
Appendix C Robustness Analysis

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 C.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 11 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 blue line 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 11: *Summary of RD results, administrative data, non-employment*

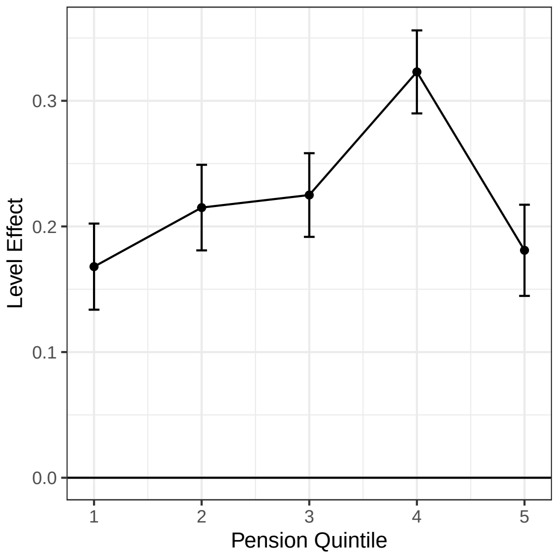


Note: The figure shows RD results from Equation 1 in the administrative data. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals. In this figure labor force participation is defined by employment status only.

Appendix C.2 Alternative Wealth Definitions, Administrative data

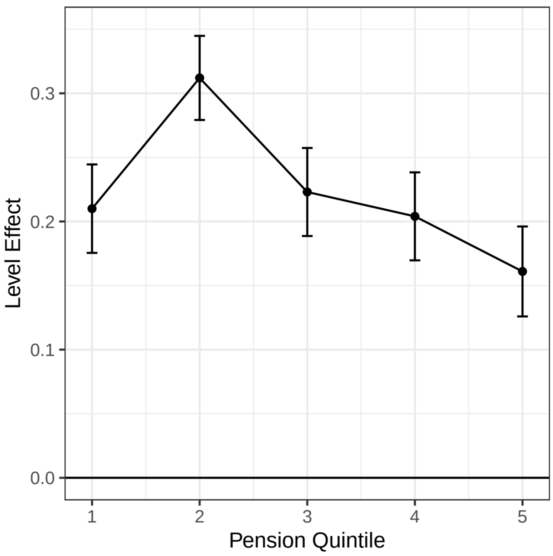
Most pension accounts are paid out as either fixed-term or indefinite annuities and as such pension wealth can be considered an income stream in retirement rather than an asset that can be accessed at will. This sets pension wealth apart from other types of wealth, e.g. liquid wealth and real estate. Figures 12 and 13 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.

Figure 12: *Summary of RD results, administrative data, stratified by liquid wealth*



Note: The figure shows RD results from Equation 1 in the administrative data. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals. In this figure wealth quintile as determined using liquid wealth at age 59.

Figure 13: *Summary of RD results, administrative data, stratified by total wealth*

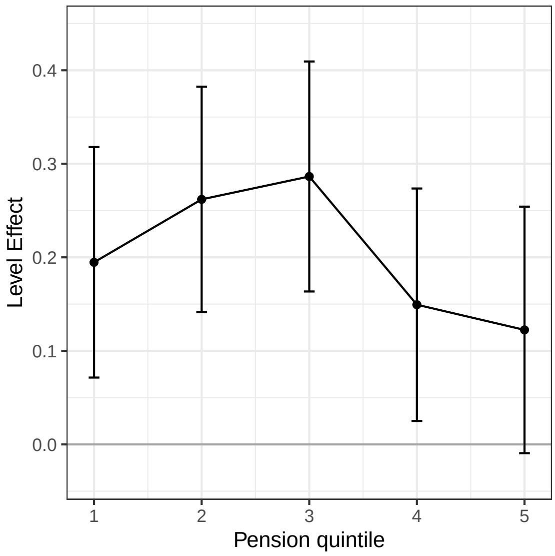


Note: The figure shows RD results from Equation 1 in the administrative data. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals. In this figure wealth quintile as determined using total wealth including real estate at age 59.

Appendix C.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 14.

Figure 14: *Summary of RD results, survey data, stratified by current pension wealth*

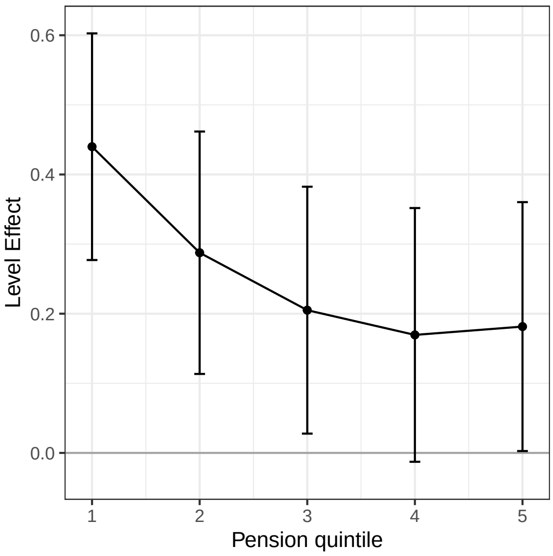


Note: The figure shows RD results from Equation 1 in the survey data. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals. In this figure wealth quintile as determined using pension wealth measured in 2020.

Appendix C.4 Smaller Bandwidth, Survey data

In the main analysis, I use two 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 15.

Figure 15: *Summary of RD results, survey data, only one cohort instead of two on either side*



Note: The figure shows RD results from Equation 1 in the survey data using only one cohort on either side of the cutoff. The x-axis shows the pension wealth quintiles and the y-axis show the estimate of the treatment dummy $\hat{\beta}_1$ with 95% confidence intervals.

Appendix D Copenhagen Life Panel Survey

I use answers to the Copenhagen Life Panel survey[19] 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[20]. 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*

[19] 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.

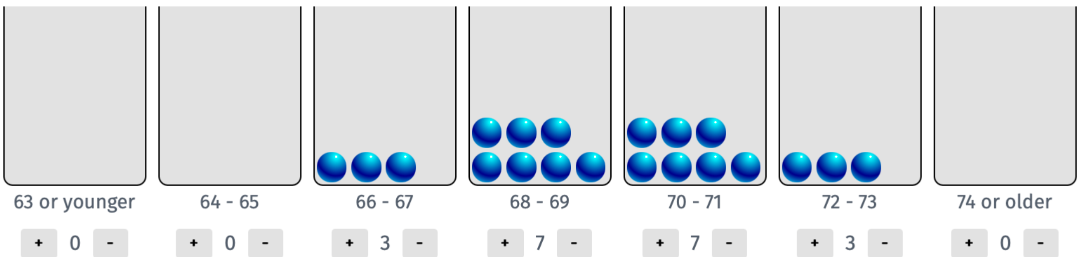
[20] For validation exercises see Caplin et al. (2022) that use the same survey wave.

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 16 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 16: *"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 D.1 Survey Data Balance Table

Table 3: *Balance table, survey data*

	Respondent	Non-respondent	Difference	p-value
N	5006	18796		
Female	0.5	0.494	0.006	0.447
	(0.5)	(0.5)	(0.008)	
Age	54.768	54.515	0.253	0
	(3.411)	(3.448)	(0.054)	
College	0.505	0.33	0.175	0
	(0.5)	(0.47)	(0.008)	
Employed	0.919	0.786	0.134	0
	(0.272)	(0.41)	(0.005)	
Earnings	505,021	386,956	118,065	0
	(360,972)	(387,674)	(5,833)	
Total wealth	531,604	453,628	77,975	0.004
	(1,530,873)	(2,201,550)	(26,945)	
Pension savings	2,141,693	1,672,916	468,776	0
	(1,829,145)	(1,739,611)	(28,799)	

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 DKK30,000 in 2020, *Earnings* is 2020 earnings in DKK, *Total wealth* includes real estate, *Pension savings* is total occupational and supplementary pension savings end of 2020.

