The Wider Effects of Higher Pension Ages: Evidence on Labor Supply, Welfare Spillovers, and Savings[*]

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Abstract

We study the effects of a six-month increase in Denmark's early, incentivized, and full pension ages using a regression discontinuity design. Over 12 years, we estimate responses in employment, benefit claiming, and savings. The reform increased the average retirement age by 3.3 months, driven by strong labor-supply responses during newly restricted eligibility periods and persistent effects thereafter. Extended employment is accompanied by higher savings, with no evidence of anticipatory adjustment. Responses around early eligibility ages align with financial incentives, whereas those near the full pension age appear norm-driven. Strong labor-supply responses and limited spillovers into alternative benefits generate fiscal gains.

JEL Classification: H55, J26, J14, D91, H53

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1 Introduction

The long-term sustainability of public pension systems has become a pressing challenge in many countries, driven by rising life expectancy and declining fertility. In response, governments around the world are implementing pension reforms that extend working lives to alleviate fiscal pressure. One common reform is to increase the age at which individuals become eligible for retirement benefits, a policy currently being pursued in more than half of OECD countries (OECD, 2025). While raising pension eligibility ages mechanically reduces pension expenditures, the overall fiscal impact depends on how individuals adjust their behavior. Some workers may postpone retirement, increasing employment and tax revenues, while others may replace forgone pension income with alternative programs such as disability insurance (DI) or unemployment insurance (UI). Such substitution may be passive, when existing welfare claimants remain on benefits until reaching the new eligibility age, or active, when workers transition from employment into alternative programs in response to postponed pension eligibility (Staubli and Zweimüller, 2013; Rabaté et al., 2024). Individuals may also adjust along intensive margins by changing hours worked or drawing on private savings. Drawing down savings may initially limit reliance on welfare programs but increase it later as private resources are depleted. Together, these responses determine how much of the mechanical reduction in pension expenditures translates into fiscal gains.

Pension systems commonly feature an early retirement age (ERA), at which individuals first become eligible for public pension benefits, and a full retirement age (FRA), at which they become eligible for full benefits. While much of the literature studies reforms to either age in isolation, demographic pressures may increasingly require countries to raise both simultaneously.

Our contribution is to provide a comprehensive assessment of such a reform raising multiple eligibility ages, across behavioral margins over a long 12-year horizon. In our setting, financial incentives to retire vary across eligibility ages and by wealth, providing variation that helps discipline the mechanisms underlying the responses. Beyond extensive labor supply and pension claiming, we examine spillovers into alternative welfare programs and intensive-margin responses in labor supply and private saving, exploiting administrative data on individuals' full asset portfolios, including pension wealth. We study responses both within and beyond the newly restricted claiming windows between the old and new eligibility ages. We distinguish upstream responses between the reform announcement and the first restricted window, midstream responses between successive windows, and downstream responses after the new FRA, capturing the full dynamic adjustment to higher pension ages.

We exploit the 2011 Danish pension reform using a regression discontinuity design (RDD) and high-quality administrative register data. The reform introduced stepwise six-month increases across birth cohorts in three key public pension eligibility ages: the ERA, the incentivized early retirement age (IERA), and the FRA, previously set at 60, 62, and 65. At the IERA, two years after the ERA, individuals who postpone claiming receive higher early retirement benefits and are no longer subject to the means test against private pension wealth. At the FRA, those receiving early retirement benefits transition to the old-age pension (OAP) scheme. Importantly, these cohorts were unaffected by the earlier 2006 reform and therefore expected to retain their existing eligibility ages, only to face increases under the 2011 reform. This created quasi-experimental birthdate cutoffs around which otherwise highly comparable individuals face different pension ages. Our RDD focuses on the first cutoff, January 1, 1954, which provides the cleanest identification because individuals born just before the cutoff form an unaffected control group.

To provide a theoretical benchmark for our empirical analysis, we develop a life-cycle model of consumption, saving, and endogenous retirement calibrated to the Danish institutional setting. Building on Hurd et al. (2012), we extend their framework by allowing workers to exit the labor market through either pension benefits or alternative welfare programs such as UI and DI. The model captures the trade-off between consumption and post-retirement leisure and accounts for stigma costs associated with welfare claiming.

The model delivers three main predictions. First, retirement bunching is strongest at the ERA, where retiring earlier entails forgoing five years of ERP benefits, followed by the IERA, where benefits increase and cease to be means-tested against private pension wealth. Following the reform, bunching shifts to the new eligibility ages, generating positive employment responses primarily within the newly restricted claiming windows. No bunching is predicted at the FRA, where there is no comparable discrete financial incentive to retire. Second, delayed pension eligibility induces passive and active welfare substitution, with active substitution strongest at the ERA, weaker at the IERA, and absent at the FRA. Third, extended careers increase lifetime resources and, through consumption smoothing, reduce upstream saving while increasing asset accumulation during periods of extended employment and around retirement, followed by faster dissaving thereafter.

Our empirical findings support some but not all predictions. Consistent with the model, retirement bunching is strongest at the ERA, followed by the IERA, and shifts to the new thresholds following the reform, accompanied by strong employment responses in the newly restricted claiming windows. We find no significant upstream responses, but positive midstream and downstream employment effects indicate more persistent adjustments than predicted and demonstrate that the reform's effects extend well beyond the local responses between the old and new eligibility ages. The main departure from the model concerns the FRA. While the model predicts no bunching at this age, we observe pronounced retirement bunching that shifts with the reform despite the absence of discrete financial incentives to retire. Welfare spillovers closely match the model: existing claimants remain on welfare longer, while active substitution is limited and concentrated around the ERA. Saving responses also largely follow the predicted pattern, with negative but statistically insignificant upstream effects and significantly higher asset accumulation during periods of extended employment.

Quantitatively, the reform increases the average effective retirement age by 3.3 months, resulting in 19,438 USD in additional labor earnings over 2011–2022 (ages 57–68). Over the same period, private savings rise by 10,608 USD. The increase is concentrated in occupational pension wealth, which rises by 9,707 USD through higher contributions, deferred withdrawals, and associated capital gains, highlighting the importance of observing occupational pension wealth when evaluating pension reforms. The fiscal effects are substantial: net taxes increase by 10,580 USD, reflecting higher tax revenues of 4,068 USD and lower transfer payments of 6,460 USD. Thus, despite some substitution into alternative welfare programs, the strong labor-supply response generates sizable fiscal gains alongside greater private wealth accumulation.

What mechanisms underlie these responses? Our evidence points to an important role for financial incentives, although they cannot explain the labor response around the FRA. First, bunching and its shift with the reform are strongest at the ERA, where financial incentives to retire are largest, followed by the IERA, where they are smaller. These patterns remain pronounced across liquid-wealth groups, suggesting that liquidity constraints alone cannot account for them. Second, responses vary with pension wealth in line with the underlying incentives: individuals with low pension wealth respond particularly strongly at the ERA, whereas those with high pension wealth respond relatively more strongly at the IERA, when means-testing on pension wealth is relaxed.

The response around the FRA provides evidence that financial incentives alone are insufficient. Substantial bunching emerges and shifts with the FRA despite the absence of a discrete financial incentive to retire at that age. Occupational pensions, which complement the public pension system, also provide no discrete incentive to retire at the FRA, as postponed claiming is compensated through actuarially fair benefit adjustments. Demand-side explanations also appear unconvincing in the Danish context, where neither mandatory retirement nor a decline in employment protection occurs at the eligibility ages. This leaves nonfinancial mechanisms, including social norms and framing effects, as a residual explanation for the observed labor response (Behaghel and Blau, 2012; Lalive et al., 2023; Seibold, 2021).

The saving response broadly aligns with consumption smoothing. Extended careers increase lifetime resources, predicting higher upstream consumption, lower upstream saving, and greater asset accumulation during periods of extended employment, as observed in the data. However, the negative upstream saving response is statistically insignificant, so we cannot rule out saving inertia, whereby individuals continue their pre-reform saving behavior (García-Miralles and Leganza, 2024). This mechanism predicts no upstream response because consumption and saving do not adjust in anticipation of higher future earnings. Moreover, the concentration of additional wealth accumulation in quasi-mandatory occupational pension accounts points to a passive saving channel that may underlie this inertia, as extended employment mechanically prolongs contributions and delays withdrawals (Chetty et al., 2014).

Beyond these mechanisms, heterogeneity by liquid and private pension wealth reveals who drives the reform response and the associated fiscal gains. While responses vary across wealth groups at specific eligibility ages, cumulative earnings responses are similar across liquid-wealth groups but substantially smaller among individuals with high pension wealth. This latter gradient has broader policy implications: as funded occupational pensions become more important, increases in statutory pension ages could become less effective at extending working lives (Andersen et al., 2026).

Literature. Our paper contributes to the literature on the effects of increasing the ERA or FRA. Most of this literature focuses on labor-market responses (Duggan et al., 2007; Mastrobuoni, 2009; Behaghel and Blau, 2012; Staubli and Zweimüller, 2013; Vestad, 2013; Manoli and Weber, 2016b; Fetter and Lockwood, 2018; Seibold, 2021; Haller, 2021; Deshpande et al., 2024), while evidence on saving responses remains scarce, largely due to data limitations (Etgeton et al., 2023; Nakazawa, 2025; García-Miralles and Leganza, 2024). At a broad level, we contribute by jointly studying labor supply, welfare spillovers, and saving, by following responses over a 12-year horizon beyond the locally affected claiming windows, and by exploiting full asset-portfolio data to assess effects on retirement wealth.

More specifically, we first connect studies estimating labor-supply and welfare-spillover effects within the newly restricted claiming windows created by ERA and FRA reforms (Staubli and Zweimüller, 2013; Atalay and Barrett, 2015; Cribb et al., 2016) with work examining broader effects on retirement timing (Mastrobuoni, 2009; Manoli and Weber, 2016b; Lalive et al., 2023; Rabaté et al., 2024). We estimate an elasticity of the effective retirement age with respect to the simultaneous increase in the ERA, IERA, and FRA of slightly above one-half. This is close to the elasticity of about one-half estimated by Mastrobuoni (2009) and Lalive et al. (2023) for FRA increases in the United States and Switzerland, respectively, and larger than the elasticity of 0.21 estimated by Rabaté (2019) for the Netherlands. We further contribute by examining adjustment outside the newly restricted claiming windows. Consistent with previous studies of upstream and downstream effects (Mastrobuoni, 2009; Geyer and Welteke, 2021; Rabaté et al., 2024), we find no upstream response.[1] In contrast to previous evidence, we find meaningful downstream labor-supply effects. Finally, our welfare-spillover results show substantial passive but limited active substitution, broadly in line with previous evidence (Oguzoglu et al., 2020; Manoli and Weber, 2016a; Rabaté et al., 2024; Staubli and Zweimüller, 2013; Atalay and Barrett, 2015).

Second, we contribute to the strand on mechanisms behind retirement-age reform effects and bunching. We find suggestive evidence that both financial incentives and norms play important roles in retirement decisions. The gradient in bunching across eligibility ages and its variation with pension wealth point to a role for financial incentives, consistent, for example, with Manoli and Weber (2016a). At the same time, substantial bunching at the FRA despite the absence of discrete financial incentives supports a role for social norms and reference dependence (Behaghel and Blau, 2012; Lalive et al., 2023; Seibold, 2021). Demand-side factors documented by Rabaté et al. (2024) in the Netherlands, where employment protection weakens after the FRA, appear less likely to account for our findings in the Danish setting.

Finally, we contribute to the small literature on saving responses to retirement-age reforms. Most closely related to our work, García-Miralles and Leganza (2024) studies the same Danish reform around the ERA and IERA and emphasizes saving inertia as the key mechanism behind the response. We extend their analysis through the FRA and into the downstream period and incorporate capital gains on occupational pension wealth. We find lower upstream saving and higher saving during extended employment, broadly in line with consumption smoothing, although the imprecision of the upstream response prevents us from rejecting saving inertia. Related evidence consistent with consumption smoothing comes from Etgeton et al. (2023), who document lower pre-retirement saving following an ERA increase in Germany, and Nakazawa (2025), who finds higher non-pension saving during restricted claiming years in Japan.

The remainder of the paper is structured as follows. Section 2 provides institutional background on the Danish pension and social welfare system, with a particular focus on the 2011 retirement reform. Section 3 develops the life-cycle model used to derive testable predictions. Section 4 describes the data and presents descriptive statistics. Section 5 outlines the empirical strategy. Section 6 presents the main results, including effects on labor supply, benefit claiming, saving, and public finances. Section 7 examines the determinants of these responses. Finally, Section 8 concludes.

[1] Carta and De Philippis (2021) is an exception, documenting upstream labor-supply effects among middle-aged women following an ERA reform in Italy.

2 Institutional Details

2.1 Pension and Other Social Insurance Schemes

The Danish pension system has three pillars: public pensions, occupational pensions, and private savings. The first pillar consists of the Early Retirement Pension (ERP, Efterløn) and the Old-Age Pension (OAP, Folkepension). The OAP is a pay-as-you-go, tax-financed benefit available at the FRA, conditional on residency history. It consists of a universal base amount of roughly 13,000 USD per year and a supplement of up to 13,500 USD, which, after a deductible, is means-tested at a 31% taper rate against old-age income outside the public pension system. Specifically, the supplement is means-tested against pension income from the second and third pillars, other capital income, and labor earnings.[2] Beyond these core components, the system includes additional benefits available only to retirees with very low wealth. Finally, deferred OAP claims are actuarially adjusted.

The ERP scheme provides an insurance-based pathway to early labor market exit. Eligibility requires membership in a UI fund and regular contributions. Although partially funded through individual premiums, the ERP is heavily subsidized by the state and provides access to benefits from the ERA. Among the cohorts we study, 61% of individuals make regular contributions and are therefore eligible to claim ERP upon reaching the ERA. The earliest claiming age is the ERA, which was 60 for pre-reform cohorts. Individuals claiming before the IERA receive benefits equal to 91% of the maximum UI benefit and are subject to a means test on second- and third-pillar pension wealth. At the IERA, benefits increase to 100% of the UI benefit, and the pension wealth means test is removed for individuals satisfying work-hour requirements over the preceding two years. Deferring claims beyond the IERA yields small tax-free lump-sum bonuses. Throughout the ERP period, benefits remain means-tested against labor and capital income. Unlike the OAP, the ERP lacks actuarial adjustments, creating strong incentives to claim at the ERA or IERA rather than defer retirement.

The second pillar is the main source of retirement income for most recent retirees. It consists of funded, defined-contribution occupational pension schemes, similar to 401(k) plans in the United States. Participation is widespread, with sector-level collective bargaining agreements typically requiring contributions of 11–17% of earnings through payroll deductions jointly financed by employers and employees. Pension wealth is invested through pension funds and can be accessed from age 60.[3] Second-

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[2] The labor income test was abolished in 2024.

[3] Early withdrawals before age 60 are heavily penalized. All pension accounts established before May

pillar savings are paid out as annuities, providing insurance against longevity risk.[4]

Third-pillar arrangements consist of voluntary, tax-favored individual pension plans, similar to individual retirement accounts (IRAs) in the US, that offer flexible contributions and tax advantages. These plans are offered either by the same pension funds that administer employer-sponsored schemes, typically as top-ups to mandatory contributions, or by banks and life-insurance companies. Contributions can be directed to different payout products, including annuity-based pensions as well as plans that allow lump-sum withdrawals at retirement.

Beyond the pension system, Denmark provides a range of welfare schemes offering income insurance. These include UI for individuals satisfying recent employment and contribution requirements, sickness benefits covering temporary health-related work incapacity, and disability pensions for individuals with permanent work limitations. Cash assistance programs function as residual support for individuals with minimal income and assets. Collectively, these programs provide alternative pathways out of the labor market and may substitute for pension benefits when pension eligibility is postponed.

2.2 The 2011 Retirement Reform

To ensure long-term fiscal sustainability in the face of population aging, Denmark linked pension eligibility ages to life expectancy in the 2006 Welfare Reform. In 2011, the government accelerated this transition by increasing eligibility ages for cohorts that had previously been expected to remain unaffected by the 2006 reform. Specifically, the 2011 reform introduced six-month increases in pension eligibility ages for successive half-year birth cohorts born in 1954 and 1955.