Appendix D.2 Expected Retirement

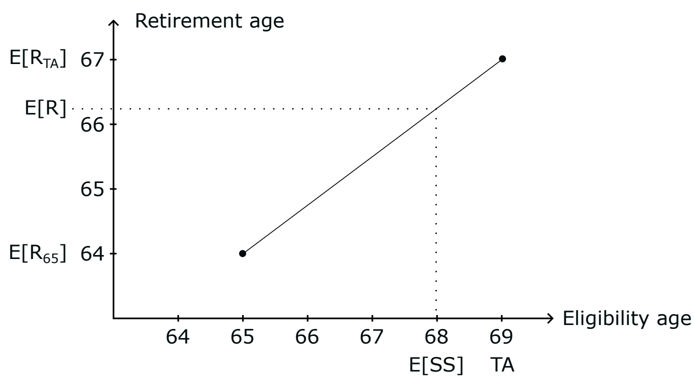
I derive the expected retirement age, $\mathbb{E}[R]$, using the expected social security eligibility age to interpolate between two hypothetical retirement scenarios. The metric of interest, the expected retirement age, $\mathbb{E}[R]$, is not directly elicited in the survey. In line with Caplin et 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 age 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 E Admin Balance Table

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

Table 4: *Balance table, admin data, control cohort 1953.5*

	Control	Treatment	Difference	p-value
N	9728	10635		
Female	0.399	0.4	0.001	0.921
	(0.49)	(0.49)	(0.007)	
College	0.623	0.613	-0.01	0.144
	(0.485)	(0.487)	(0.007)	
Earnings	458,165	454,452	-3,712	0.409
	(317,523)	(324,199)	(4,500)	
Total wealth	826,225	797,393	-28,832	0.454
	(2,350,327)	(3,116,955)	(38,489)	
Pension savings	1,698,540	1,696,856	-1,684	0.957
	(2,331,798)	(2,055,963)	(30,926)	

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 5: *Balance table, admin data, control cohort 1954.0*

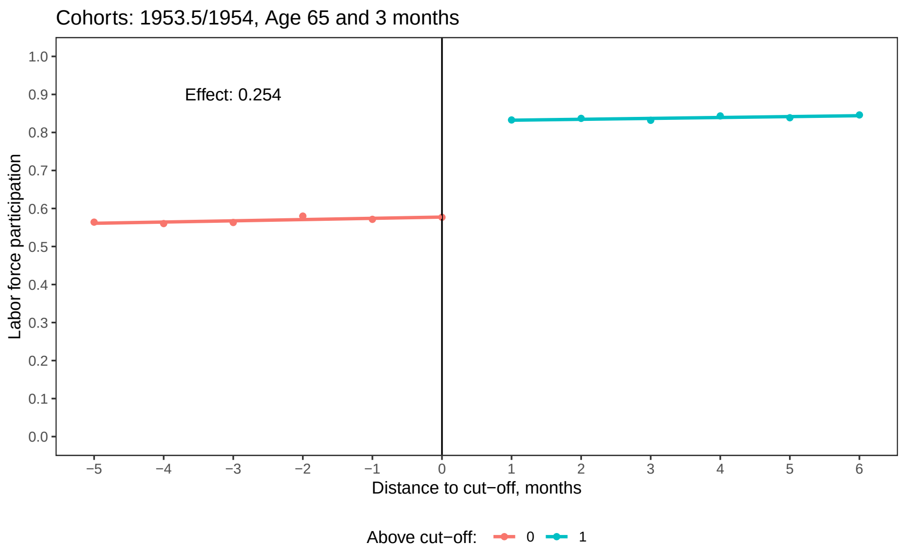
	Control	Treatment	Difference	p-value
N	10635	9790		
Female	0.4	0.403	0.004	0.586
	(0.49)	(0.491)	(0.007)	
College	0.613	0.623	0.011	0.122
	(0.487)	(0.485)	(0.007)	
Earnings	454,452	452,158	-2,295	0.657
	(324,199)	(405,794)	(5,167)	
Total wealth	797,393	1,256,014	458,620	0.312
	(3,116,955)	(44,809,967)	(453,888)	
Pension savings	1,696,856	1,688,831	-8,025	0.798
	(2,055,963)	(2,397,353)	(31,377)	

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.

Appendix F Additional Admin Results

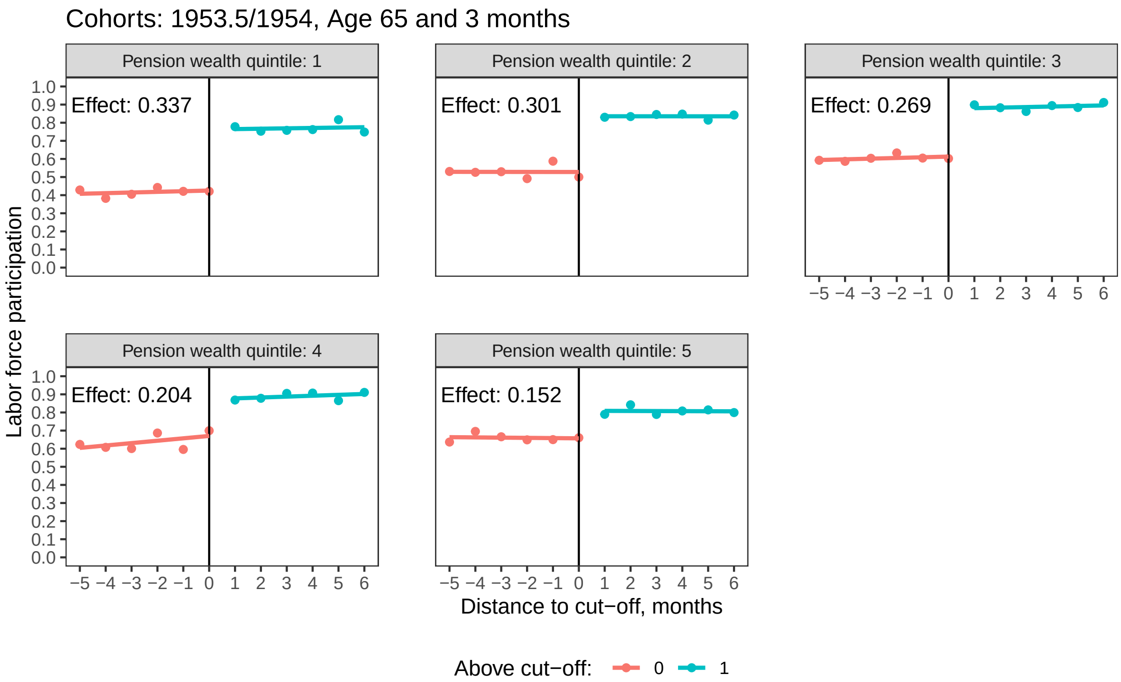
Figure 17 illustrates the results for the inaugural age cutoff: cohort 1953.3 (red) is on the left, and cohort 1954.0 (blue) on the right. The y-axis depicts the proportion not retired at age 65 and 3 months. The running variable signifies the interval in months from the birth date to January 1st, 1954. A distinct jump at the cutoff reveals that individuals from cohort 1954.0 exhibit a 25.4 percentage point higher likelihood to retire; this tendency remains uninfluenced by the distance to the cutoff. The analysis recurs for the 2nd age cutoff centered on cohorts 1954.0 and 1954.5 and the RD result for cutoff at ages 65.0 and 65.5 are shown, both in the aggregate and by pension wealth.

Figure 17: *RD results, 65.0 cutoff, administrative data*



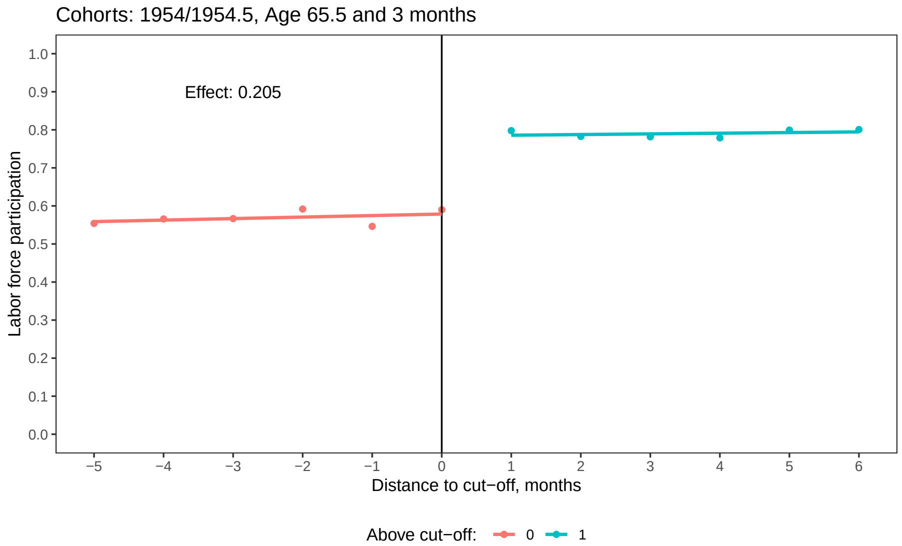
Note: Results for the cutoff at age 65.0. The figure shows RD estimates from Equation 1 for the two cohorts 1953.5 and 1954.0 in the administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

Figure 18: *RD results, 65.0 cutoff, by pension wealth, administrative data*



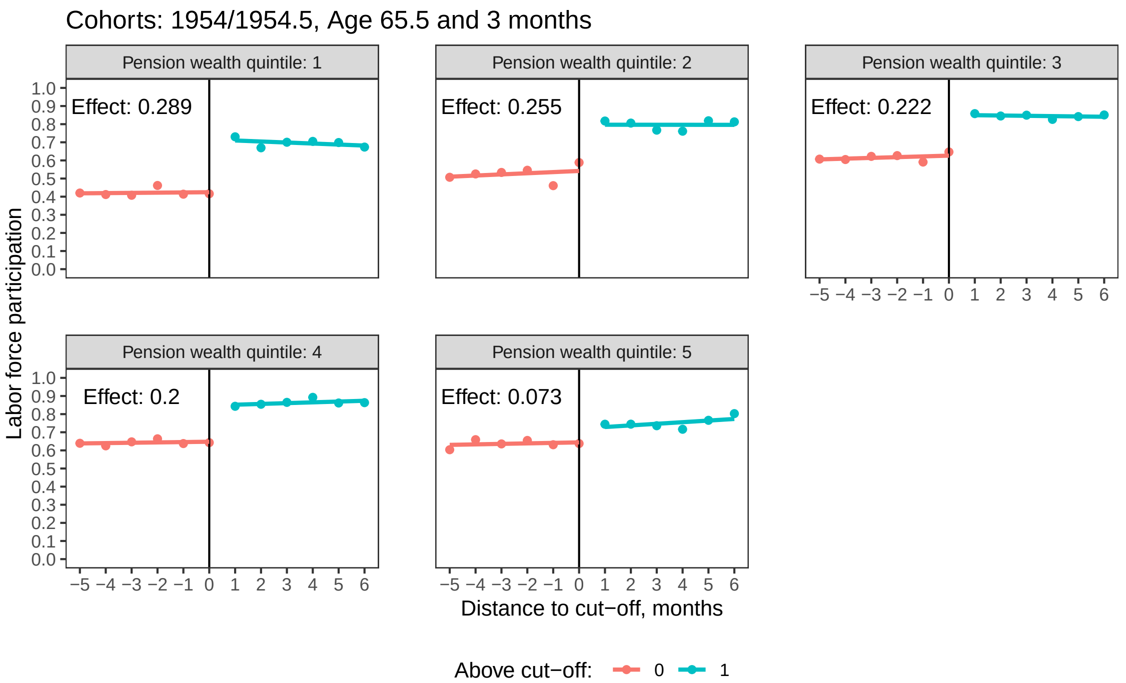
Note: Results for the cutoff at age 65.0. The figure shows RD estimates from Equation 1 for the two cohorts 1953.5 and 1954.0 in the administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Figure 19: *RD results, 65.5 cutoff, administrative data*



Note: Results for the cutoff at age 65.5. The figure shows RD estimates from Equation 1 for the two cohorts 1954.0 and 1954.5 in the administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

Figure 20: *RD results, 65.5 cutoff, by pension wealth, administrative data*

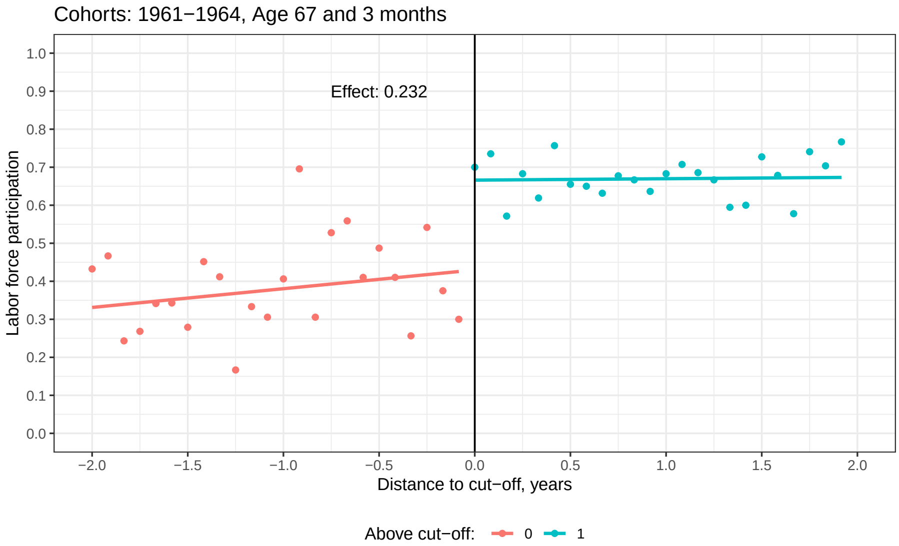


Note: Results for the cutoff at age 65.5. The figure shows RD estimates from Equation 1 for the two cohorts 1954.0 and 1954.5 in the administrative data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Appendix G Additional Survey Results