Our analysis focuses on the first birthdate cutoff, January 1, 1954, which offers a clean quasi-experimental setting. Specifically, individuals born on or after January 1, 1954, learned in 2011 that their ERA would rise to 60.5, their IERA to 62.5, and their FRA to 65.5. In contrast, those born just one day earlier retained the original retirement ages of 60, 62, and 65. Our identification strategy therefore exploits the sharp discontinuity at the cutoff, comparing individuals born immediately on either side who are

1, 2007 can be accessed without a penalty as early as age 60, whereas accounts opened after that date may have a higher, often cohort-dependent, early-access age. The vast majority of individuals in our sample opened their accounts before this cutoff and can therefore access them at 60. The only reason to open a new account is a job change into a sector with a different pension fund; in that case, contributions to the new account may not be accessible at age 60, while balances in the original pre-2007 account remain accessible at 60.

[4] Individuals can typically deviate from the default by converting up to 30% of balances into annuities with payout periods as short as ten years. Lump-sum withdrawals are not permitted.

comparable in all respects except for the reform-induced changes in their retirement incentives. Because subsequent birthdate thresholds introduced by the reform involve treatment changes on both sides of the cutoff, we restrict our analysis to this first and cleanest discontinuity.

To analyze the reform at the first cutoff, we distinguish between upstream, midstream, and downstream periods surrounding the critical years in which eligibility differs across birth cohorts, as illustrated in Figure 1. Identification exploits the discontinuity at the January 1, 1954 cutoff. For individuals born on January 1, age aligns exactly with calendar years, such that in year t they are $t - 1954$ years old throughout the year (e.g., age 60 in 2014). The upstream period (2011–2013) spans the years between the reform announcement and the pre-reform ERA, during which affected individuals could adjust their behavior in anticipation of delayed eligibility. The critical eligibility years occur in 2014, 2016, and 2019. In each of these years, individuals born just before the cutoff are eligible for the full year, while those born just after are eligible only for the second half of the year, creating sharp discontinuities in access. The midstream period (2015, 2017, and 2018) captures responses between successive eligibility thresholds. The downstream period begins after the FRA (2020 onward), when all cohorts have passed the eligibility thresholds. This decomposition allows us to separately identify anticipatory responses, adjustments at eligibility thresholds, and longer-run responses, thereby providing a comprehensive assessment of the reform's consequences.

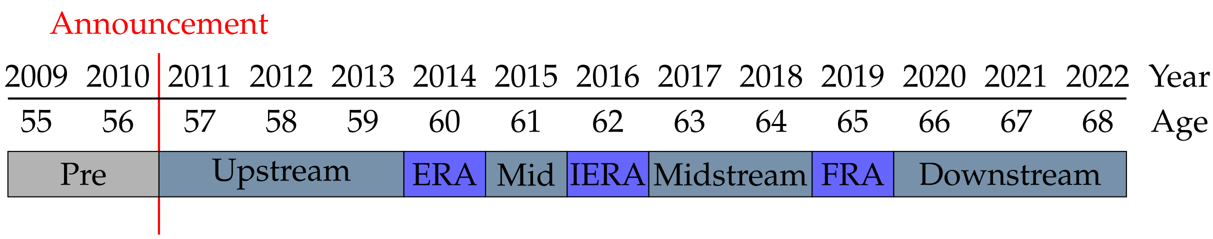


Figure 1: Reform Timeline and Classification of Analysis Periods

Notes: The figure illustrates the timing of the 2011 retirement reform relative to age and calendar year for individuals born on January 1, 1954. Shaded areas define the pre-reform, upstream, midstream, and downstream periods, as well as the critical years in which the ERA, IERA, and FRA increased by six months.

3 Model

We use a parsimonious life-cycle model of consumption and endogenous retirement to guide the empirical analysis. Building on Hurd et al. (2012) and the Danish adaptation in García-Miralles and Leganza (2024), we extend the framework by allowing individuals to exit the labor market through either pension claiming or alternative welfare programs. This extension captures substitution from pension benefits into programs such as UI, sickness benefits, and DI when pension eligibility ages increase. For tractability, we model the welfare route as an unemployment-type exit and abstract from health dynamics.

3.1 Setup and Optimality Conditions

Primitives. Time is continuous and indexed by age, $t \in [0, T_{\max}]$, with terminal age $T_{\max}$. We normalize $t = 0$ to age 57, when the reform was announced for individuals born on the birthday cutoff. The interest and discount rates are denoted by r and ρ. The pension system features three eligibility ages: the ERA, IERA, and FRA, ordered $T_{\text{ERA}} < T_{\text{IERA}} < T_{\text{FRA}}$. Before retirement, labor supply is inelastic and generates constant after-tax income $y_t \equiv y$. Agents face no borrowing constraints and leave no bequests. Let $x \in \{P, W\}$ index the pension and welfare exit routes, respectively, and let R^x denote the corresponding labor-market exit age.

Benefits. Let $b^{\text{ERS}}(t; R^P)$ denote after-tax early retirement benefits, $b^W(t; R^W)$ after-tax welfare benefits, and $b^{\text{OAP}}(t; R^x)$ old-age pension benefits after the FRA. For exit route $x \in \{P, W\}$ with $R^x < T_{\text{FRA}}$, the present value (PV) of benefits is[5]

$$B^x(R^x) = \int_{R^x}^{T_{\text{FRA}}} e^{-rt} b^x(t; R^x)\, dt + \int_{T_{\text{FRA}}}^{T_{\max}} e^{-rt} b^{\text{OAP}}(t; R^x)\, dt.$$

This PV of benefits features notches or kinks at T_{ERA}, T_{IERA}, and T_{FRA}. Consequently, the marginal present value of benefits with respect to the labor-market exit age,

$$B^{x\prime}(R^x) \equiv \frac{dB^x(R^x)}{dR^x}$$

is piecewise smooth, with discrete changes at the eligibility ages.

Preferences. Individuals derive utility from consumption and retirement, while welfare claiming entails a stigma cost. Preferences over consumption are represented by a standard increasing and concave utility function $u(c)$. Retirement provides utility from leisure, captured by an individual-specific preference parameter $\Gamma_i > 0$, which is continuously distributed across individuals.

Claiming welfare benefits before the FRA entails a stigma cost

$$s_i^x(t) = \begin{cases} 0, & x = P, \\ \theta_i\, \bar{s}(t) \cdot \mathbf{1}\{t < T_{\text{FRA}}\}, & x = W, \end{cases}$$

[5] For $R^x \geq T_{\text{FRA}}$, the exit route is irrelevant because individuals receive only OAP benefits, which remain a function of R^x through actuarial adjustments for deferred claiming.

where $\bar{s}(t)$ is a smooth, age-dependent stigma profile satisfying $\bar{s}'(t) \leq 0$, and $\theta_i > 0$ is an individual-specific loading capturing heterogeneity in stigma intensity. We assume θ_i is orthogonal to leisure preferences Γ_i (i.e., $\theta_i \perp \Gamma_i$). For notational simplicity, individual subscripts i are suppressed below.

Household problem. Agents choose consumption, the labor-market exit age R^x, and the labor-market exit route $x \in \{P, W\}$. Life-time utility is given by the upper envelope of the exit-route-specific value functions,

$$U = \max\{V^P, V^W\},$$

where the value associated with exit route x is

$$\begin{aligned} V^x = & \max_{R^x, \{c_t\}, \{a_t \geq 0\}} \int_0^{T_{\max}} e^{-\rho t} \left[u(c_t) + \Gamma \, \mathbf{1}\{t \geq R^x\} - s^x(t) \mathbf{1}\{x = W\} \right] dt \\ & \text{s.t.} \\ & \dot{a}_t = \begin{cases} r a_t + y - c_t, & t < R^x, \\ r a_t + b^x(t; R^x) - c_t, & R^x \leq t < T_{\text{FRA}}, \\ r a_t + b^{\text{OAP}}(t) - c_t, & t \geq T_{\text{FRA}}, \end{cases} \qquad a_0 = 0, \; a_{T_{\max}} = 0. \end{aligned} \tag{1}$$

Optimality conditions. The Euler equation is

$$\dot{u}'(c_t) = (\rho - r) u'(c_t),$$

and holds independently of the exit route. When $\rho = r$, consumption is perfectly smoothed over the life cycle. Lifetime resources under exit route $x \in \{P, W\}$ are therefore

$$Y^x(R^x) = \int_0^{R^x} e^{-rt} y \, dt + B^x(R^x),$$

implying constant optimal consumption under CRRA utility:

$$c^* = \frac{r}{1 - e^{-r T_{\max}}} Y^x(R^x). \tag{2}$$

For interior retirement choices (with $R^x < T_{\text{FRA}}$), the first-order condition is

$$\left(y + B^{x\prime}(R^x) \right) u'(c_{R^x}) = \begin{cases} \Gamma, & x = P, \\ \Gamma - \theta \, \bar{s}(R^W), & x = W, \end{cases} \tag{3}$$

where the left-hand side of the above FOC captures the marginal utility gain from postponing exit: the effective marginal return to working longer multiplied by the marginal utility of consumption, given by current labor income plus the marginal increase in the present value of future benefits. The right-hand side reflects the marginal utility value of leisure, equal to Γ under the pension exit path and reduced by stigma costs when claiming welfare benefits before the FRA.

3.2 Incentives and Model Predictions

We now use the model to characterize retirement and savings incentives before and after the reform.

Pre-reform incentives. The FOC for retirement in (3) implies that retirement behavior is governed by the slope of the benefit schedule, $B^{x\prime}(R^x)$: the larger $B^{x\prime}(R^x)$, the stronger the incentive to postpone retirement. Between statutory eligibility ages, benefits are differentiable, so retirement incentives therefore vary smoothly with age, implying a continuous mapping from individual types to retirement ages.

However, Figure 2 shows that the present value of pension benefits, $B^P(R^P)$, is not differentiable at the statutory eligibility ages. For individuals unaffected by the reform (solid blue line), pension wealth exhibits a large notch at the ERA, reflecting the loss of ERP benefits when retiring earlier. A second, smaller notch arises at the IERA, where the ERP benefit level increases from 91% to 100% of UI benefits and the means test on private pension wealth is removed. Beyond the IERA, small lump-sum bonuses partially offset the decline in pension wealth until the FRA.

These discontinuities in $B^P(R^P)$ generate spikes in lifetime resources and consumption, as shown in Figure 3. Because crossing the ERA or IERA yields a discrete increase in pension wealth, individuals whose optimum lies just before an eligibility age optimally postpone retirement until reaching that age, generating bunching at the ERA and IERA. By contrast, the FRA features only a kink arising from actuarial adjustments to OAP benefits. As a result, the model predicts no bunching at the FRA.

Turning to the welfare exit route, the red solid line in Figure 2 shows that before the IERA, the PV of welfare benefits, $B^W(R^W)$, calibrated to UI benefits,[6] exceeds that of the pension route because ERP benefits amount to only 91% of UI benefits. After the IERA, however, ERP benefits rise to 100% and include lump-sum bonuses, causing the pension route to dominate. Consequently, for sufficiently low stigma costs, some individuals optimally substitute into welfare programs prior to the IERA, whereas welfare exits largely disappear thereafter.

[6] For tractability, we calibrate the welfare route to UI benefits. In practice, individuals may also exit through DI, which is somewhat more generous, or cash assistance, which is somewhat less generous. Our reduced-form welfare route should therefore be interpreted as representing an intermediate benefit schedule.

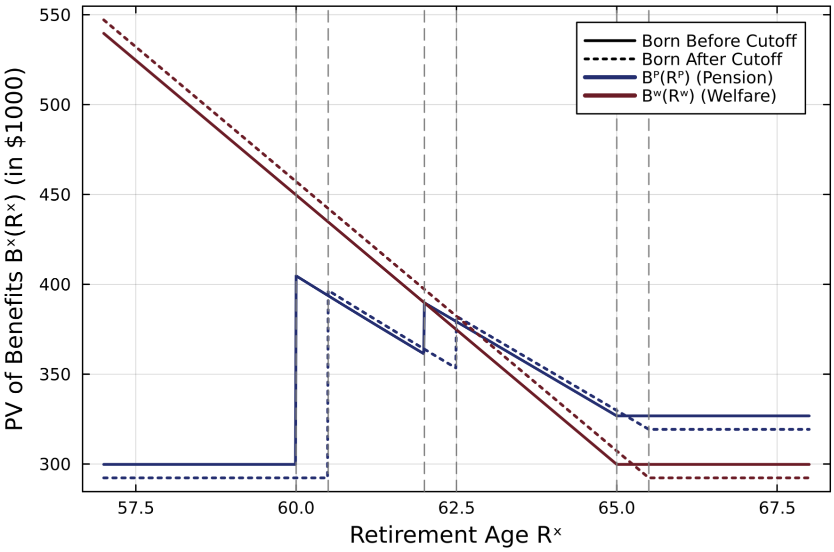


Figure 2: PV of benefits by retirement age and labor market exit path.

Notes: The figure plots the PV of benefits under the pension exit path $B^P(R^P)$ (blue) and welfare exit path $B^W(R^W)$ (red) as a function of the exit age. Solid lines correspond to individuals born before the birthday cutoff (control), while dashed lines correspond to individuals born after the cutoff (treated). Discrete jumps at the ERA and IERA in $B^P(R^P)$ reflect eligibility thresholds and changes in benefit rules within the ERP program, generating financial notches. At the FRA, the schedule exhibits a kink due to actuarial adjustment of OAP benefits, which is absent in ERP benefits. The reform shifts all eligibility ages upward by 0.5 years for treated cohorts, delaying benefit access and reducing pension wealth at earlier exit ages. The y-intercept of $B^P(R^P)$ corresponds to the PV of OAP benefits; the higher level of $B^P(R^P)$ after the FRA reflects the lump-sum bonus for postponing ERP claims beyond the IERA. The calibration assumes annual old-age pension benefits of 15,000, ERP benefits of 27,000 before the IERA (subject to wealth taper) and 30,000 thereafter, welfare benefits of 30,000, private assets of 250,000, terminal age of 85, annual labor income of 40,000, and a discount rate equal to zero.

Post-reform incentives. The reform raises all eligibility ages by 0.5 years, shifting benefit schedules and optimal consumption accordingly, as illustrated by the dotted lines in Figures 2 and 3. By reducing lifetime resources at a given exit age, the reform increases the marginal utility of consumption and therefore the incentive to postpone retirement. Individuals who would previously have retired at a given age optimally shift toward later exit within the same route, generating a rightward shift in the retirement distribution around the affected eligibility thresholds.

Figure 3 shows that consumption differs mainly within the restricted windows around the ERA, IERA, and FRA, implying limited changes in retirement behavior outside these periods, that is upstream, midstream, and downstream. Treated individuals planning to retire between the old and new ERA may either postpone retirement or substitute into welfare programs, depending on the trade-off between leisure and welfare stigma. The model therefore predicts active substitution into welfare programs around the ERA, with weaker effects approaching the IERA as the PV differential of benefits between the welfare and pension exit route narrows. After T_{IERA}, pension benefits dominate welfare benefits, implying no further exits into welfare programs. Nevertheless, passive substitution persists because individuals remain on welfare benefits for longer when pension eligibility is delayed.

Given the post-reform financial notches at the new ERA and IERA, we expect re-

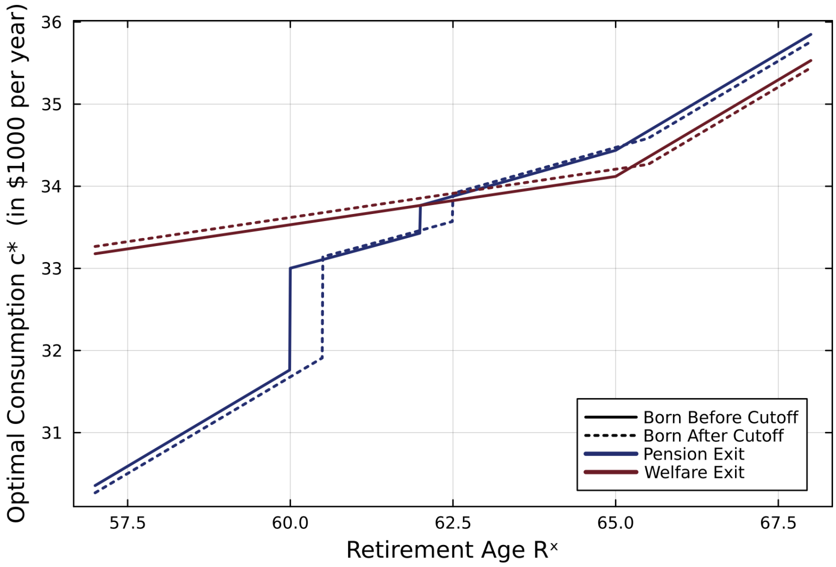


Figure 3: Consumption by retirement age and labor market exit path.