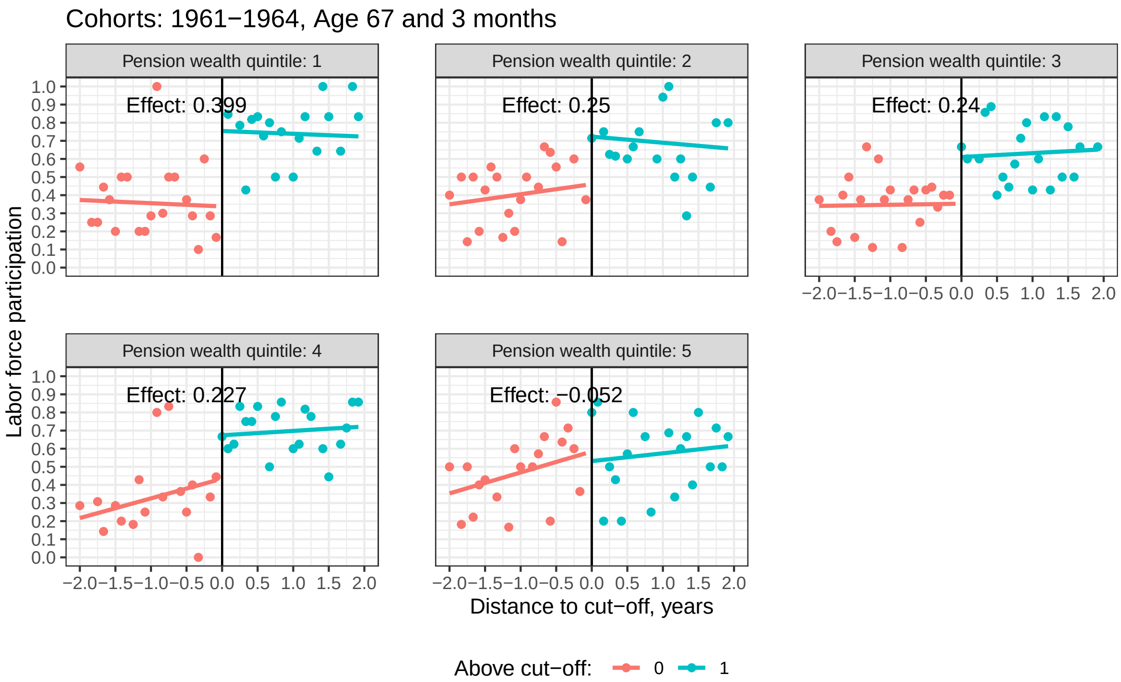
The following pages show the RD results for each age cutoff in the survey, both in the aggregate and by pension wealth.

Figure 21: *RD results, 67 cutoff, survey data*



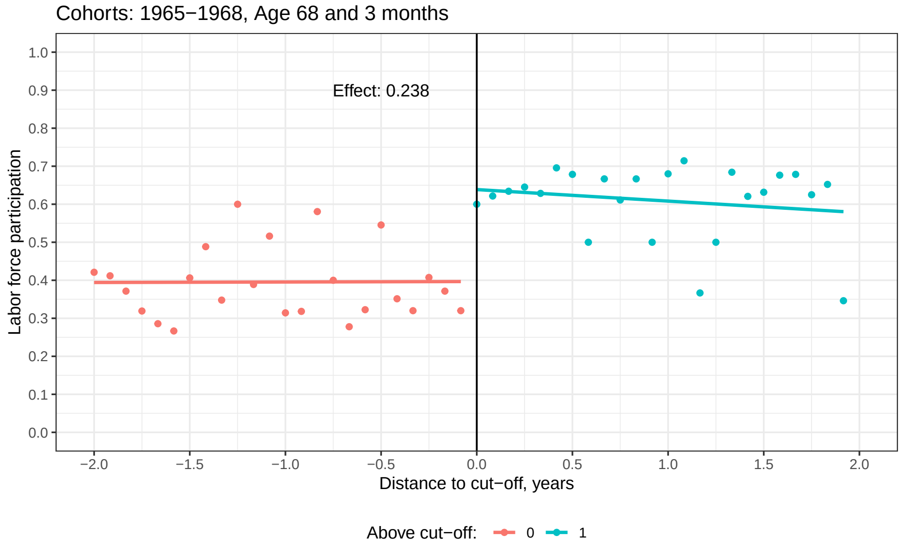
Note: Results for the cutoff at age 67. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

Figure 22: *RD results, 67 cutoff, by pension wealth, survey data*



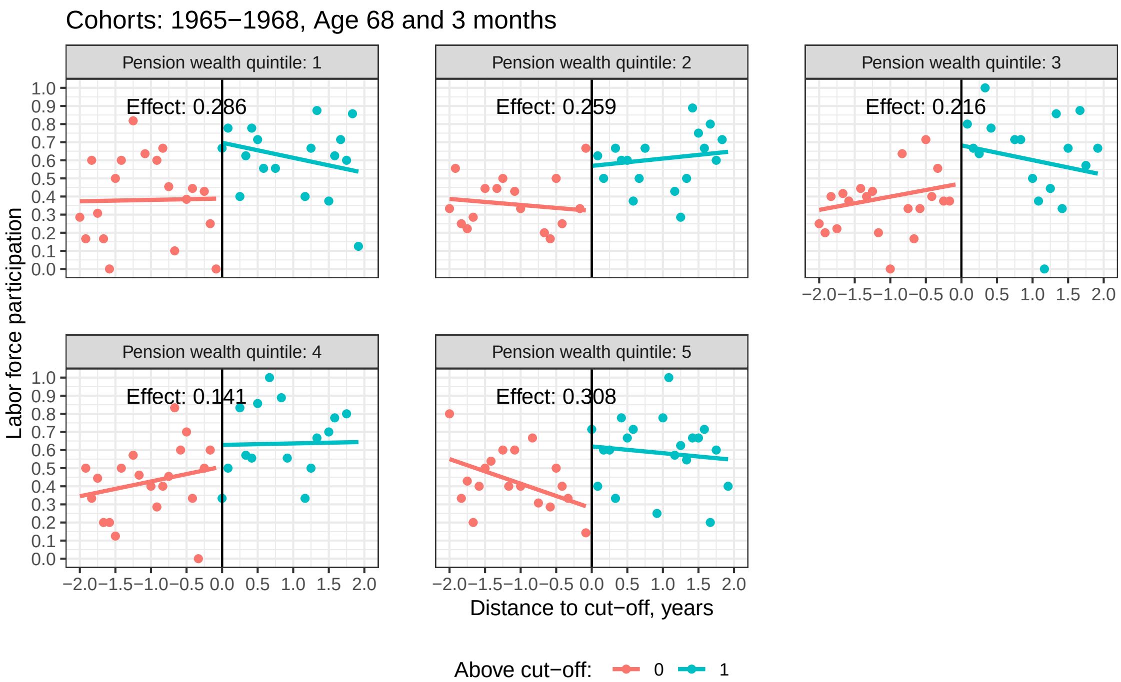
Note: Results for the cutoff at age 67. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Figure 23: *RD results, 68 cutoff, survey data*



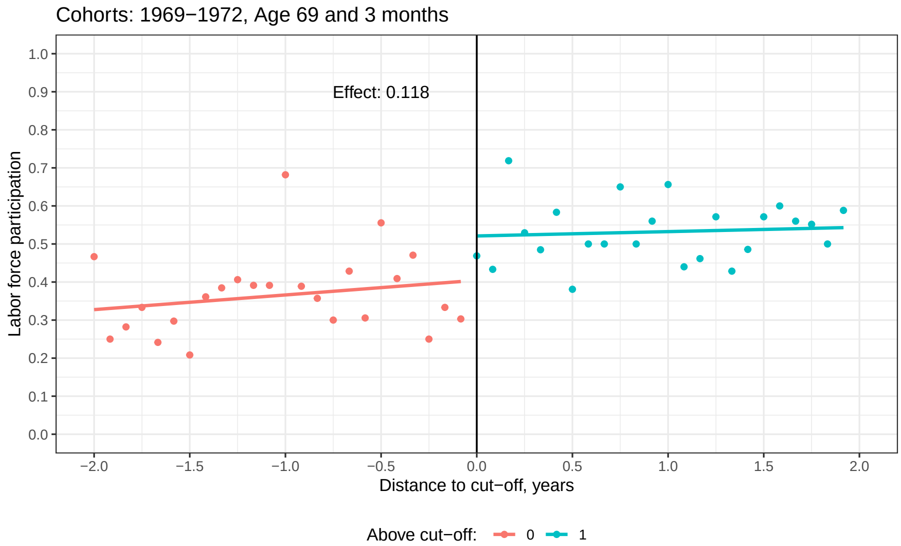
Note: Results for the cutoff at age 68. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

Figure 24: *RD results, 68 cutoff, by pension wealth, survey data*



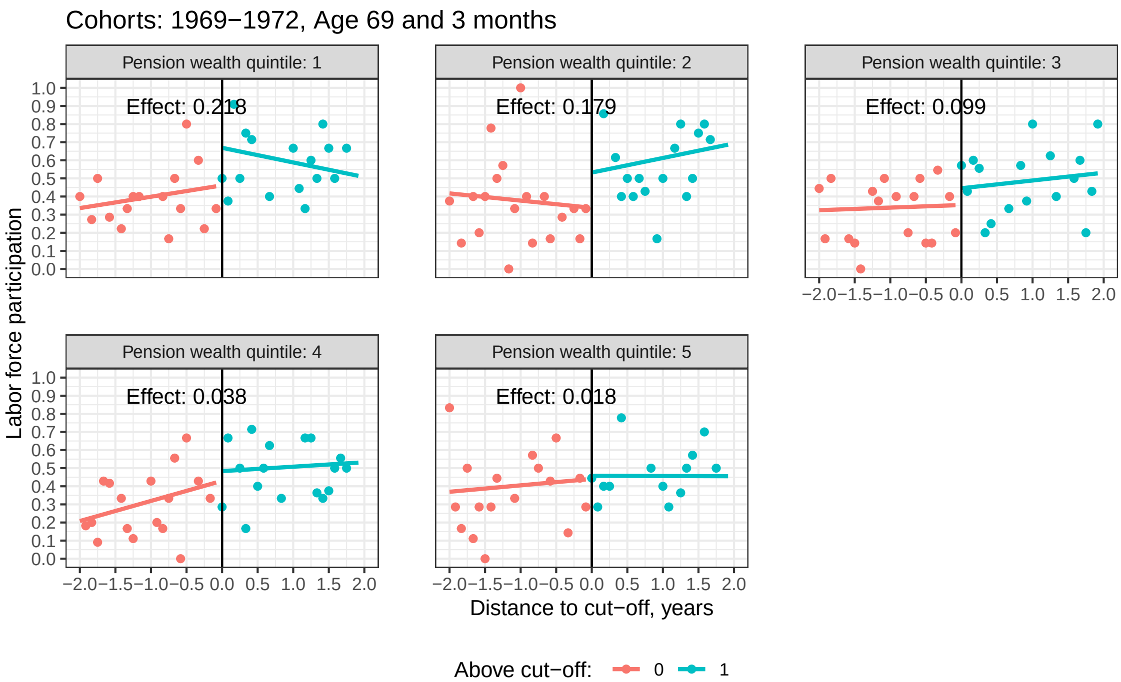
Note: Results for the cutoff at age 68. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Figure 25: *RD results, 69 cutoff, survey data*



Note: Results for the cutoff at age 69. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold.

Figure 26: *RD results, 69 cutoff, by pension wealth, survey data*

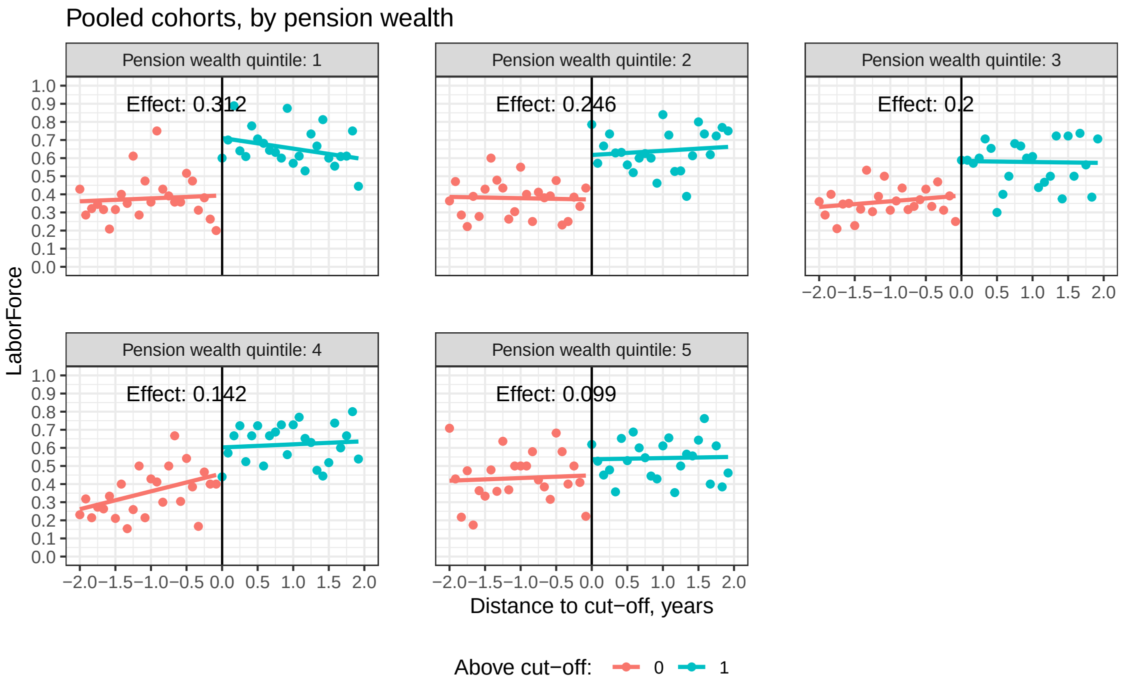


Note: Results for the cutoff at age 69. The figure shows RD estimates from Equation 1 for the two cohorts on either side of the cutoff in the survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Appendix G.1 Survey results, pooled

Pooled results for all three cutoffs by pension wealth quintile. The effect sizes correspond to Figure 6b in the main document.

Figure 27: *RD results, pooled cutoffs, by pension wealth, survey data*



Note: The figure shows RD estimates from Equation 1 for the pooled survey data.. The x-axis shows distance to cutoff in months, left side is the older cohort not affected by the reform. The y-axis is the labor force participation defined as share of people not yet retired measured 3 months after the threshold. Each panel shows the estimates separately for each pension wealth quintile.

Appendix H Welfare Model Setup

Appendix H.1 Model

The model and the derivations are taken from Kolsrud et al. (2024). Individual i, age t, state $\pi_{i,t}$, consumption $c_{\pi_{i,t}}$, other choices/characteristics $\zeta_{\pi_{i,t}}$ (which can depend flexibly on entire history).

$U_i(c,\zeta,\pi)$ is utility, $a_{i,t}$ is assets, $R(\pi_{i,t})$ real interest rate, and $y(\pi_{i,t})$ is income equal to earnings minus taxes $w(\pi_{i,t}) - \tau(\pi_{i,t})$ when working ($s(\pi_{i,t}) = 1$) and equal to benefits $b(\pi_{i,t})$ when not working ($s(\pi_{i,t}) = 0$). The following equations describe the model:

$$\begin{aligned} U_i(c,\zeta,\pi) &= \sum_{t=0}^{T} \beta^t \int u(c(\pi_{i,t}), \zeta(\pi_{i,t}))dF(\pi_{i,t}) \\ a_{i,t+1}(\pi_{i,t}) &= R(\pi_{i,t})\left[a_{i,t}(\pi_{i,t-1}) + y(\pi_{i,t}) - c(\pi_{i,t})\right] \\ y(\pi_{i,t}) &= \begin{cases} w(\pi_{i,t}) - \tau(\pi_{i,t}) & \text{, if } s(\pi_{i,t}) = 1 \\ b(\pi_{i,t}) & \text{, if } s(\pi_{i,t}) = 0 \end{cases} \end{aligned} \tag{7}$$

Given $\pi_{i,t}$, then indirect utility $\mathcal{U}_i(b,\tau)$ governs the choice, s, to retire at age r.

Individuals are grouped by age at retirement, r, and homogeneity within group is assumed, that is $c(\pi_{i,t}) = c_{r,t}$ and $\zeta(\pi_{i,t}) = \zeta_{r,t}$ hold when $r(\pi_{i,t}) = r$.