Notes: The figure plots optimal constant consumption c^* defined in equation (2) as a function of the retirement age under the pension exit path (blue) and welfare exit path (red). Solid lines correspond to individuals born before the birthday cutoff (control), while dashed lines correspond to individuals born after the cutoff (treated). Consumption equals the annuity value of lifetime resources, consisting of discounted labor income and the present value of benefits. Jumps in consumption mirror the financial notches in $B^P(R^P)$ at the ERA and IERA, while the kink at the FRA reflects actuarial adjustment of OAP benefits. The reform shifts eligibility ages upward by 0.5 years for treated cohorts, reducing lifetime resources and consumption at earlier exit ages within the affected windows. The calibration is identical to that used in Figure 2.

tirement bunching to shift to the new eligibility thresholds. More generally, when replacement rates are below 100%, delayed retirement increases lifetime resources because labor income exceeds retirement benefits: $\Delta Y^x = \Delta R^x \cdot y \cdot (1 - \text{replacement rate})$. Optimal consumption equals the annuity value of lifetime resources, implying

$$\Delta c^* = \frac{r}{1 - e^{-rT}} \Delta Y^x.$$

Consumption therefore rises upon the reform announcement, reducing upstream saving. Once employment is extended, higher earnings raise asset accumulation and retirement wealth, followed by faster wealth decumulation after retirement.

3.3 Testable Model Predictions

The pre- and post-reform incentive structure yields the following empirically testable implications, under the maintained assumption that stigma is sufficiently strong to prevent welfare claiming from becoming the dominant exit route.

Employment responses. The model predicts excess retirement mass to be largest at the ERA, somewhat smaller at the IERA, and absent at the FRA, reflecting financial notches at the former ages and only a kink at the latter. Following the reform, this bunching mass should shift to the new eligibility thresholds. Outside the eligibility windows directly affected by the reform, employment responses are predicted to be modest, as lifetime incentives change only marginally.

Spillovers to welfare programs. The reform generates two distinct forms of spillovers into welfare programs. First, passive substitution arises mechanically when delayed pension eligibility prolongs reliance on alternative benefits within the affected windows. Second, active substitution occurs when individuals optimally substitute from employment into welfare in response to changes in the relative generosity of exit routes. This active substitution is predicted to be strongest around the ERA, weaker around the IERA, and absent around the FRA, where pension benefits strictly dominate welfare benefits in present value terms.

Savings and wealth dynamics. By extending careers in the restricted windows between the old and new ERA and IERA, the reform raises lifetime resources through additional earnings. Under consumption smoothing, this induces higher consumption upon the reform announcement, implying lower saving upstream, midstream, and downstream. At the same time, savings increase during the career-extension periods, leading to higher wealth at retirement due to continued labor income and delayed retirement.

Fiscal effects. The reform affects public finances through three channels. First, a savings channel operates as postponed eligibility delays pension payments and reduces the present value of pension expenditures. Second, a substitution channel arises if individuals finance delayed pension access through welfare programs, either mechanically via longer spells or behaviorally via new take-up. Third, a revenue channel emerges from extended employment and higher labor income tax payments. The net fiscal effect is theoretically ambiguous and depends on the strength of stigma and the extent of welfare substitution. For moderate to high stigma, the savings and revenue channels are predicted to dominate, implying an improvement in public finances.

4 Data

4.1 Administrative Data

The analysis relies on Danish population-wide administrative register data covering 2001–2022, linked at the individual level through anonymized personal identifiers.

We use the population register (BEF) to obtain the demographic characteristics, including age, gender, region of residence, marital status, birthplace and citizenship. To account for attrition, we link the data to death (DOD) and migration (VNDS) registers. Educational attainment is obtained from the education register (UDDA) and grouped into four categories: below high school, high school, bachelor, and master or higher.[7] Pre-reform self-employment status is obtained from the work classification register (AKM). Information on non-pension wealth, personal income, taxes, and transfers is sourced from the income register (IND). Pension contributions to second- and third-pillar accounts are recorded in the pension payments register (INPI), pension payouts in the pension withdrawal register (INHP), and pension wealth in the pension wealth register (FORMPERS).

In addition to the annual registers described above, we use monthly registers on salaried labor income (BFL) and government transfers (ILME) to construct monthly labor market states.

4.2 Outcome Variables

We use both monthly and annual outcome variables to analyze labor supply, retirement behavior, savings, and fiscal effects of the reform.

Monthly outcomes. Our primary monthly outcome is labor market status. Using monthly labor-income and transfer registers, we classify each individual-month into one of six mutually exclusive labor market states: (1) employment, (2) retirement, (3) UI, (4) DI, (5) other benefits, and (6) no income. Individuals are assigned to the category corresponding to their primary income source in a given month. Retirement income primarily consists of ERP and OAP benefits,[8] UI includes unemployment benefits and cash assistance, while DI consists of disability pensions and sick leave benefits. Other benefits comprise a broad range of public transfers, including leave-related benefits, activation programs, and wage-subsidy schemes. Individuals classified as having no income are assumed to rely on private savings.

Annual outcomes. Annual outcomes include labor earnings, pension benefits, welfare benefits, contributions to retirement accounts, total saving, and net taxes.

Annual labor earnings are defined as earnings subject to labor-market contributions.[9] Pension benefit receipts are measured using the retirement income variable underlying the monthly retirement measure, aggregated from monthly to annual fre-

[7] For the 788 individuals for whom educational attainment is unobserved, we assign them to the lowest education group. Following the data restrictions described in Section 4.3, only 65 of these individuals remain in the final sample.

[8] We also include smaller schemes such as civil servant pensions, *tidlig pension*, *delpension*, survivor pensions, deferred-claiming bonuses, and ATP.

[9] This includes wages and salaries, bonuses and fringe benefits, self-employment and business income, honoraria and fees for personal work, certain taxable employee benefits, and employer contributions to labor-market pension schemes.

quency. Similarly, welfare benefit receipts are measured as the sum of UI, DI, and other transfer benefit payments underlying the definition of monthly labor market status, aggregated to annual frequency.

Our primary saving outcome is total saving, measured as the annual change in net wealth, including pension wealth held in second- and third-pillar retirement accounts.[10] Non-pension wealth includes bank deposits, stocks, property assets, and foreign assets, net of liabilities. This measure captures both pension and non-pension wealth accumulation and incorporates capital gains and losses. To better understand the mechanisms underlying changes in total saving, we also examine contributions to second- and third-pillar retirement accounts. These contributions include both employer-sponsored occupational pensions and alternative private pension plans. Retirement contributions can further be divided into annuity-based plans and plans allowing lump-sum withdrawals at retirement. The latter consist exclusively of voluntary third-pillar savings, whereas annuity-based plans are predominantly quasi-mandatory second-pillar occupational pensions, although they may also include voluntary third-pillar retirement saving.

Net taxes are defined as taxes paid net of public transfers received. All monetary outcome variables are deflated to 2010 prices. To reduce the influence of outliers in the regression analysis, we winsorize net taxes and total saving at the 1st and 99th percentiles, and labor earnings and retirement contributions at the 99th percentile.

4.3 Data Restrictions

The sample selection process is detailed in Table 1. We begin with the universe of individuals born within six months of the cutoff date. We then restrict the sample to native Danes—defined as individuals with at least one parent who is both a Danish citizen and born in Denmark—to ensure comparability in institutional exposure. Next, we restrict the sample to individuals who made voluntary contributions to the ERP scheme in at least seven years between 2001 and 2010, making them likely to be eligible, as eligibility requires contributions in 25 of the previous 30 years. Finally, we require a balanced panel from 2006 to 2019. To achieve this, we exclude individuals who died, resided abroad for more than two years, or have missing data for earnings or labor market contributions in any year during this period. The following section provides summary statistics for this analysis sample as well as the unrestricted population.

[10] Pension wealth from the pension register (FORMPERS) is available only for 2014–2022. For 2009–2013, we impute pension wealth using observed contributions and pension wealth returns, as described in Appendix A.

Table 1: Sample Selection

	Number of Individuals
Restriction 1: Born around the cutoff date	73,875
Restriction 2: Only native Danes	67,740
Restriction 3: Regular contributions to the ERS	44,826
Restriction 4: Balanced sample	41,748

Notes: The table reports the number of individuals remaining after each sample restriction: birth within ±6 months of the January 1, 1954 cutoff; native Danes (at least one parent born in Denmark and holding Danish citizenship); at least seven years of voluntary ERP contribution during 2001-2010; and a balanced panel over 2006–2019.

4.4 Descriptive Statistics

Summary Statistics. To assess the potential implications of sample selection, we compare descriptive statistics for two different sampling choices, focusing on the pre-reform year 2010. The unrestricted sample contains all individuals born within six months of the relevant cutoff. The analysis sample applies the data restrictions outlined above. Table 2 presents summary statistics on individual characteristics across the different samples.

The analysis sample is balanced by gender (48% male) and consists primarily of married individuals (73%). The average age is 56.99 years. Mean annual earnings amount to 61,408 USD, and 8% are self-employed.[11] The share receiving welfare benefits, defined as receiving more than USD 1,400 in unemployment and disability benefits in total, is 17%. Average retirement contributions equal 8,672 USD, and mean total wealth is 284,129 USD. Finally, 52% of the sample is treated.

Our analysis sample is broadly similar to the unrestricted sample in terms of observed characteristics. The main differences are earnings and wealth, which are higher in the analysis sample. In addition, individuals in the analysis sample are more likely to be married and, slightly less likely to be male and on welfare benefits, and more likely to contribute to retirement plans.

Effective Retirement Age. Next, we present the distribution of the effective retirement age in Figure 4. This measure captures when individuals stop working and therefore may not coincide with when they claim pension benefits.[12] We find retirement bunching at the ERA, IERA, and FRA, with the reform shifting the bunching by six months. While there are clear financial incentives to delay retirement until the ERA or IERA – since public pension wealth rises sharply at these thresholds, as shown in

[11] We classify individuals as self-employed if self-employment constituted their primary source of income in at least one year during the pre-reform period 2006–2010.

[12] See Appendix B for a detailed definition of the effective retirement age.

Table 2: Summary Statistics

	Analysis Sample		Unrest. Sample	
	Mean	SD	Mean	SD
Demographics				
Age	56.99	0.29	56.99	0.29
Male	0.48	0.50	0.51	0.50
Married	0.73	0.45	0.66	0.47
% of People with a Bachelor education or higher	0.31	0.46	0.27	0.45
Treated	0.52	0.50	0.52	0.50
Labor Market Status				
Earnings	61,408	40,733	53,068	53,594
Share of self-employed	0.08	0.26	0.08	0.27
Share collecting welfare benefits	0.17	0.37	0.25	0.44
Savings and Wealth				
Any Contribution to Retirement Plans	0.92	0.27	0.75	0.43
Contributions to Retirement Plans	8,672	9,417	6,769	11,842
Contributions to Annuities	7,240	9,245	5,687	11,573
Contributions to Lump-sum Withdrawables	1,431	2,335	1,081	2,116
Total Wealth	284,129	338,971	237,899	900,796
Number of individuals	41,748		73,875	

Notes: Summary statistics are measured in the pre-reform year 2010. Earnings, contributions, and wealth are reported in USD. The share collecting welfare benefits includes individuals receiving more than 1,400 USD in unemployment and disability benefits in total. Treated indicates indicates individuals born on or after the birthday cutoff. The unrestricted sample includes all individuals born within six month of the reform cutoff date. The analysis sample applies three additional restrictions described in Section 4.3.

Figure 2 – no such jump occurs at the FRA. Yet, bunching patterns around the FRA are similar to those observed at earlier eligibility ages.

Labor Market States. Figure 5 plots the share of individuals in each labor market state by age and birth cohort, separately for those born before and after the cutoff. Panel 5a shows that employment falls sharply at each eligibility age, with the entire exit profile for the treated cohort shifted to the right in line with the six months increase in eligibility ages. Correspondingly, Panel 5b shows that entry into retirement is concentrated at these same ages, with the treated group's transition occurring six months later. Turning to alternative non-employment states, Panel 5c indicates that UI participation rises initially, drops discretely at the ERA, then declines gradually before falling to near zero at the FRA, which is a patterns consistent with transitions from UI into pension benefits once these pathways become available. Panel 5d shows that DI participation rises gradually up to the ERA, declines slightly once early-retirement

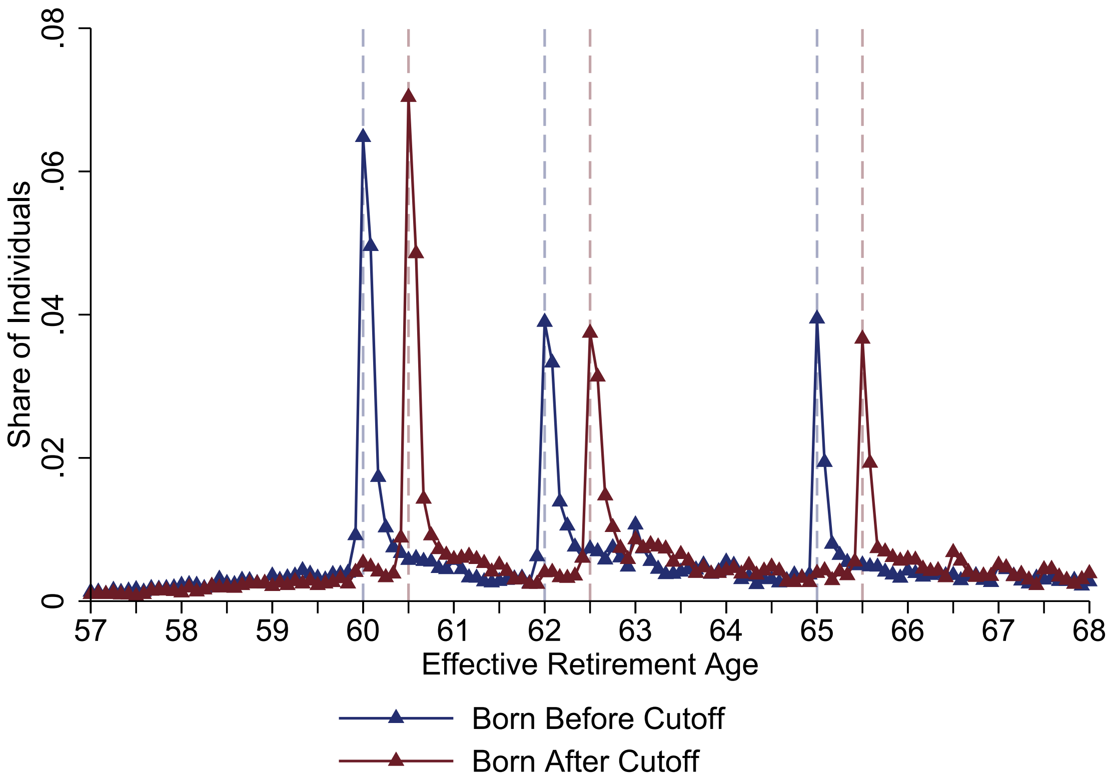


Figure 4: Effect of the Reform on the Effective Retirement Age Distribution

Notes: This figure shows the distribution of effective monthly retirement age for two groups: those born within the six months before and those born within the six months after the January 1, 1954 birthday cutoff. Effective monthly retirement age is defined based on the timing of when individuals stop receiving labor income, as described in detail in Appendix B.

benefits become available, and then increases again through the early 60s before dropping sharply to zero at the full retirement age. Panel 5e shows that claims of other benefits remain quantitatively small and fairly stable, but decline around the ERA and fall to zero at the FRA. Finally, Panel 5f shows that the share with no recorded income, that is, living off savings—remains, roughly constant at about 2% and falls to near zero at the FRA, indicating that only a tiny fraction postpone claiming OAP.

Overall, the figure suggests that the reform primarily delays retirement entry and extends employment. The age profiles in Panels 5c–5e also point to substitution from pension receipt toward alternative welfare benefits: when access to early and full pension benefits is postponed by six months, the treated cohort remains on UI, DI, and other benefits for correspondingly longer before transitioning into retirement. This is also evident in the combined welfare-benefit share—defined as the aggregated shares on UI, DI, and other benefits—shown in Figure 6. Whether this reflects mainly passive substitution—mechanically extending time on pre-existing benefits—or also active substitution through changes in program take-up and behavior cannot be inferred from the descriptive patterns alone and therefore requires formal regression analysis discussed in the next section.

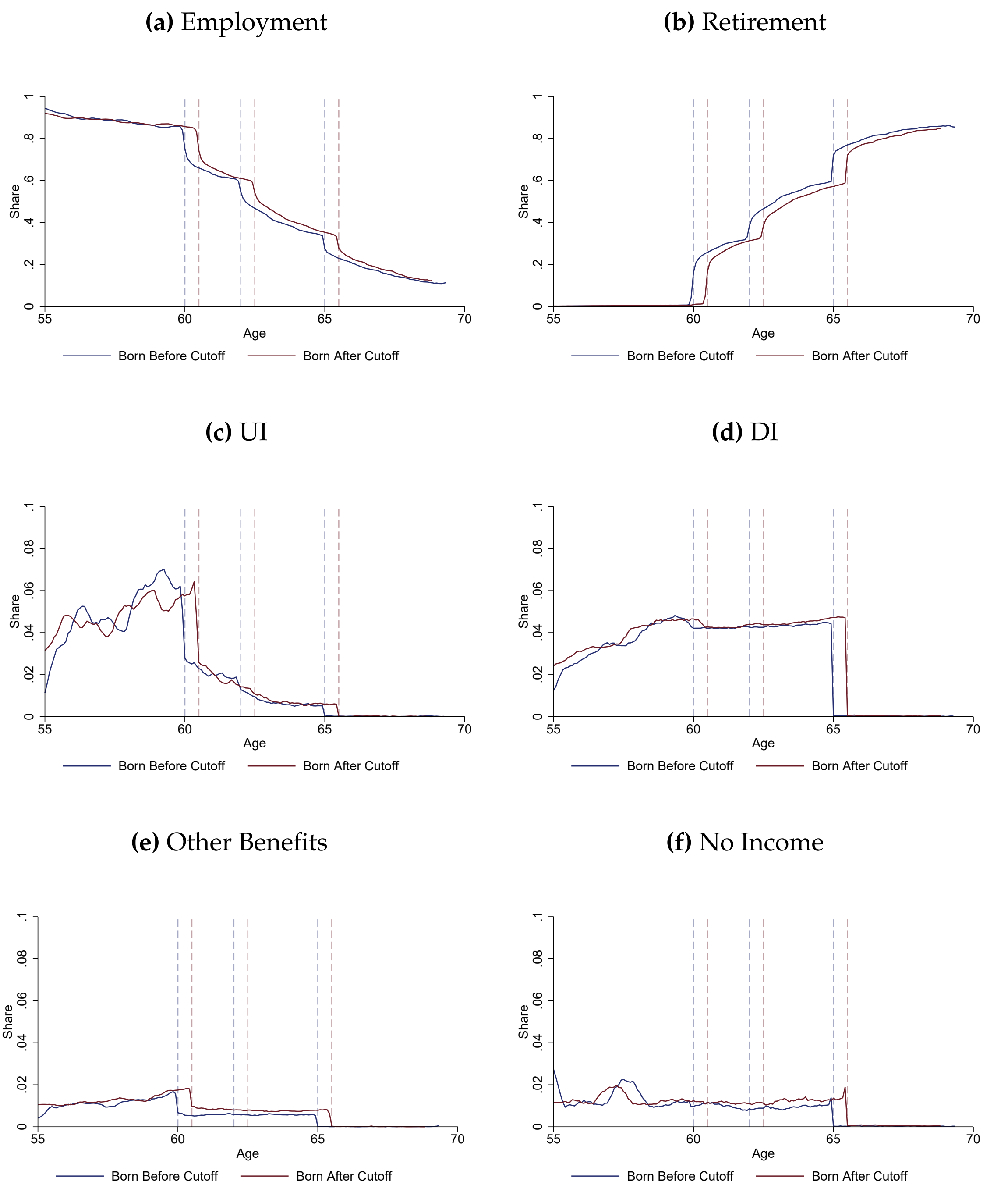


Figure 5: Shares in Different Labor Market States by Age

Notes: This figure shows the share of individuals in each of the six mutually exclusive labor market states by age (in months), as defined in Section 4.2. Labor market status is assigned based on the individual's main source of income in each month.

5 Empirical Strategy

5.1 Regression Discontinuity Design

To estimate the causal impact of the reform on our outcome variables, we exploit the cohort-based rollout of the policy in a regression discontinuity design (RDD). Identifi-

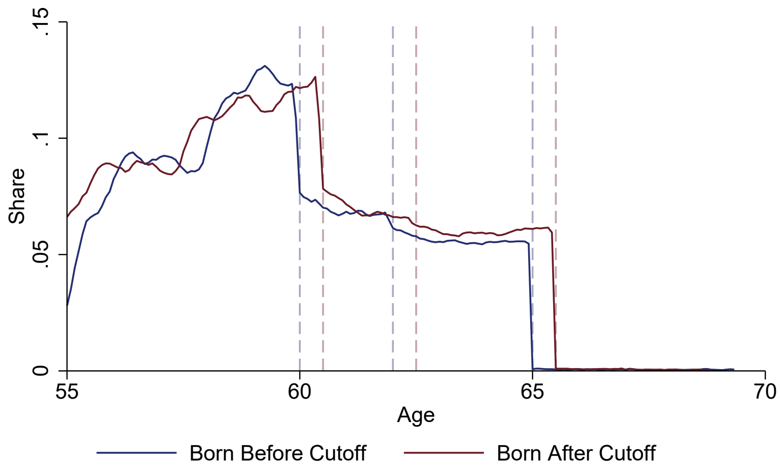


Figure 6: Share on Welfare Benefits by Age

Notes: The figure shows the share of individuals on welfare benefits by age (in months). Individuals are classified as being on welfare if their primary source of income is UI, DI, or other public benefits.

cation stems from the discrete change in pension eligibility ages at the birthdate cutoff. Specifically, individuals born on January 1, 1954 or in the six months thereafter, face an increase of six months in the ERA, IERA, and FRA—from 60, 62, and 65 to 60.5, 62.5, and 65.5, respectively.

We estimate the following RDD specification to identify discontinuities in outcomes at the birthdate cutoff:

$$y_{ij} = \alpha + \beta_j \cdot \mathbf{1}[x_i \geq c] + \gamma_j \cdot (x_i - c) + \delta_j \cdot \mathbf{1}[x_i \geq c] \cdot (x_i - c) + Z_i'\theta + \varepsilon_{ij} \quad (4)$$

where y_{ij} denotes an outcome for individual i measured at index j, which captures either calendar time or age (measured in months). The variable x_i is the running variable (birthdate), c is the cutoff date, and Z_i is a vector of categorical controls for gender, education, marital status, and region of residence, all measured in the pre-announcement year 2010. The error term is denoted by ε_{ij}. The coefficient of interest, β_j, captures the causal effect of the increase in eligibility ages for individuals born just after the cutoff at time (or age) j.

Our baseline specification estimates equation (4) using the full analysis sample, comprising individuals born within six months on either side of the cutoff, with local linear regressions estimated separately on each side of the threshold. We assess robustness in two ways. First, we replace the local linear specification with local quadratic regressions, following, for example, Rabaté et al. (2024). Second, we implement the robust bias-corrected estimator of Calonico et al. (2014) using triangular kernel weights.

Identification relies on the assumption that individuals born just before and just after the cutoff are comparable in all relevant characteristics, except for the differential incentives induced by the reform. Under this condition, discontinuities in outcomes at the cutoff can be interpreted as causal effects of the reform.

The validity of the identification strategy is supported by several considerations. First, manipulation of the running variable around the cutoff—representing the standard threat to identification in an RDD setting—is not a concern, as birthdate is predetermined. A second concern is that other policies or institutional changes may vary discontinuously at the cutoff and thus confound the estimates. We are not aware of any such reform or institutional change at the January 1, 1954, birthdate cutoff. Third, the increase in pension eligibility ages could affect mortality and thereby induce selection bias when using a balanced panel over time. In this context, two Danish studies by Nielsen (2019) and Vejlin et al. (2026) find muted effects of increases in pension eligibility ages on mortality, health, and healthcare utilization.[13] Finally, there is a concern that Danish children are expected (though not required) to start school in the calendar year in which they turn six. This may create a discontinuity in school starting age at January 1, 1954, which could affect outcomes late in life independently of the reform. While we do not observe school-starting age for individuals born around January 1, 1954, we can conduct placebo tests in pre-reform years. These tests reveal no discontinuities in our outcomes of interest (see Appendix C).

6 Results

In this section, we present our main findings on the causal effects of increasing pension eligibility ages. We first examine the effects of the reform on labor market states—the extensive margin—around the three eligibility ages. Second, we analyze the effect on the average retirement age. Finally, we turn to the dynamic responses of labor earnings, pension and welfare benefits, and saving, and assess their implications for fiscal externalities.

We present both graphical nonparametric evidence and regression-based estimates of the causal effects of the reform. The graphical analysis plots average outcomes by one-week birth cohorts around the cutoff and overlays local linear fits estimated separately on each side of the threshold using the underlying individual-level data. We complement this visual evidence with formal regression discontinuity estimates to quantify effect sizes and assess statistical significance.

[13]Evidence from Norway, Sweden, and France likewise shows no effects of changes in retirement ages on mortality; see Hernaes et al. (2013), Hagen (2018), and Bozio et al. (2021).

6.1 Effects on labor market status

We use the monthly labor market status measure to study how the reform affected individuals' labor market attachment within the newly restricted claiming windows induced by the higher eligibility ages. Figure 7 presents graphical evidence for employment, retirement, and welfare benefit receipt, where the latter combines UI, DI, and other benefits. The figure plots the share of individuals in each state three months after the pre-reform eligibility age, by one-week birth cohort around the cutoff. With one exception, all panels display clear discontinuities at the birthdate cutoff and otherwise smooth trends, consistent with a causal effect of the reform. Across all eligibility ages, the reform increases employment and reduces retirement, while welfare benefit receipt also rises at the ERA and FRA.

The regression estimates with controls in Table 3 confirm the graphical evidence in Figure 7: the reform increases employment at all three postponed eligibility ages, with corresponding declines in retirement. At the ERA, employment rises by 17.1 pp while retirement falls by 22.3 pp. Given a pre-threshold employment rate of 86% (the population at risk), this corresponds to a retirement hazard (conditional retirement response) of around 10% and implies retirement bunching at the eligibility threshold of roughly 8% of employed individuals. Most of the reduction in retirement is absorbed by continued employment, although welfare benefit receipt also increases by 5.2 pp, reflecting spillovers into alternative programs. Overall, roughly three-fourths of the decline in retirement is offset by higher employment, while about one-fourth translates into higher welfare receipt.

At the FRA, employment increases by 8.8 pp and retirement declines by 15.8 pp. Relative to the substantially lower pre-threshold employment rate of 35%, this implies a higher retirement hazard of around 14%, despite the smaller absolute employment response. Welfare benefit receipt rises by 5.6 pp, suggesting more pronounced spillovers at this threshold. The composition of these spillovers also differs: at the ERA, they are primarily driven by UI and other benefits, whereas at the FRA they mainly reflect increased DI receipt.

By contrast, we find no economically or statistically meaningful spillovers to welfare benefits at the IERA, where the 10.7 pp decline in retirement is almost entirely absorbed by a 10.3 pp increase in employment. This is consistent with the institutional setting, as treated individuals remain eligible for early retirement benefits at the old IERA and therefore do not lose access. Instead, the reform delays the exemption from means-testing, which applies only from the new IERA. With a pre-threshold employment rate of 61%, the implied retirement hazard is around 9%, corresponding to bunching of approximately 6% at the eligibility age.

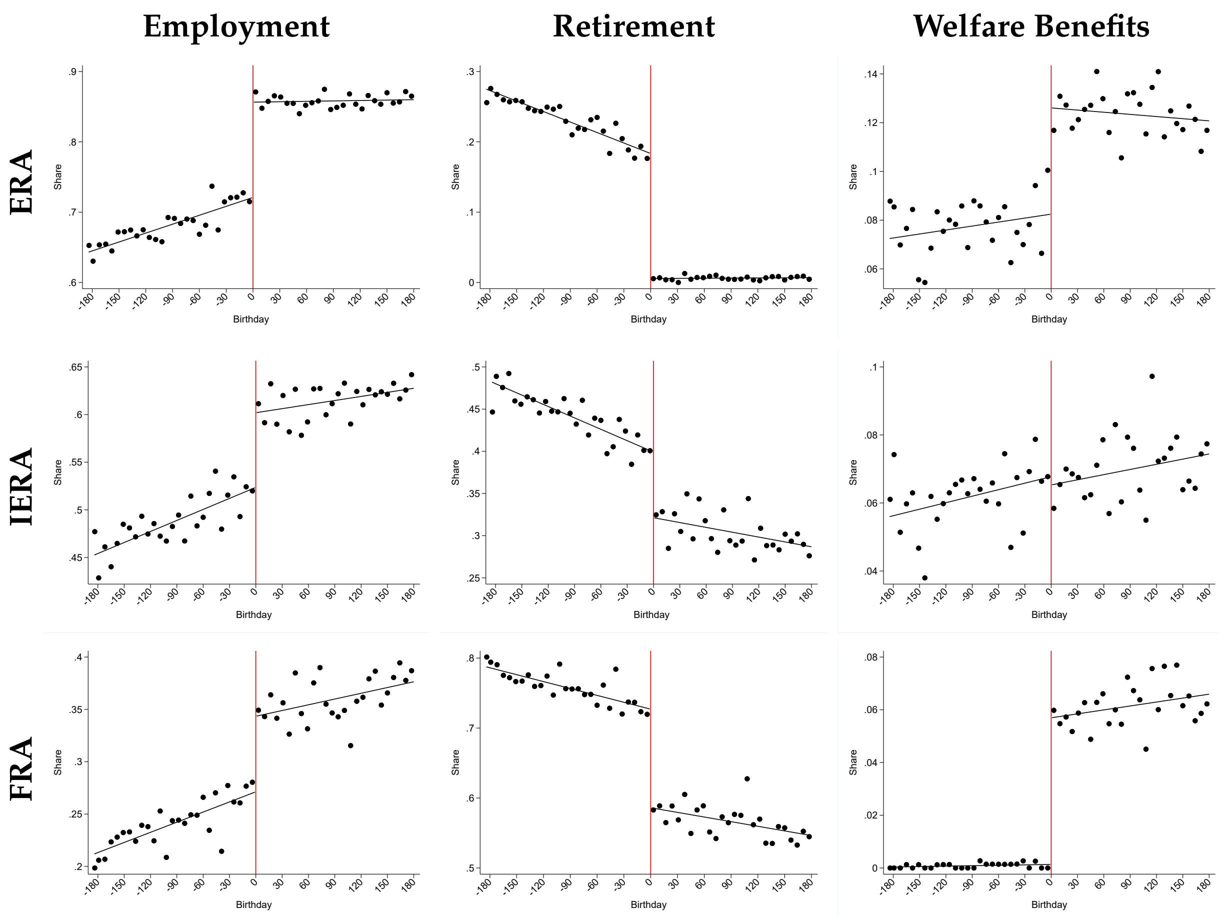


Figure 7: RDD Plots of Labor Market Responses at Eligibility Thresholds
Notes: This figure plots the average share of individuals in employment, retirement, and receipt of welfare benefits in the month three months after the pre-reform eligibility ages, by one-week birth cohorts. Each dot represents the mean outcome within a weekly cohort. Columns correspond to outcome variables, while rows correspond to the three eligibility ages (ERA, IERA, and FRA). The red vertical lines indicate the January 1, 1954 birth-date cutoff. Local linear regressions are fitted on either side of the cutoff using the underlying (unbinned) data without controls.