The government is represented as maximizing aggregate welfare under a budget constraint:

$$\begin{aligned} W(b,\tau) &= \int_i \omega_i \mathcal{U}_i(b,\tau) + \lambda GBC(b,\tau)di \\ GBC(b,\tau) &= \sum_r \left[S(r)\frac{\tau_r}{R^r} + \left[S(r-1) - S(r)\right]NPV_r\right] - G_0 \end{aligned} \tag{8}$$

where NPV_r is net present value of benefits $b_{r,t}$ and G_0 is a fixed cost.

Appendix H.2 Retirement Reform

A stylized reform that reallocates resources from early (retiring at age r) to late (retiring at age r') retirees, will affect the average social marginal utility (SMU) of individuals and induce a behavioral response affecting tax revenue, denoted the fiscal externality (FE).

Assuming $\beta R = 1$ and that labor supply varies along the extensive margin, the following first order condition holds:

$$\underbrace{\mathbb{E}\left[\omega_i \frac{\partial u(c_{r,t},\zeta_{r,t})}{\partial c}\middle| r_i = r\right]}_{SMU} = \lambda\left[1 + \underbrace{\sum_{r'}\left[[\tau_{r'} - (NPV_{r'+1} - NPV_{r'})]\frac{\partial(1-S(r'))}{\partial b_{r,t}}\frac{1}{S(r)}\right]}_{FE}\right] \tag{9}$$

with $S(r)$ is the survival rate at age r.

The left hand side is the social marginal utility of transferring a dollar to individuals with retirement age r and where ω_i is an individual welfare weight. The effect is only affected by changes in consumption since the envelope theorem gives us that behavioral responses are second order and do not affect individual marginal utility, SMU.

Conversely the behavioral responses do affect fiscal externality FE on the right hand side through $\frac{\partial(1-S(r'))}{\partial b_{r,t}}$ along with government revenue $\tau_{r'} - (NPV_{r'+1} - NPV_{r'})$. FE is scaled by the marginal value of public funds, λ.

Optimality requires that for all r, r' the following holds:

$$\frac{SMU_{r,t}}{SMU_{r',t}} = \frac{1 + FE_{r,t}}{1 + FE_{r',t}} \tag{10}$$

Defining the average tax as $T_{r'} = \tau_{r'} - (NPV_{r'+1} - NPV_{r'})$ and omitting the age subscripts, Equation 9 can be expressed as:

$$\begin{aligned} SMU_r &= \lambda\left[1 + FE_r\right] \\ &= \lambda\left[1 - \sum_{r'} T_{r'}\frac{\partial(S(r'))}{\partial b_r}\frac{1}{S(r)}\right] \end{aligned} \tag{11}$$

Appendix H.3 Welfare Effects

Consider a budget neutral reform at age $\tilde{r}$ with two groups: those retire after age $\tilde{r}$ and those who retire earlier. The total welfare change is given as the left hand side minus the right hand side of Equation 11 for both groups, multiplied by their respective benefits. Budget neutrality ensures that changes to the benefit system are given by:

$$\begin{aligned} db_r &= db_{r>\tilde{r}},\ for\ r > \tilde{r} \\ db_r &= db_{r\leq\tilde{r}},\ for\ r \leq \tilde{r} \\ db_{r>\tilde{r}} &= -\frac{1-S(\tilde{r})}{S(\tilde{r})}db_{r\leq\tilde{r}} \end{aligned} \tag{12}$$

Total change in welfare can be formulated the as sum of the change in benefits, db, times the change in welfare, $SMU - \lambda [1 + FE]$, for both early and late retirees, weighted by their proportions ($(1 - S)$ and S):

$$\begin{aligned} dW =& db_{r \le \tilde{r}} (1 - S(\tilde{r})) [SMU_{r \le \tilde{r}} - \lambda [1 + FE_{r \le \tilde{r}}]] + \\ & db_{r > \tilde{r}} S(\tilde{r}) [SMU_{r > \tilde{r}} - \lambda [1 + FE_{r > \tilde{r}}]] \\ =& db_{r > \tilde{r}} S(\tilde{r}) [SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \\ & \lambda [FE_{r \le \tilde{r}} + FE_{r > \tilde{r}}] \\ =& db_{r > \tilde{r}} S(\tilde{r}) [SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \\ & \lambda \left[\sum_{r'} T_{r'} \left[\frac{\partial S(r')}{\partial b_{r \le \tilde{r}}} db_{r \le \tilde{r}} + \frac{\partial S(r')}{\partial b_{r > \tilde{r}}} db_{r > \tilde{r}} \right] \right] \end{aligned} \tag{13}$$

Next, Kolsrud et al. (2024) make the following three assumptions:

- for any $\tilde{r}$, $\frac{\partial (S(r))}{\partial b_{r \le \tilde{r}}} \approx 0$ for $r > \tilde{r}$; and $\frac{\partial (S(r))}{\partial b_{r > \tilde{r}}} \approx 0$ for $r \le \tilde{r}$
- for any $\tilde{r}$, $\sum_{r' \le \tilde{r}} \frac{\partial S(r')}{\partial b_{r \le \tilde{r}}} \approx \sum_{r' > \tilde{r}} \frac{\partial S(r')}{\partial b_{r > \tilde{r}}} \frac{1 - S(\tilde{r})}{S(\tilde{r})}$ and $T_{\tilde{r}} \approx T$
- for any $\tilde{r}$, $-\frac{\partial S(\tilde{r})}{\partial b_{r \le \tilde{r}}} = \frac{\partial S(\tilde{r})}{\partial b_{r > \tilde{r}}} \approx \frac{\partial S(\tilde{r})}{\partial w_{\tilde{r}}}$

allowing them to rewrite Equation 13 into:

$$\begin{aligned} dW =& db_{r > \tilde{r}} S(\tilde{r}) [SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \lambda T_{\tilde{r}} \frac{\partial S(\tilde{r})}{\partial w_{\tilde{r}}} [db_{r > \tilde{r}} - db_{r \le \tilde{r}}] \\ =& db_{r > \tilde{r}} S(\tilde{r}) \left([SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \lambda T_{\tilde{r}} \frac{\partial S(\tilde{r})}{\partial w_{\tilde{r}}} \frac{1}{S(\tilde{r})} \left[1 - \frac{db_{r \le \tilde{r}}}{db_{r > \tilde{r}}} \right] \right) \\ =& db_{r > \tilde{r}} S(\tilde{r}) \left([SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \lambda \frac{T_{\tilde{r}}}{w_{\tilde{r}}} \left[\varepsilon_{S(\tilde{r}), w_{\tilde{r}}} - \varepsilon_{1 - S(\tilde{r}), w_{\tilde{r}}} \right] \right) \\ =& db_{r > \tilde{r}} S(\tilde{r}) \left([SMU_{r > \tilde{r}} - SMU_{r \le \tilde{r}}] + \lambda \frac{T_{\tilde{r}}}{w_{\tilde{r}}} \varepsilon_{\frac{S(\tilde{r})}{1 - S(\tilde{r})}, w_{\tilde{r}}} \right) \end{aligned} \tag{14}$$

To arrive at a monetary metric for the welfare change of allocating one dollar from early retirees ($r \le \tilde{r}$) to late retirees ($r > \tilde{r}$), they further assume $SMU_{NRA} = \lambda$:

$$\begin{aligned} \Delta W =& \frac{dW / [db_{r > \tilde{r}} S(\tilde{r})]}{SMU_{NRA}} \\ =& \frac{T_{\tilde{r}}}{w_{\tilde{r}}} \varepsilon_{\frac{S(\tilde{r})}{1 - S(\tilde{r})}, w_{\tilde{r}}} - \frac{SMU_{r \le \tilde{r}} - SMU_{r > \tilde{r}}}{SMU_{NRA}} \end{aligned} \tag{15}$$

The two terms in Equation 15 are referred to as fiscal externality, FE, and consumption smoothing cost, CS, in the main text, where all metrics are presented without subscripts for legibility.