Table 3: Labor Market Responses at Eligibility Thresholds

	Results (in percentage points (pp))							At ERA, IERA, or FRA		
	(1) Employment	(2) Retirement	(3) UI	(4) DI	(5) Other Benefits	(6) No Income	(7) Welfare Benefits	(8) Employment rate (%)	(9) Hazard rate (Level)	(10) Bunching (%)
ERA	17.13***	−22.33 ***	4.02***	0.15	1.07***	−0.03	5.23***	86	0.1	8
IERA	10.33***	−10.66 ***	−0.07	0.01	0.14	0.25	0.08	61	0.09	6
FRA	8.78***	−15.82 ***	0.51***	4.45***	0.63***	1.44***	5.60***	35	0.14	5

Notes: The table reports the reform's estimated local treatment effects on individuals' labor market state two months prior to the post-reform eligbility ages, expressed as the pp change in the probability of being in each state. Columns (1)-(6) report estimates of β_j from the RDD specification in equation (4) for the mutually exclusive labor market states defined in Section 4.2. Column (7) differs in that it aggregates UI, DI, and other benefits into a single indicator for welfare benefit. Columns (8)-(10) summarize economic magnitudes at each eligibility threshold, reporting the pre-threshold employment rate, the implied hazard (the discrete drop in employment at the threshold), and bunching, defined as the share of employed individuals retiring at that age. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

These spillovers to welfare programs at the ERA and FRA may reflect both passive and active substitution. Passive substitution arises mechanically when delayed pension eligibility prolongs the duration of existing welfare benefit receipt until pension benefits become available. To test for active substitution—that is, transitions from employment into welfare receipt induced by the reform—we estimate equation (4) using transition indicators as outcomes. Specifically, we compare individuals' labor market status in the month preceding the old eligibility age with their status five months later, right before the new eligibility threshold. The coefficient β_j therefore captures the causal effect of the reform on transitions from employment into welfare programs.

Table 4 provides evidence of active substitution at the ERA: among individuals employed just before the old ERA, the treated cohort is 0.62 pp more likely to receive welfare benefits four months later. This implies that around 12% of the total welfare spillover (+5.23 pp) reflects active substitution, while the remainder is driven by passive substitution among individuals already receiving benefits before the cutoff. By contrast, we find no significant active substitution at the IERA or FRA, suggesting that welfare spillovers at these thresholds mainly reflect mechanical delays in pension claiming rather than new take-up of welfare programs.

Table 4: Active Substitution into Welfare Benefits

	Active Substitution (in pp)				At ERA, IERA, or FRA		
	(1) Welfare Benefits	(2) UI	(3) DI	(4) Other Benefits	(5) Employment rate (%)	(6) Hazard rate (Level)	(7) Bunching (%)
ERA	.62***	.23***	.31	.08	86	.1	8
IERA	-.07	.02	-.12	.04	61	.09	6
FRA	.12	-.03	.08	.07	35	.14	5

Notes: The table reports the reform's estimated local treatment effects on transitions from employment into welfare receipt around the eligibility ages. Columns (1)-(4) report estimates of β_j from the RDD specification in equation (4) for $j \in \{\text{ERA, IERA, FRA}\}$, where the outcome is a transition indicator equal to one if an individual employed in the month preceding the pre-reform eligibility age transitions into welfare receipt two months prior to the post-reform eligbility age. Column (1) measures transitions into any welfare benefit and therefore aggregates the specific categories reported separately in columns (2)-(4): DI, UI, and other benefits. Columns (5)-(7) report the corresponding pre-threshold employment rate, the implied hazard (the discrete drop in employment at the threshold), and bunching, defined as the share of employed individuals retiring at that age. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Overall, the reform increases employment and reduces retirement at all eligibility ages. Spillovers into welfare programs are economically meaningful at the ERA and FRA but absent at the IERA, and appear to be driven mainly by passive substitution, with a smaller but non-negligible active component at the ERA.

Effect on Average Retirement Age. While the RDD estimates identify local effects at specific ages, they do not directly capture the effect on the average retirement age. Following Mastrobuoni (2009) and Rabaté et al. (2024), we recover this effect by aggregating the age-specific employment responses in Figure 8. Under the assumption that the reform affects retirement behavior only within the estimated age range from 57 to 68, these responses map into changes in the retirement age distribution and hence in the average retirement age. Using this approach, detailed further in Appendix D, we estimate that a six-month increase in eligibility ages raises the average retirement age by 3.3 months, implying an elasticity of 0.55. Figure 8 shows that the increase is driven mainly by higher employment between the old and new eligibility ages, with some midstream and downstream effects and limited evidence of upstream responses.

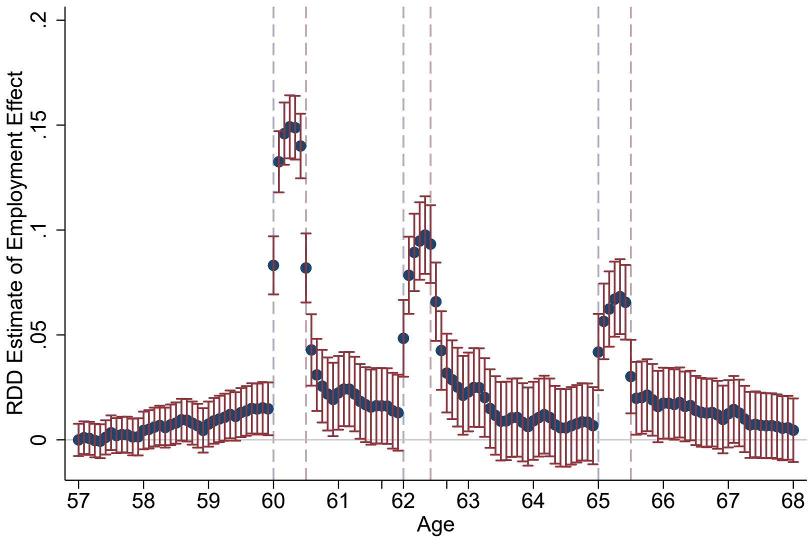


Figure 8: RDD Employment Effect by Age (in months)

Notes: The table reports the reform's estimated local treatment effects β_j from the RDD specification in equation (4) on employment, defined as one minus effective retirement, in percentage-point changes in the probability of being employed. We show the 95% confidence band around the estimates.

6.2 Dynamic responses

Next, we examine dynamic responses to the reform over the 12 years following its 2011 announcement using the annual outcome variables. For individuals born on January 1, for whom age aligns exactly with calendar years, we can trace outcomes from ages 57 to 68 over the years 2011 to 2022. In the critical years 2014, 2016, and 2019, these individuals are aged 60, 62, and 65, respectively, and face a six-month increase in eligibility ages relative to adjacent pre-cutoff cohorts.

Figure 9 illustrates the dynamic responses in four key outcomes: labor earnings, welfare benefit receipt, total saving, and net taxes. The figure plots average USD amounts over the relevant subperiods by one-week birth cohorts. For example, consistent with the extensive-margin results above, the labor earnings panels at the critical ages 60, 62, and 65 exhibit clear upward discontinuities at the birthdate cutoff, alongside smooth patterns on either side.

Table 5 reports the corresponding estimates β_j from the RDD specification in equation (4) for these key variables as well as pension benefits and contributions to retirement plans. For reference, control-group means are shown in gray beneath each coefficient estimate. While the table (and Figure 9) presents estimates aggregated over the upstream, midstream, and downstream periods, Figure 10 reports year-specific estimates.

Upstream. The first column of Table 5 shows no statistically significant upstream responses across any outcomes. Point estimates for labor earnings and welfare benefits are quantitatively negligible, whereas savings decline considerably and net taxes increase.

Critical Ages (Years). Columns (2)–(4) show sharp responses at the eligibility ages directly affected by the reform. At ages 60 and 62, when the ERA and IERA increase by six months, labor earnings rise by 5,722 USD (+13% relative to the mean) and 4,730 USD (+14%), respectively, while pension benefits decline by 3,847 USD and 2,697 USD, consistent with delayed retirement and postponed pension claiming.

At age 65, when the FRA increases by six months, earnings rise by 3,979 USD (+24%), while the decline in pension benefits is more modest. This reflects a composition effect: treated individuals often receive a mix of ERP and OAP within the calendar year, whereas adjacent pre-cutoff cohorts typically receive a full year of OAP. Since ERP benefits are more generous on average, the decline in annual pension income is therefore dampened.

Consistent with the rise in welfare benefit receipt at the newly restricted ages (5.2 pp at the ERA and 5.6 pp at the FRA), average welfare benefit amounts increase by 721 USD at age 60 and 916 USD at age 65.

Contributions to retirement plans increase significantly at all critical ages and closely mirror the rise in labor earnings, consistent with quasi-mandatory occupational pension contribution rates of roughly 10–17% of earnings. Accordingly, the increase is driven primarily by employer-administered contributions to annuity-based pension accounts, while voluntary saving in lump-sum-withdrawable accounts remains limited.

Total saving rises in the critical years, particularly at ages 62 and 65, and by more than retirement contributions alone, indicating broader increases in wealth accumulation during the delayed retirement period.

Together, these responses generate positive fiscal externalities at ages 60 and 62 through higher tax payments and lower transfers. At age 65, however, the fiscal effect turns slightly negative, as weaker earnings responses and greater welfare receipt more than offset the savings from lower OAP payments.

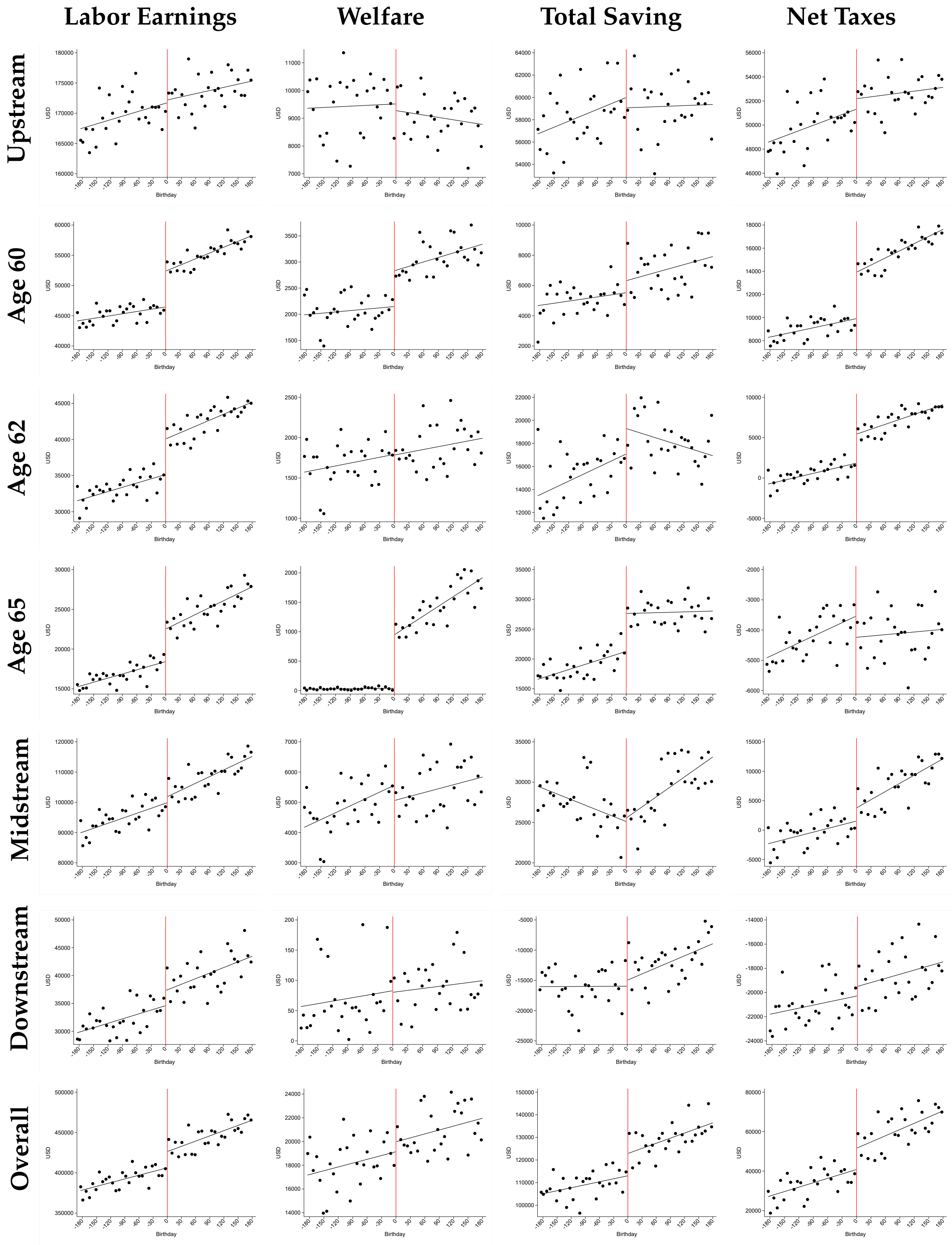


Figure 9: RDD Plots of Dynamic Responses

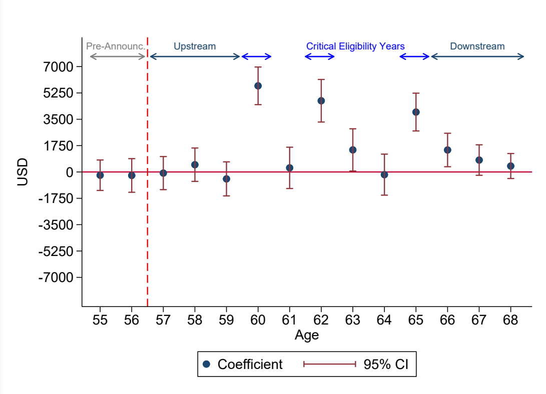
Notes: This figure plots the average USD amounts of outcome variables by one-week birth bins. Each dot represents the mean outcome within a weekly cohort. The red vertical lines correspond to the January 1, 1954, birth date cutoff. Local linear regressions are fitted on either side of the cutoff using the underlying (unbinned) data without controls. Outcomes (columns) are aggregated over different phases of the adjustment period following the reform (rows): upstream (ages 57–59; years 2011–2013), midstream (ages 61, 63–64; years 2015, 2017–2018), downstream (ages 66–68; years 2020–2022), and the full period (ages 57–68; years 2011–2022). For these periods, coefficients represent cumulative effects within indicated years. In contrast, panels for the critical years (ages 60, 62, and 65; years 2014, 2016, and 2019) report point-in-time outcomes for those specific years, in which treated individuals face a shorter eligibility duration.

Table 5: Dynamic Response to the Reform

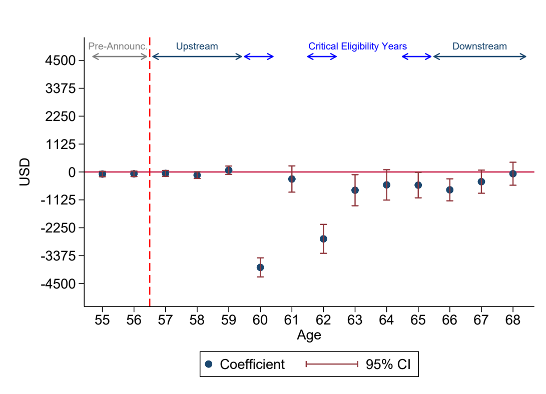
	Upstream	Critical Years			Midstream	Downstream	Overall Response
Years	2011–13	2014	2016	2019	2015, 2017–18	2020–22	2011-22
Ages	57–59	60	62	65	61, 63–64	66–68	57-68
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Labor supply							
Labor Earnings	-36	5,722***	4,730***	3,979***	1,510	2,645*	19,438***
	(1,611)	(637)	(721)	(640)	(1,922)	(1,428)	(5,794)
	169,558	45,283	33,314	16,811	94,813	32,134	391,914
B. Public Benefits Claiming							
Pension Benefits	-112	-3,847***	-2,697***	-530**	-1,507*	-1,138*	-9,813***
	(199)	(196)	(298)	(262)	(809)	(610)	(1,849)
	2,716	8,378	16,393	21,888	50,431	71,379	171,185
Welfare Benefits	-89	721***	11	916***	-456	-2	928
	(418)	(149)	(135)	(83)	(376)	(25)	(984)
	9,436	2,067	1,680	30	4,857	70	18,141
C. Saving							
Contributions to Retirement Plans	-209	586***	698***	704***	325	448*	2,911***
	(360)	(127)	(127)	(117)	(339)	(248)	(1,087)
	22,931	6,188	4,864	2,757	14,297	5,117	56,154
Annuities	-100	578***	694***	518***	385	225	2,564**
	(347)	(124)	(121)	(101)	(314)	(206)	(1,008)
	19,972	5,785	4,267	1,978	11,951	3,630	47,583
Lump-sum Withdrawable	-90	14	14	190***	-50	188*	359
	(91)	(24)	(30)	(48)	(91)	(98)	(266)
	2,934	398	591	771	2,327	1,480	8,501
Total Saving	-1,134	899	2,221**	6,492***	46	1,476	10,608***
	(1,381)	(705)	(967)	(1,081)	(1,632)	(1,707)	(3,412)
	58,335	5,091	15,247	18,887	27,332	-15,997	108,895
D. Fiscal Externalities							
Net Taxes	617	3,901***	3,490***	-765*	1,984	695	10,580***
	(937)	(390)	(464)	(407)	(1,240)	(886)	(3,617)
	49,927	9,089	522	-4,241	-412	-21,048	33,836
Taxes	-43	890***	813***	1,005***	156	838	4,068*
	(651)	(230)	(246)	(227)	(650)	(564)	(2,256)
	64,080	19,930	18,740	15,656	55,115	43,230	216,751
Transfers	-639	-2,991***	-2,691***	1,745***	-1,835**	200	-6,460***
	(504)	(242)	(299)	(259)	(802)	(584)	(2,062)
	13,993	10,799	18,201	19,869	55,434	64,134	182,430

Notes: The table reports the reform's estimated local treatment effects from the RDD specified in equation (4), where the estimation sample includes all individuals born within six months on either side of the cutoff. Outcomes are aggregated over different phases of adjustment period following the reform: upstream (2011-2013), midstream (2015,2017,2018), downstream (2020-2022), and the full period (2011-2022). For these periods, coefficients represent cumulative effects within indicated years. In contrast, the estimates reported for critical years (2014, 2016, and 2019) are point-in-time effects for those specified years and are not cumulative. Standard errors are shown in brackets and control-group means are shown in gray. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

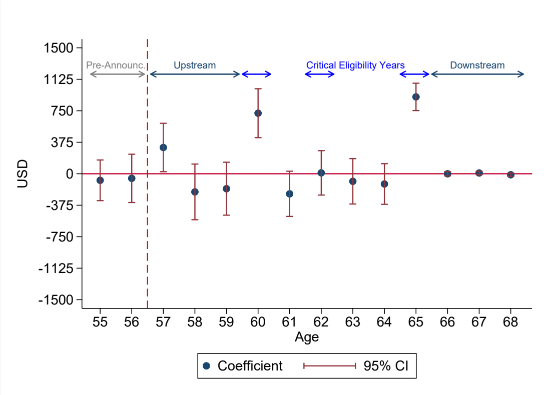
(a) Labor Earnings



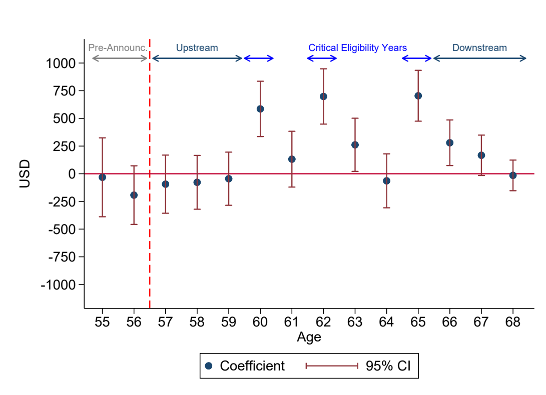
(b) Pension Benefits



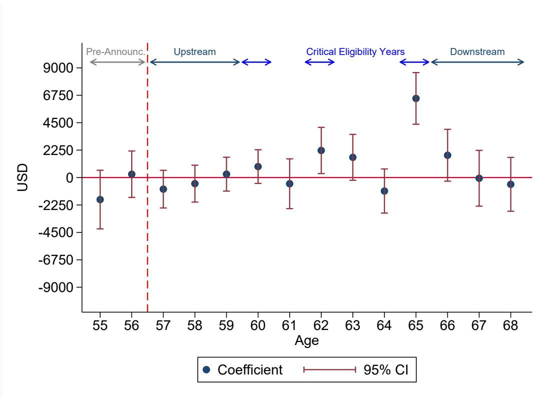
(c) Welfare Benefits



(d) Contributions to Retirement Plans



(e) Total Saving



(f) Net Taxes

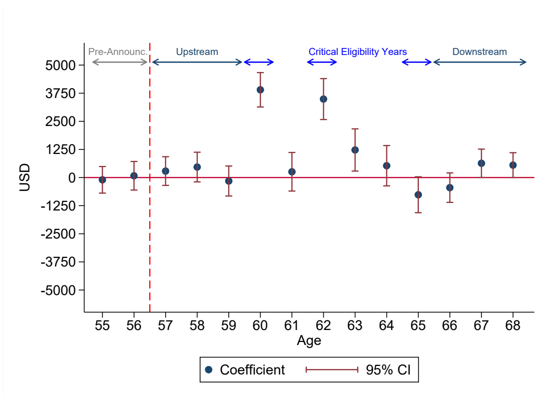


Figure 10: Year Effects for Key Outcome Variables

Notes: The figure reports estimates of β_j from the annual RDD specification in equation (4) by outcome variable and year. Each coefficient measures the discontinuity, in USD, at the January 1, 1954, birthdate cutoff, comparing individuals born just after the cutoff with those born just before it.

Midstream. Column (5) shows more moderate and less precisely estimated responses during the midstream period. Labor earnings increase, although the estimate is not statistically significant. Panel 10a, however, reveals a significant increase in labor earnings at the isolated midstream age of 63, suggesting that the reform's effects extend beyond adjustments within the newly restricted pension claiming windows. This increase in labor supply coincides with further postponement of pension claiming, helping to explain the continued decline in pension benefits. Overall, the midstream period appears to reflect a continuation of the adjustments observed during the critical years, albeit with smaller magnitudes and less precise estimates.

Downstream. In the downstream period reported in column (6), labor earnings increase by a further 2,645 USD, while pension benefits continue to decline due to further postponement of claiming. However, Panel 10a shows heterogeneity across downstream ages: the earnings response is concentrated in the earliest downstream years—particularly at age 66—and largely disappears by ages 67–68. Similar patterns emerge for saving and fiscal outcomes.

Overall Response. Column (7) summarizes the cumulative effects over the full period. The reform leads to a substantial and statistically significant increase in total labor earnings of 19,438 USD, accompanied by a reduction in pension benefits of 9,881 USD. Net tax payments rise by 10,580 USD, reflecting both higher earnings and lower transfer receipt. Total saving increases by 10,608 USD. As shown in Table 6 and in Figure 11, nearly all of this increase is accounted for by higher pension wealth accumulation. Of the total increase in saving, 9,707 USD (91.5%) reflects additional pension wealth accumulation, driven by higher contributions (2,911 USD), postponed pension withdrawals (3,662 USD), and associated returns (2,529 USD), calculated as the difference between the total change in pension wealth and the sum of contributions and reduced withdrawals. In contrast, non-pension wealth rises by a more modest and statistically insignificant 1,252 USD. Overall, the results indicate that the reform leads to a meaningful reallocation from public transfers toward labor income and private saving, with most of the additional saving occurring within retirement accounts.

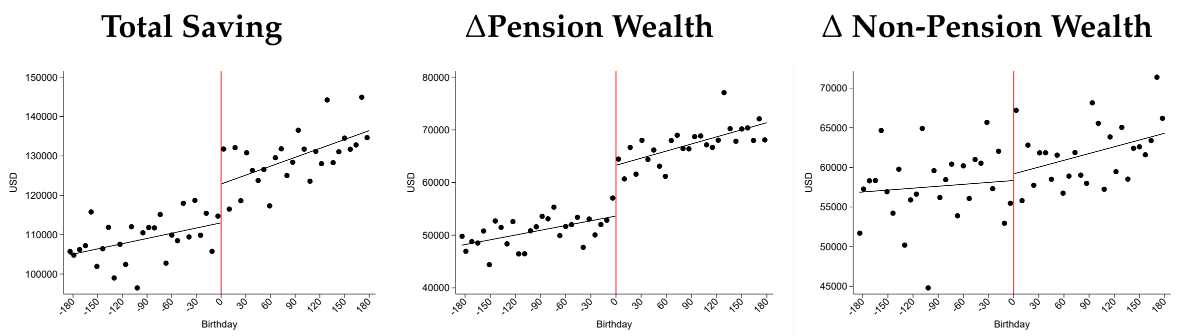


Figure 11: RDD Plots of Overall Saving Response (Ages 57–68)

Notes: This figure presents RDD plots for total saving and its decomposition into pension wealth and non-pension wealth over ages 57–68. Each dot represents the average outcome within a one-week birth cohort. The vertical red line marks the January 1, 1954 birth-date cutoff. The fitted lines show local linear regressions estimated separately on either side of the cutoff using the underlying individual-level data without controls.

Table 6: Decomposition of the Overall Saving Response (Ages 57–68)

	Response	Decomposition (%)
Total Saving	10,608***	100.0
Change in Pension Wealth	9,707***	91.5
Contributions	2,911***	
Reduced Withdrawals	3,662***	
Capital Gains	2,529*	
Change in Non-Pension Wealth	1,252	11.8
Residual	-351	-3.3

Notes: The table decomposes the estimated cumulative increase in total saving over ages 57–68 (2011–2022). Shares are calculated relative to the overall effect on total saving of 10,608 USD. The contribution of withdrawals is reported as the reduction in withdrawals (i.e., $-1\times$ the estimated effect on withdrawals), implying that delayed withdrawals increased accumulated pension wealth by 3,662 USD. The residual reflects that the reported components are estimated separately rather than derived from a common accounting identity, and, therefore, do not mechanically equal the sum of the reported components. Returns are calculated as the difference between the total change in pension wealth and the sum of contributions and reduced withdrawals.

Robustness. Appendix E demonstrates that our results summarized in Table 5 are robust to alternative specifications. First, we replace the baseline local linear specification with local second-degree polynomial regressions on either side of the cutoff (see, e.g., Rabaté et al. (2024)). Second, we implement the robust bias-corrected estimator with triangular weighting following Calonico et al. (2014). The corresponding estimates, reported in Tables 12 and 13, remain very similar to the baseline results. Third, Figure 14 provides further evidence of the robustness by showing that the overall responses of our main outcomes over ages 57–68 remain stable across alternative bandwidth choices.

6.3 Heterogeneity in the Dynamic Response

This section examines how dynamic responses to the reform vary across economically meaningful subgroups. Table 7 summarizes the results by liquid wealth (Panel A) and pension wealth (Panel B).

Liquid wealth. Panel A shows stronger short-run responses among individuals with low liquid wealth. At age 60, their earnings increase more than those of high-liquid-wealth individuals, consistent with tighter liquidity constraints. They also exhibit a larger rise in welfare benefit receipt, indicating greater reliance on transfers when retirement is delayed. Differences narrow at later ages: earnings responses and cumulative earnings are similar across groups, suggesting that liquidity constraints matter primarily at early eligibility thresholds.

Pension wealth. Panel B shows that responses align with institutional incentives. Individuals with low pension wealth respond most strongly at the ERA, while those with high pension wealth respond more at the IERA, when means-testing is relaxed. Overall, cumulative earnings responses are substantially larger for the low-pension-wealth group, indicating greater career extension. Welfare reliance also increases more for this group. These findings suggest that private pension wealth dampens labor-supply responses to higher public retirement ages.

Table 7: Heterogeneity in Responses to the Reform

	Upstream	Critical Years			Midstream	Downstream	Overall Response
Years	2011–13	2014	2016	2019	2015, 2017–18	2020–22	2011-22
Ages	57–59	60	62	65	61, 63–64	66–68	57-68
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: By Liquid Wealth							
Labor Earnings							
Bottom quintile	1,182	5,721***	4,155***	3,685***	3,171	28	16,032
Top quintile	-2,769	3,397**	3,748**	3,800**	-3,937	4,662	9,881
Welfare Benefits							
Bottom quintile	665	1,249***	486	1,577***	1,091	-51	5,581**
Top quintile	-1,500*	702**	-138	636***	-1,017	-35	-1,602
Total Saving							
Bottom quintile	-2,365	1,733	2,111	4,419**	-5,437*	5,079*	5,391
Top quintile	-3,496	939	1,758	13,914***	1,869	2,347	18,243
Net Taxes							
Bottom quintile	500	3,850***	2,805***	-271	1,488	-745	6,168
Top quintile	907	3,060***	3,768***	-586	1,927	3,857*	13,788
Panel B: By Pension Wealth							
Labor Earnings							
Bottom quintile	2,381	8,803***	3,044**	3,327***	4,200	2,551	25,989***
Top quintile	-5,537	4,114**	5,373***	4,588**	611	3,580	11,690
Welfare Benefits							
Bottom quintile	640	1,514***	134	1,758***	215	-11	3,971
Top quintile	-176	135	231	725***	-217	23	968
Total Saving							
Bottom quintile	1,885	2,910**	535	4,257**	-3,048	1,915	8,511*
Top quintile	-3,977	-224	6,620**	2,674	1,711	6,660	13,487
Net Taxes							
Bottom quintile	669	5,947***	2,096**	-568	2,229	657	11,873*
Top quintile	-695	2,975***	4,044***	-575	1,598	-302	7,281

Notes: The table reports the reform's estimated local treatment effects from the RDD specified in equation (4), where the estimation sample includes all individuals born within six months on either side of the cutoff. Estimates are shown by subgroups defined by liquid wealth (Panel A) and pension wealth (Panel B). Individuals are classified into quintiles based on pre-reform measures (2010), and we report results for top and bottom quintiles. Outcomes are aggregated over different phases of adjustment period following the reform: upstream (2011-2013), midstream (2015,2017,2018), downstream (2020-2022), and the full period (2011-2022). For these periods, coefficients represent cumulative effects within indicated years. In contrast, the estimates reported for critical years (2014, 2016, and 2019) are point-in-time effects for those specified years and are not cumulative. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

7 Determinants of Reform Effects

7.1 Labor Response

Model Consistency and Underlying Mechanisms. The labor response aligns with the model in two key respects: we find no upstream adjustment, while bunching is strongest at the ERA and shifts by six months with the ERA and IERA to the new thresholds. The main deviation concerns the FRA, where the model predicts no retirement bunching because there is no discrete retirement incentive. Empirically, however, we observe bunching and a corresponding shift in mass at the FRA, which the theoretical framework cannot rationalize. Finally, the model cannot account for the economically meaningful positive labor responses in the midstream and downstream periods.

Several mechanisms could account for the excess bunching at the FRA. One potential explanation is that occupational (second-pillar) pensions create retirement incentives at the FRA through kinks in the budget constraint. However, these pensions are largely annuitized, approximately actuarially fair, and generally lack age-based discontinuities, making this explanation unlikely.

A second possibility is demand-side factors that concentrate retirements at the FRA, such as employer-driven retirement policies (e.g., Rabaté, 2019), particularly where employment protection weakens after the FRA. Such mechanisms appear less compelling in the Danish context, where neither mandatory retirement nor a decline in employment protection occurs at the eligibility ages. While more subtle employer-side responses cannot be ruled out, Danish law prohibits age discrimination in employment, suggesting a limited role for this channel.

A third explanation is borrowing constraints. If individuals cannot smooth consumption, delayed eligibility may force them to work until pension access, potentially amplifying bunching. However, our heterogeneity results provide little support for this mechanism at the FRA. Figure 12 shows similar bunching shifts across liquid-wealth groups, and Table 7 reports comparable earnings responses and strong saving responses for both groups. By contrast, at the ERA, low-liquid-wealth individuals respond somewhat more strongly, consistent with tighter liquidity constraints. Overall, borrowing constraints may matter at the ERA but are unlikely to explain much of the bunching at the FRA.