Appendix I Fiscal Externality

The fiscal externality, FE, captures the effects of the behavioral response to transferring one dollar from early retirees to to late retirees: some workers will continue to work post the old age cutoff, and this will generate added tax revenue. I consider a budget-balanced reform and small transfers, $db_{r\leq\tilde{r}}$ from individuals retiring at age $r \leq \tilde{r}$ to individuals retiring at age $r > \tilde{r}$ (they receive $db_{r>\tilde{r}}$).

The relationship between the amounts is governed by the average tax rate and the behavioral response, ensuring budget neutrality, as stated in Equation 12. $S(\tilde{r})$ is the share of people retiring at age $\tilde{r}$, and $1 - S(\tilde{r})$ is the share who retired prior to $\tilde{r}$.

To arrive at the first term in Equation 15, I need to estimate both the average tax rate and the log odds ratio elasticity $\varepsilon_{\frac{S(\tilde{r})}{1-S(\tilde{r})},w_{\tilde{r}}}$.

Appendix I.1 Estimating Elasticities

In the main text the labor supply is stated as ε, but what the model needs is the log odds ratio elasticity. I obtain it using the reform variation to first estimate the labor supply elasticity:

$$\varepsilon_{S(\tilde{r}),w_{\tilde{r}}}$$

and then follow the procedure in Kolsrud et al. (2024) and multiply by $1 + \frac{S(\tilde{r})}{1-S(\tilde{r})}$ to make it the log odds ratio elasticity needed for Equation 15:

$$\varepsilon_{\frac{S(\tilde{r})}{1-S(\tilde{r})},w_{\tilde{r}}} = \varepsilon_{S(\tilde{r}),w_{\tilde{r}}}\left[1 + \frac{S(\tilde{r})}{1-S(\tilde{r})}\right] \tag{16}$$

I estimate the labor supply elasticity, $\varepsilon_{S(\tilde{r}),w_{\tilde{r}}}$, following Laun (2017) by regressing labor supply (at the extensive margin) $P_{i,t}$ on the net-of-participation tax rate $(1 - \tau_{i,t}^{A})$, controlling for year fixed effects, δ_t, and month of birth, μ_a, and $u_{i,t}$ being an error term:

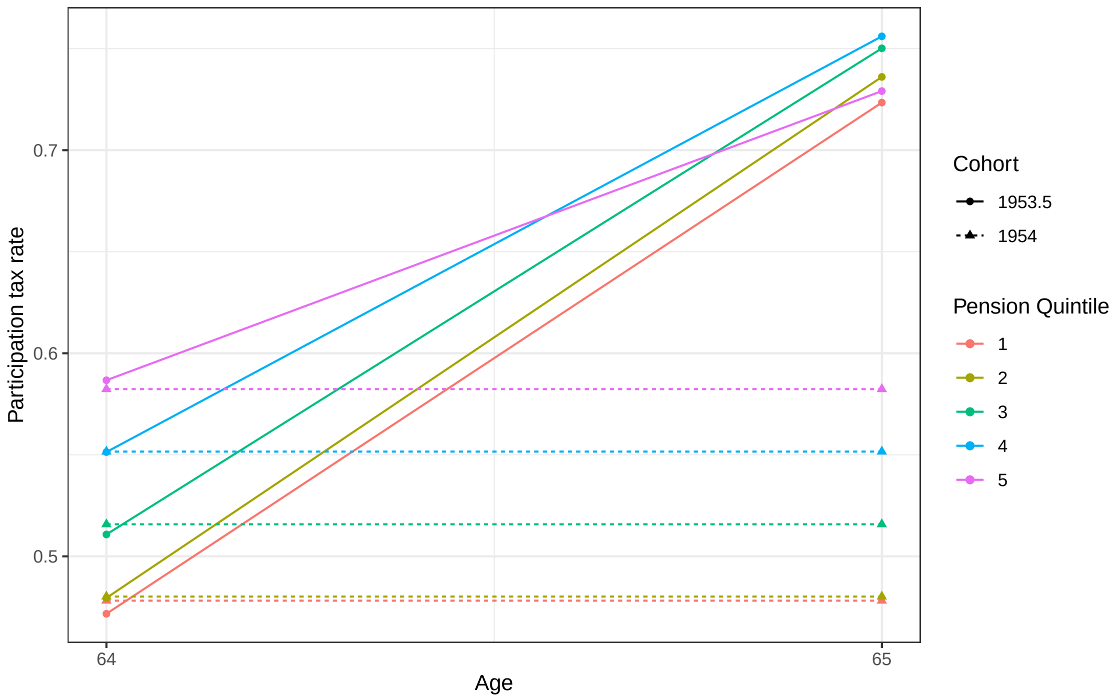
$$P_{i,t} = \varepsilon_{S(\tilde{r}),w_{\tilde{r}}} \log(1 - \tau_{i,t}^{A}) + \delta_t + \mu_a + u_{i,t} \tag{17}$$

I use an IV approach and instrument $\log(1-\tau_{i,t}^{A})$ using a binary variable which assumes a value of 1 post the age cutoff for the control group and 0 otherwise. Under the assumption of parallel trends where we expect identical behavior between the two cohorts in the absence of the reform the instrument should correlate with $\log(1-\tau_{i,t}^{A})$, but not with other influencing factors. The individual net-of-participation tax rate is given by Equation 5.

Appendix J Hedonic Regressions and Auxiliary Results

I only observe individuals in my sample in either a working or a non-working state post retirement, so to get a consistent estimate of their income in both states I use a different set of older cohorts (1948-53) and regress income at age 66 on a set of observables, to be able to predict disposable income and gross earnings for my main sample in both states. This allows me to calculate individual net-of-participation tax rates needed for Equation 17. The average tax rates by control/treatment and below/above cutoff are shown in Figure 28, and the picture is clear: those eligible for social security (cohort 1953.5 at age 65) face higher participation tax rates due to foregone transfers if they work. Due to the progressive Danish tax system, high wealth individuals face higher participation tax rates.

Figure 28: *Participation tax rates*

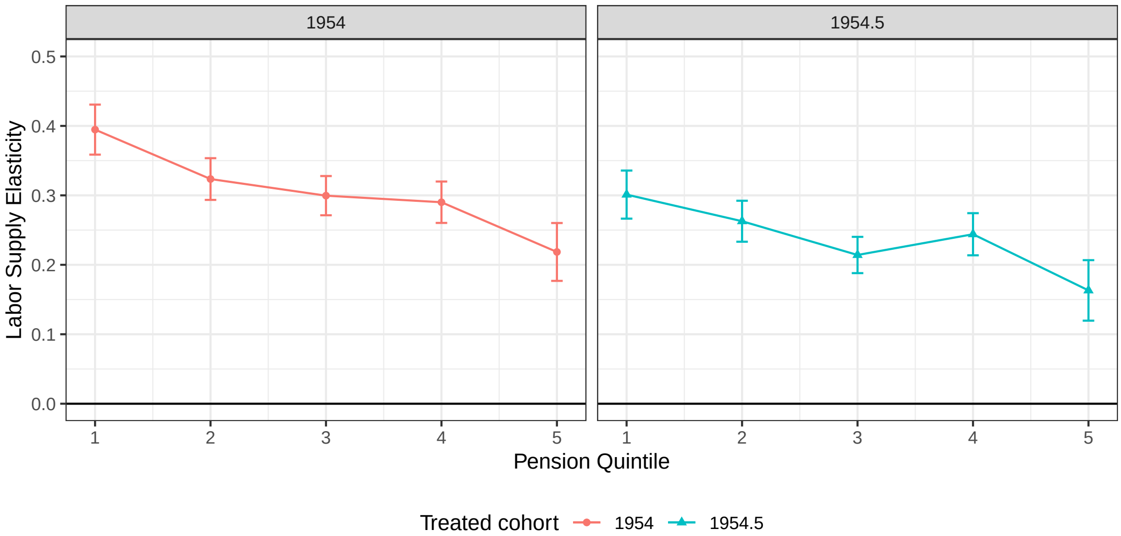


Note: Participation tax rates are calculated using Equation 5 separately for cohorts 1953.5 and 1954.0 by pension wealth quintile.

Figure 29 shows the estimated log odds ratio elasticities by pension wealth quintiles. The elasticities resemble the causal effect sizes estimated in Section 5 (summarized in Figure 4) and display the same behavior, decreasing in pension wealth.

Taken together, the average tax rates and log odds ratio elasticities combine into the fiscal externality shown in the main text, Panel A of Figure 7.

Figure 29: *Estimated log odds ratio elasticities, by pension wealth and cutoff*



Note: Log odds ratio elasticities are estimated using Equations 16 and 17.

Appendix K Consumption Smoothing Cost and SMU

Following Chetty and Finkelstein (2013), Kolsrud et al. (2024)[21] proceed with a Taylor expansion of marginal utility that allows for mapping changes in consumption into social marginal utility:

$$\frac{\partial u(c_{r,post},\zeta_{r,t})}{\partial c} \approx \frac{\partial u(c_{r,pre},\zeta_{r,t})}{\partial c}\left[1 - \frac{-\frac{\partial^2 u(c_{r,pre},\zeta_{r,t})}{\partial c^2} c_{r,pre}}{\frac{\partial u(c_{r,pre},\zeta_{r,t})}{\partial c}} \frac{c_{r,post} - c_{r,pre}}{c_{r,pre}}\right] \tag{18}$$

By assuming a utility function that is CRRA in consumption with risk aversion γ, the second term reduces to $1+\gamma\frac{c_{r,pre}-c_{r,post}}{c_{r,pre}}$. Under the stated assumptions we can now express a ratio of SMU's as:

$$\begin{aligned}
\frac{SMU_{r,t}}{SMU_{r',t}} &= \frac{\mathbb{E}\left[\omega_i \frac{\partial u(c_{r,t},\zeta_{r,t})}{\partial c}\middle| r_i = r\right]}{\mathbb{E}\left[\omega_i \frac{\partial u(c_{r,t},\zeta_{r,t})}{\partial c}\middle| r_i = r'\right]} \\
&= \frac{\omega_r \frac{\partial u(c_{r,pre},\zeta_{r,t})}{\partial c}}{\omega_{r'} \frac{\partial u(c_{r',pre},\zeta_{r',t})}{\partial c}} \frac{1+\gamma\frac{c_{r,pre}-c_{r,post}}{c_{r,pre}}}{1+\gamma\frac{c_{r',pre}-c_{r',t}}{c_{r',pre}}} \\
&= \frac{1+\gamma\frac{c_{r,pre}-c_{r,post}}{c_{r,pre}}}{1+\gamma\frac{c_{r',pre}-c_{r',t}}{c_{r',pre}}}
\end{aligned} \tag{19}$$

Second equality below comes from Equation 18, and by assuming constant welfare weights, ω_i, and marginal utility being the same for different r conditional on consumption we get the third equality.

I define the average within-person difference in consumption over all those who retire at age r as

$$\Delta\bar{c}_r = \frac{1}{N_r}\sum_r \Delta c_i = \frac{1}{N_r}\sum_r \frac{c_{i,pre} - c_{i,post}}{c_{i,pre}} \tag{20}$$

Consumption smoothing costs (second term in Equation 15) can then be expressed following Equation 19 as:

$$CS = \frac{SMU_{r\leq\tilde{r}} - SMU_{r>\tilde{r}}}{SMU_{NRA}} = \frac{\gamma\Delta\bar{c}_{r\leq\tilde{r}} - \gamma\Delta\bar{c}_{r>\tilde{r}}}{1+\gamma\Delta\bar{c}_{NRA}} \tag{21}$$

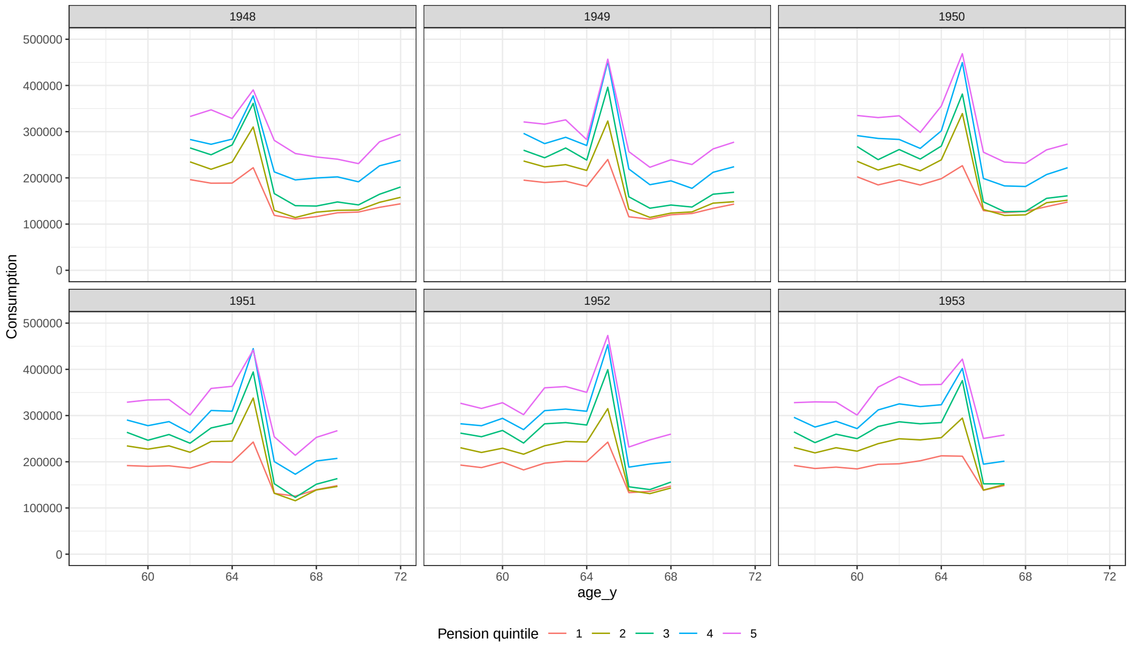
In line with Kolsrud et al. (2024) I use risk aversion $\gamma = 4$. The results are not sensitive to varying this parameter.

[21] The implementation I use presently is the one denoted Δ*Consumption drop*, where the individual drop in consumption around retirement is considered.

Appendix K.1 Measure of Consumption

Following Browning and Leth-Petersen (2003), I impute annual consumption as the difference between disposable income and the change in wealth stock. In line with the literature started by Banks et al. (1998), I find a significant consumption drop around retirement. Figure 30 is meant as an illustration to show how mean consumption exhibits a drop around the retirement age 65 as the levels before and after clearly differ. This is true across cohorts (each panel is a different cohort) and pension wealth (the colors denote quintiles of pension wealth pre-retirement). The positive spikes exactly at age 65 are mainly due to one-time payouts at retirement.

Figure 30: *Mean consumption around retirement, by pension wealth and cohort*



Note: Average consumption around retirement, calculated by pension wealth quintile (colors) and cohort (panels).

In the analysis I compute individual consumption changes, Δc_i, as differences in mean consumption after (years $t+1, t+2, t+3$) and before (years $t-3, t-2, t-1$), where t is the individual retirement age. I then aggregate the changes in consumption to all those retiring at age 65 or before vs. those retiring after using Equation 20, spilt by pension wealth quintile pre-retirement. Finally, the consumption smoothing cost is calculated using Equation 21. The result is shown in the main text, Panel b of Figure 7.