The limited plausibility of these standard non-behavioral explanations points to behavioral mechanisms—such as social norms and reference-dependent preferences—as important drivers of bunching at the FRA, consistent with Behaghel and Blau (2012); Lalive et al. (2023); Seibold (2021). These behavioral channels help explain pronounced

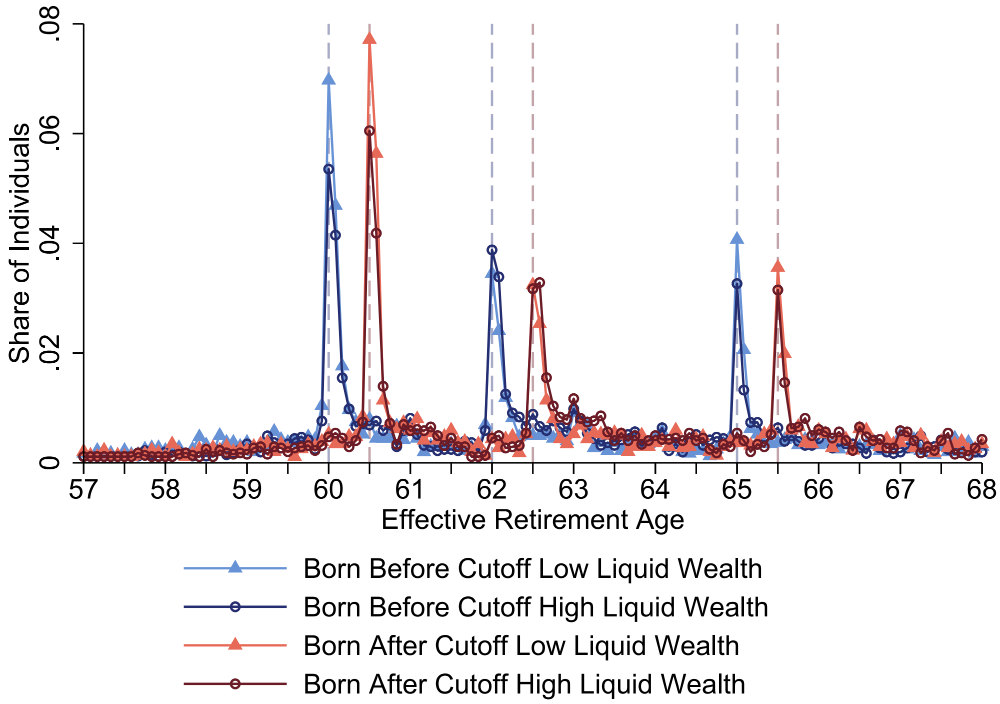


Figure 12: Reform Effect on the Distribution of Effective Retirement Age by Liquid Wealth

Notes: This figure shows the distribution of effective monthly retirement age for four groups: individuals born within six months before and after the January 1, 1954 cutoff, each split into low- and high-liquid-wealth individuals. Effective monthly retirement age is defined based on the timing of when individuals stop receiving labor income, as described in detail in Appendix B.

bunching at the FRA despite the absence of sharp eligibility incentives. While financial incentives and discrete notches are strong at the ERA and IERA, we cannot rule out that social norms and reference points also contribute to bunching at these ages.

Reconciling with the Literature. Following Rabaté et al. (2024), we present Table 8, which situates our employment results for the restricted periods between old and new eligibility ages within the broader international evidence on retirement-age reforms. Two points are worth highlighting.

First, for reforms that increase eligibility ages, the international evidence in Table 8 reports employment effects of roughly +6 to +33 pp. Our estimates range from +8.8 to +17.1 pp across the three Danish eligibility ages, with the largest effect at the ERA (+17.1 pp), placing them near the middle of the international distribution. These level effects partly reflect the relatively high pre-threshold employment rates in our setting and imply a comparatively strong response per year of age increase. Second, unlike settings where behavior concentrates around a single early and a single normal or statutory age, the coexistence of three distinct pension eligibility ages, the ERA, IERA, and FRA, likely spreads adjustment across margins. Consistent with this interpretation, retirement hazards in our setting range from 0.09 to 0.14, and bunching rates from 5 to 8 percent, generally below the substantially higher hazard reductions and

Table 8: Results of Related Studies on Effects near Eligibility Ages

Study	Country	Reform	Method	Results: Employment rate (pp)	Results: Retirment rate (pp)	At ERA, IERA, or FRA: Employment rate[a] (%)	At ERA, IERA, or FRA: Hazard rate[b] (Level)	At ERA, IERA, or FRA: Bunching[c] (%)
Staubli and Zweimüller (2013)	AUT	♂: ERA 60 → 62	DID	9.8	−24.8	28	0.5	14
		♀: ERA 55 → 58+3m	DID	11.0	−25.4	57	0.25	14
Vestad (2013)	NOR	ERA 64 → 62	DID	33.2	×	65	0.46	30
Atalay and Barrett (2015)	AUS	♀: FRA 60 → 65	DID	7.7	×	30–50	×	×
Cribb et al. (2016)	UK	♀: ERA 60 → 62	DID	6.3	−11.5	55	0.25	14
De Vos et al. (2019)	NLD	FRA 65 → 65+6m	DID	10	×	43	0.35	15
Rabaté and Rochut (2020)	FRA	FRA 60 → 61	DID	20.9	−47.8	45	0.5	23
Geyer and Welteke (2021)	GER	♀: ERA 60 → 63	RDD	13.5	−27.6	62	0.19	12
Rabaté et al. (2024)	NLD	ERA 65+m → 66	RDD	21.2	−59.5	29	0.72	21
This paper	DK	ERA 60 → 60+6m	RDD	17.13	−22.33	86	0.1	8
This paper	DK	IERA 62 → 62+6m	RDD	10.33	−10.66	61	0.09	6
This paper	DK	FRA 65 → 65+6m	RDD	8.78	−15.82	35	0.14	5

Notes: The table summarizes estimates from relevant studies examining reforms that increase eligibility ages. Reported employment and retirement effects are measured near the relevant eligibility age.
[a] The employment rate just before the eligibility age.
[b] The drop in the share of employed person at the eligibility age over the share of the employed persons just before it.
[c] The share of employed persons retiring at the eligibility age.

bunching rates observed in several other countries.

Finally, the reform-induced reductions in retirement rates are of a similar order of magnitude to the employment gains, ranging from −10.7 to −22.3 pp. This pattern indicates that the policy largely operated by delaying retirement into continued employment, consistent with its intended purpose.

This evidence primarily concerns responses around eligibility thresholds. A related question is whether retirement-age reforms generate adjustments outside the newly restricted claiming windows. Consistent with our findings, the literature generally reports limited upstream responses. Rabaté et al. (2024) and Staubli and Zweimüller (2013) find little to no upstream employment adjustment following reforms implemented late in the life cycle, while Mastrobuoni (2009) and Geyer and Welteke (2021) find similarly small responses despite longer announcement horizons. An important exception is Carta and De Philippis (2021), who documents substantial anticipatory responses. Previous studies have attributed limited upstream adjustment to labor-market frictions and institutional rigidities. Our model, which embeds the financial retirement incentives in Denmark, generates little upstream labor-supply response even without such frictions, and our empirical findings are consistent with this prediction. Downstream, however, we somewhat depart from previous evidence. While existing studies generally find little adjustment after the new eligibility age (Staubli and Zweimüller, 2013; Rabaté et al., 2024), we document significant downstream labor-earnings effects and some midstream adjustment. Our results therefore suggest that retirement-age reforms can affect labor supply over a broader part of the life cycle than the newly restricted claiming windows alone would imply.

Overall, the six-month increase in eligibility ages raises the effective retirement age by 3.3 months, implying an elasticity slightly above one-half. This is close to the elasticity of about one-half estimated by Mastrobuoni (2009) and Lalive et al. (2023) for FRA increases in the United States and Switzerland, respectively, and above the 0.21 estimated by Rabaté (2019) for the Netherlands.

7.2 Spillovers to welfare programs

Model Consistency and Underlying Mechanisms. Overall, the spillover patterns into welfare programs line up closely with the model's qualitative predictions. We observe clear passive substitution in the periods newly constrained by the reform, consistent with delayed pension eligibility mechanically prolonging non-pension benefit spells for individuals who would otherwise have claimed ERP or OAP. Our transition-based estimates further show that induced entry from employment into welfare benefits is concentrated around the ERA, while it is markedly weaker around the IERA and essentially absent around the FRA. This heterogeneity across thresholds follows naturally from the reform-induced incentive changes and the institutional setting: conditional on a given exit age, the relative consumption advantage of remaining on welfare rather than claiming pension benefits is largest in the newly restricted window around the ERA, much smaller around the IERA, and can even turn negative around the FRA.

Reconciling with the Literature. A number of related studies document substantial passive substitution, but only limited active substitution into welfare programs following increases in pension eligibility ages (Rabaté et al., 2024; Geyer and Welteke, 2021; Manoli and Weber, 2016b; Oguzoglu et al., 2020). Our findings are broadly consistent with this pattern, particularly at the IERA and FRA, where changes in welfare receipt primarily reflect mechanical delays in pension claiming rather than new benefit take-up. At the ERA, however, we find evidence of active substitution from employment into welfare: individuals employed just before the cutoff are 0.62 pp more likely to receive benefits six months later. This active substitution accounts for 12% of the total welfare spillover (+5.23 pp). Duggan et al. (2007) similarly document increased entry into DI following increases in retirement ages in the United States.

7.3 Saving and wealth

Model Consistency and Underlying Mechanisms. Since the reform extends careers and thereby increases lifetime income, the model predicts lower upstream saving and higher upstream consumption as forward-looking individuals smooth the gains from extended employment over the remaining life cycle. Empirically, we obtain a negative upstream saving estimate, although it is not statistically significant. The model further predicts positive saving responses during periods of extended employment and higher labor income. While these responses are expected to be concentrated within the newly restricted claiming windows around the ERA and IERA, the observed labor-supply effects extend beyond these windows into the restricted period around the FRA and the midstream and downstream periods. Accordingly, consumption smoothing also predicts positive saving responses in these later periods, which is what we observe. Overall, the savings results are broadly consistent with the predictions of standard life-cycle theory.

Reconciling with the Literature. The dynamic pattern of saving responses provides support for consumption smoothing, with lower saving upstream and higher saving as employment and labor income increase. This interpretation is also consistent with Etgeton et al. (2023), who documents lower pre-retirement saving following an ERA increase in Germany, and Nakazawa (2025), who finds higher non-pension saving during restricted claiming years in Japan. However, the upstream estimate is imprecise, so we cannot reject the saving inertia hypothesis proposed by García-Miralles and Leganza (2024), who studies the same Danish reform around the ERA and IERA. Under saving inertia, individuals do not adjust consumption and saving in anticipation of higher future earnings but instead continue their pre-reform saving behavior as employment is extended. This channel is particularly relevant in the context of Danish quasi-mandatory occupational pensions, where extended employment mechanically prolongs contributions (Chetty et al., 2014). The concentration of additional wealth accumulation in these accounts is therefore compatible with saving inertia. Overall, the savings response favors consumption smoothing as the underlying mechanism, but the imprecision of the upstream estimate prevents us from rejecting saving inertia.

8 Conclusion

This paper studies the long-run effects of a six-month increase in three pension eligibility ages in Denmark. Using a regression discontinuity design and following individuals for 12 years after the reform announcement, we examine adjustment in labor supply, benefit claiming, private saving, and public finances.

We find no significant anticipatory labor-supply response, while employment increases strongly in the newly restricted claiming windows. Importantly, these effects persist beyond the directly affected periods, with positive midstream and downstream responses. Retirement bunching shifts with the reform at all three eligibility ages and is strongest at the ERA, followed by the IERA and FRA. Welfare spillovers are comparatively limited and largely reflect longer benefit receipt among existing claimants rather than active substitution out of employment. Together, these responses raise the average effective retirement age by 3.3 months and generate substantial fiscal gains.

The variation across eligibility ages also sheds light on the mechanisms underlying retirement responses. The gradient in bunching mirrors the gradient in financial incentives, consistent with an important role for these incentives in shaping retirement decisions. Yet, pronounced bunching at the FRA shifts with the reform despite the absence of a discrete financial incentive, showing that financial incentives alone are insufficient. With standard demand-side and liquidity-based explanations appearing less compelling at this age, the evidence points to an additional role for nonfinancial mechanisms, including social norms and reference dependence.

Finally, extended employment is accompanied by substantial additional wealth accumulation, concentrated in occupational pension accounts. The dynamic saving response broadly supports consumption smoothing, although the imprecision of the upstream response prevents us from rejecting , defined as the continuation of pre-reform saving behavior. More broadly, our results show that evaluating increases in pension ages requires looking beyond their immediate effects on pension claiming. Their consequences unfold across multiple eligibility thresholds and over a longer horizon, affecting employment, welfare reliance, private wealth, and ultimately the fiscal effectiveness of pension reform.

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Appendix A: Pension Wealth Imputation

This appendix outlines the method used to impute after-tax pension wealth in years prior to 2014 using administrative data. The same approach applies more generally to imputing wealth at the end of year t based on observed values in year $t+1$ and recorded pension flows in year t.

We define w_{post}^{t+1} as after-tax pension wealth at the end of year $t+1$, as reported in variable `wea_pen_tot_net` (FCGX). For this variable Statistics Denmark applies a 60% weight to pre-tax pension wealth, corresponding to an assumed average withdrawal tax rate of 40%. Contributions to annuity-based (Pillar 2–3) pensions in year t, denoted c_t^{annu}, are recorded as the sum of `QARBPENL`, `QRIPENL`, and `ATPSAML`. Withdrawals from annuities in the same year, inc_t^{annu}, are given by `QANDPENS`. Contributions to non-Roth-style lump-sum accounts, c_t^{lump}, are measured as `QPRIPENK` plus `QARBPENK`, while withdrawals, inc_t^{lump}, are drawn from `KAPPEN`. Roth-style pension contributions in year t, c_t^{roth}, include `PRIPEN33` and `ARBPEN4445`. We assume Roth withdrawals inc_t^{roth} are zero before December 31, 2014. The monthly after-tax return on pension wealth in year t is denoted $r_{m,t}$.

Several assumptions underlie our imputation procedure. First, annuity contributions and withdrawals are assumed to be evenly distributed throughout the year, whereas lump-sum and Roth-style flows occur mid-year. Second, we assume annuity and lump-sum contributions are taxed at 40%, so only 60% enters the account (consistent with Statistics Denmark conventions). Roth contributions are post-tax and enter fully. Withdrawals from annuity and lump-sum accounts are also taxed at 40%, so 60% is retained. Roth withdrawals are tax-free and fully retained. Finally, annuity flows are capitalized over 5.5 months, lump-sum and Roth flows over 6 months, and final wealth is measured at the end of the year (12-month capitalization).

The imputation of after-tax wealth at the end of year t, denoted w_{post}^t, relies on the following identity:

$$w_{post}^{t+1} = w_{post}^t \cdot (1 + r_{m,t})^{12} + \text{net\_flow}_t,$$

which can be rearranged to yield:

$$w_{post}^t = \frac{w_{post}^{t+1} - \text{net\_flow}_t}{(1 + r_{m,t})^{12}}.$$

The term net_flow_t captures the net after-tax contributions to pension wealth during year t, adjusted for timing and returns. Specifically, we construct:

$$\begin{aligned}\text{net\_flow}_t = & \; 0.6 \cdot (c_t^{annu} - inc_t^{annu}) \cdot (1 + r_{m,t})^{5.5} \\ & + 0.6 \cdot (c_t^{lump} - inc_t^{lump}) \cdot (1 + r_{m,t})^{6} \\ & + c_t^{roth} \cdot (1 + r_{m,t})^{6}.\end{aligned}$$

Each term reflects the average time the funds remain in the account and their respective after-tax values. If only annual returns $r_{annual,t}$ are available, we approximate the monthly capitalization as:

$$(1 + r_{m,t})^x \approx (1 + r_{annual,t})^{x/12},$$

where x denotes the relevant capitalization period in months.

The monthly after-tax return $r_{m,t}$ is based on gross annual returns provided by the pension fund P+ and adjusted for the Pension Return Tax (PAL). Table 9 summarizes the applicable rates.

Table 9: Annual and Monthly After-Tax Returns on Pension Wealth

Year	PAL Tax Rate	Annual Return (%)	Annual After-Tax (%)	Monthly After-Tax (%)
2014	15.3%	9.50	8.05	0.644
2013	15.3%	-1.50	-1.50	-0.130
2012	15.3%	4.50	3.81	0.313
2011	15.0%	12.10	10.29	0.816
2010	15.0%	6.60	5.61	0.451
2009	15.5%	-1.60	-1.60	-0.130
2008	15.0%	-5.40	-5.40	-0.460

Appendix B: Effective Retirement Date Imputation

Most of the administrative registers used in this article are available at an annual frequency. However, for employees in salaried jobs, we also observe monthly information from the income register BFL, which allows us to measure the timing of effective retirement more precisely. For these individuals, we define the retirement month as the first month t with zero reported hours worked, conditional on positive hours in month $t - 1$. To ensure this transition reflects a persistent labor market exit, we impose two additional forward-looking criteria. First, there must be no subsequent 12-month period in which the average workload exceeds 50 percent. Second, cumulative future earnings from self-employment must not exceed 20,000 USD:

$$\text{ret\_month} \;=\; \min\{\, t \;:\; \text{hours}_t = 0 \;\;\wedge\;\; \text{hours}_{t-1} > 0 \;\;\wedge\;\; \Omega_t \}$$

Where Ω_t represents the forward-looking criteria:

$$\Omega_t \;\equiv\; \left(\max_{R^W \geq t} W_R^W < 0.5\right) \;\wedge\; \left(\sum_{k \geq \text{year}(t)} \text{SE\_earnings}_k < 20000\right).$$

For any given month R^W following the candidate retirement month t ($R^W \geq t$), we define W_R^W as the rolling workload share over the 12-month period beginning in month R^W. This share is calculated relative to a standard annual Full-Time Equivalent (FTE) benchmark of 1,665 hours (37 hours per week $\times$ 45 weeks). To account for right-censoring, either due to the end of the sample period or the individual's death, we restrict the forward-looking sum to the final observable month T_i. We also proportionally shrink the FTE denominator to match the actual number of months observed in this truncated window:

$$W_R^W \;\equiv\; \frac{\sum_{s=R^W}^{\min(R^W+11, T_i)} \text{hours}_s}{(37 \times 45) \times \left(\frac{\min(R^W+11, T_i) - R^W + 1}{12}\right)}$$

This baseline definition for salaried workers does not apply to the following three categories of workers:

(1) Strictly self-employed: Individuals who never record positive hours in salaried employment.

(2) Post-salaried self-employment: Individuals who stop providing salaried work but continue in self-employment, generating more than 20,000 USD in future self-employment income.

(3) Right-censored observations: Individuals who do not retire before the end of our sample period in 2022.

For individuals in categories (1) and (2), we apply a modified retirement definition based solely on self-employment income. For these individuals, we identify the retirement year as the final year in the dataset with non-zero self-employment income, provided this year is prior to the end of the sample (2022):

$$\text{ret\_year} \;=\; \max\left\{\, t \;:\; \text{SE\_earnings}_t \neq 0 \right\}, \quad \text{given } t < 2022.$$

To pin down the timing within this retirement year, we rely on the ratio of annual self-employment income in the retirement year relative to the income in the immediately preceding year:

$$\lambda \equiv \frac{\text{SE\_earnings}_{\text{ret\_year}}}{\text{SE\_earnings}_{\text{ret\_year}-1}}$$

The retirement month is then imputed based on this ratio. Because we only observe labor market states at a monthly frequency, we restrict the output to discrete integers. If the ratio implies negative income ($\lambda \leq 0$) or suggests income grew compared to the previous year ($\lambda > 1$), we assign a mid-year retirement month of June (Month 6). Otherwise, the fraction of the year worked is assumed to be proportional to the income ratio, rounded to the nearest whole month:

$$\text{ret\_month} = \begin{cases} 6 & \text{if } \lambda \leq 0 \;\vee\; \lambda > 1 \\ \text{round}(\lambda \cdot 12) & \text{if } 0 < \lambda \leq 1 \end{cases}$$

To illustrate the empirical coverage of this imputation procedure, Table 10 reports the distribution of individuals across the four resulting classification categories. The majority of the sample consists of wage earners, for whom retirement timing is identified using the monthly hours-worked condition. A smaller share is classified under the self-employment criteria, comprising both strictly self-employed individuals and those transitioning to self-employment, where the annual earnings ratio is applied. Finally, the table accounts for a subset of inactive individuals with no recorded labor market activity after 2007, as well as right-censored observations that do not retire before the end of the observation period in 2022.

Table 10: Retirement Category Distribution for January 2008

Category	**Count**	**Share %**	**Cumulative %**
Wage Earner	33607	80.50	80.50
Selfemployed	979	2.35	82.84
no work post 2007	1224	2.93	85.78
No retirement in 2008-2022	5938	14.22	100.00
Total	41748	100.00	100.00

Notes: This table reports the distribution of the sample across the four main retirement classification criteria defined in Section B.

Appendix C: Placebo Tests

Table 11: Placebo Tests in Pre-reform Announcement Years

	Baseline		Quadratic		RDrobust	
Years	2009	2010	2009	2010	2009	2010
Ages	55	56	55	56	55	56
Column	(1)	(2)	(3)	(4)	(5)	(6)
A. Income						
Labor Earnings	-216	-231	346	191	119	68
	(516)	(570)	(685)	(756)	(829)	(923)
	60,723	60,230	60,723	60,230	60,747	60,230
Pension Benefits	-84	-74	-67	-62	-75	-69
	(56)	(57)	(74)	(75)	(78)	(84)
	257	298	257	298	259	300
Welfare Benefits	-78	-55	-71	-72	-86	-79
	(124)	(147)	(164)	(195)	(200)	(244)
	2,053	2,673	2,053	2,673	2,042	2,664
B. Saving						
Contributions to Retirement Plans	-32	-193	338	-2	181	-57
	(182)	(135)	(241)	(179)	(288)	(212)
	9,446	8,430	9,446	8,430	9,451	8,439
Total Saving	-1,807	273	-1,823	780	-1,854	543
	(1,227)	(972)	(1,629)	(1,291)	(1,929)	(1,521)
	-34,781	1,496	-34,781	1,496	-34,689	1,407
C. Fiscal Externalities						
Net Taxes	-101	79	165	217	51	191
	(301)	(324)	(400)	(430)	(486)	(525)
	19,592	18,242	19,592	18,242	19,613	18,246

Notes: This table reports placebo estimates from the RDD specified in equation (4), using pre-reform announcemnt years (2009-2010) where no treatment effect is expected. Columns (1)-(2) report baseline local linear estimates, columns (3)-(4) allow for local second-degree polynomial regressions on both sides of the cutoff, and columns (5)-(6) report robust bias-corrected estimates and triangular weighting following Calonico et al. (2014). Standard errors are shown in brackets and control-group means are shown in gray. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

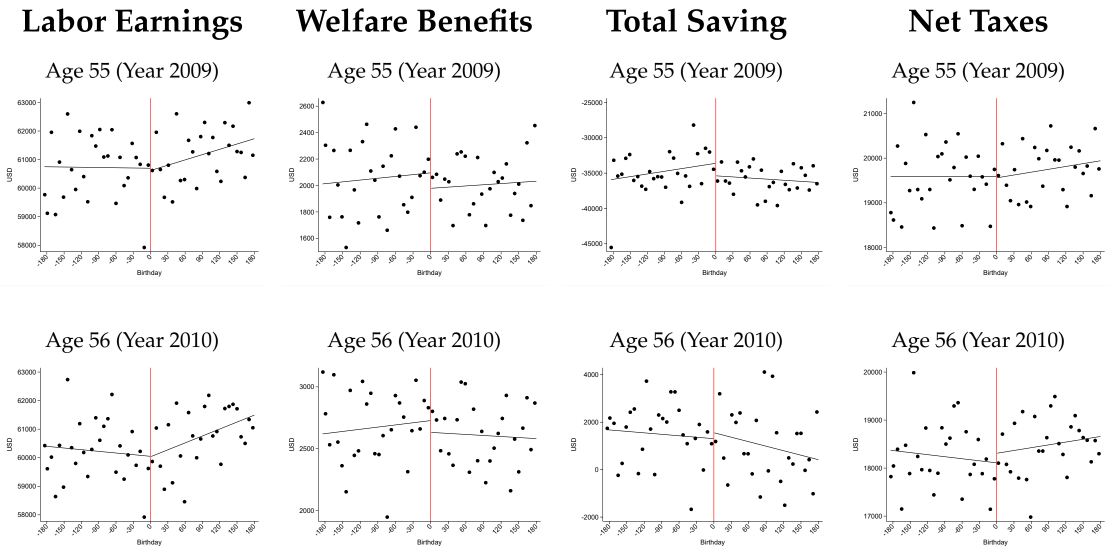

Figure 13: Placebo RDD plots by outcome for pre-reform years 2009 and 2010.

Notes: This figure plots the average USD amounts of outcome variables by one-week birth bins for pre-reform years 2009 and 2010. Each dot represents the mean outcome within a weekly cohort. The red vertical lines correspond to the January 1, 1954, birth date cutoff. Local linear regressions are fitted on either side of the cutoff using the underlying (unbinned) data without controls.

Appendix D: Effect on Average Retirement Age

This appendix outlines the procedure used to aggregate the age-specific employment effects into a single measure representing the change in the average retirement age. The methodological derivation closely follows the approach detailed in Rabaté et al. (2024) and Mastrobuoni (2009). We rely on the estimated coefficients obtained from the RDD specification presented in Section 5.1.

$$y_{ij} = \alpha + \beta_j \cdot \mathbf{1}[x_i \geq c] + \gamma_j \cdot (x_i - c) + \delta_j \cdot \mathbf{1}[x_i \geq c] \cdot (x_i - c) + Z_i'\theta + \varepsilon_{ij} \tag{5}$$

When the dependent variable y_{ij} in Equation (5) is specified as a binary indicator for employment at a given age in months j, the coefficient of interest β_j captures the causal effect of the eligibility age increase on the probability of remaining employed at that specific age.

Thus, β_j represents the reform induced increase in the probability that an individual retires after age j. Let R denote the random variable representing the observed retirement age (in months) for individuals exposed to the reform, and let R^{cf} denote the counterfactual retirement age for the same group had the reform not been implemented. We can express this relationship as:

$$\Pr(R > j) = \Pr(R^{cf} > j) + \beta_j \tag{6}$$

The total effect of the policy on the average retirement age, denoted as Δ, is the difference in the expected value of the retirement age with and without the reform. Summing over our specific period of interest, from the announcement of the reform at age 57 (684 months) up to age 68 (816 months), we get:

$$\Delta = \sum_{j=684}^{816} j \cdot \Pr(R = j) - \sum_{j=684}^{816} j \cdot \Pr(R^{cf} = j)$$

$$\Delta = \sum_{j=684}^{816} j \cdot \left[\Pr(R = j) - \Pr(R^{cf} = j)\right] \tag{7}$$

Using the property of the standard cumulative distribution function, the probability of retiring exactly in month j can be rewritten as the probability of not having retired by month $j-1$ minus the probability of not having retired by month j. That is:

$$\Pr(X = j) = \Pr(X > j-1) - \Pr(X > j). \tag{8}$$

Substituting this into Equation (7) and applying the relationship from Equation (6) yields:

$$\begin{aligned} \Delta &= \sum_{j=684}^{816} j \cdot \Big[\big(\Pr(R > j-1) - \Pr(R^{cf} > j-1)\big) - \big(\Pr(R > j) - \Pr(R^{cf} > j)\big)\Big] \\ &= \sum_{j=684}^{816} j \cdot (\beta_{j-1} - \beta_j) \end{aligned} \tag{9}$$

Assuming that the reform only affects retirement behavior within this specific age window bounded by 684 and 816 months (meaning $\beta_j = 0$ for all $j \leq 684$ and $j \geq 816$), this expression expands into a telescoping sum:

$$\begin{aligned} \Delta &= \sum_{j=684}^{816} j \cdot (\beta_{j-1} - \beta_j) \\ &= 684(0 - \beta_{684}) + 685(\beta_{684} - \beta_{685}) + \cdots + 816(\beta_{815} - 0) \\ &= \sum_{j=684}^{815} \beta_j \end{aligned} \tag{10}$$

Thus, the overall causal effect of the policy reform on the average retirement age simplifies mathematically to the sum of the age-specific coefficients (β_j) estimated via our RDD models across the relevant age window.

Appendix E: Robustness

Table 12: Robustness to Local Quadratic Specification

	Upstream	Critical Years			Midstream	Downstream	Overall Response
Years	2011–13	2014	2016	2019	2015, 2017–18	2020–22	2011-22
Ages	57–59	60	62	65	61, 63–64	66–68	57-68
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Labor supply							
Labor Earnings	2,065	6,031***	4,810***	4,145***	4,128	2,772	24,527***
	(2,138)	(846)	(957)	(850)	(2,551)	(1,896)	(7,689)
	169,558	45,283	33,314	16,811	94,813	32,134	391,914
B. Public Benefits Claiming							
Pension Benefits	-118	-3,864***	-2,544***	-726**	-2,023*	-1,019	-10,290***
	(264)	(260)	(395)	(348)	(1,074)	(809)	(2,454)
	2,716	8,378	16,393	21,888	50,431	71,379	171,185
Welfare Benefits	-144	553***	-101	872***	-732	-29	283
	(555)	(197)	(179)	(110)	(499)	(33)	(1,305)
	9,436	2,067	1,680	30	4,857	70	18,141
C. Saving							
Contributions to Retirement Plans	256	701***	696***	560***	501	544*	3,490**
	(478)	(169)	(169)	(155)	(450)	(329)	(1,442)
	22,931	6,188	4,864	2,757	14,297	5,117	56,154
Annuities	427	709***	728***	442***	563	272	3,293**
	(460)	(165)	(161)	(134)	(417)	(273)	(1,338)
	19,972	5,785	4,267	1,978	11,951	3,630	47,583
Lump-sum Withdrawable	-149	-4	-25	120*	-57	241*	201
	(121)	(32)	(39)	(63)	(121)	(129)	(353)
	2,934	398	591	771	2,327	1,480	8,501
Total Saving	-1,388	1,671*	2,904**	5,416***	-783	1,836	9,968**
	(1,833)	(936)	(1,284)	(1,436)	(2,166)	(2,266)	(4,529)
	58,335	5,091	15,247	18,887	27,332	-15,997	108,895
D. Fiscal Externalities							
Net Taxes	1,701	4,062***	3,382***	-513	3,076*	389	12,579***
	(1,244)	(518)	(616)	(541)	(1,646)	(1,175)	(4,801)
	49,927	9,089	522	-4,241	-412	-21,048	33,836
Taxes	620	826***	742**	1,011***	470	681	4,645
	(864)	(306)	(327)	(301)	(863)	(749)	(2,994)
	64,080	19,930	18,740	15,656	55,115	43,230	216,751
Transfers	-1,089	-3,233***	-2,644***	1,501***	-2,668**	330	-7,969***
	(669)	(321)	(397)	(344)	(1,065)	(775)	(2,737)
	13,993	10,799	18,201	19,869	55,434	64,134	182,430

Notes: The table reports the reform's estimated local treatment effectsfrom the RDD specified in equation (4), where we deviate from the baseline local linear specification and instead allow for a local second-degree polynomial regressions on both sides of the cutoff. Outcomes are aggregated over different phases of adjustment period following the reform: upstream (2011-2013), midstream (2015,2017,2018), downstream (2020-2022), and the full period (2011-2022). For these periods, coefficients represent cumulative effects within indicated years. In contrast, the estimates reported for critical years (2014, 2016, and 2019) are point-in-time effects for those specified years and are not cumulative. Standard errors are shown in brackets and control-group means are shown in gray. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 13: Robustness to CCT Bias Correction

	Upstream	Critical Years			Midstream	Downstream	Overall Response
Years	2011–13	2014	2016	2019	2015, 2017–18	2020–22	2011-22
Ages	57–59	60	62	65	61, 63–64	66–68	57-68
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Labor supply							
Labor Earnings	3,382	6,556***	5,097***	3,558***	4,681	2,275	25,500***
	(2,608)	(1,031)	(1,150)	(1,023)	(3,123)	(2,328)	(9,465)
	169,610	45,268	33,320	16,813	94,836	32,220	392,069
B. Public Benefits Claiming							
Pension Benefits	-214	-3,967***	-2,644***	-662*	-2,172	-607	-10,227***
	(298)	(326)	(475)	(391)	(1,337)	(987)	(2,986)
	2,679	8,372	16,384	21,875	50,418	71,323	171,051
Welfare Benefits	588	575**	-17	966***	-431	-54	1,498
	(664)	(232)	(220)	(97)	(627)	(39)	(1,556)
	9,415	2,058	1,677	30	4,853	71	18,105
C. Saving							
Contributions to Retirement Plans	872	922***	748***	483**	740	486	4,372**
	(568)	(207)	(205)	(191)	(550)	(406)	(1,755)
	22,957	6,195	4,867	2,754	14,304	5,128	56,203
Annuities	993*	937***	799***	350**	784	222	4,124**
	(548)	(201)	(195)	(164)	(511)	(340)	(1,630)
	19,993	5,791	4,269	1,972	11,957	3,641	47,622
Lump-sum Withdrawable	-106	-12	-50	128*	-51	223	207
	(145)	(39)	(48)	(76)	(146)	(154)	(423)
	2,940	399	591	774	2,327	1,480	8,510
Total Saving	70	1,681	1,739	5,452***	289	2,303	11,675**
	(2,170)	(1,138)	(1,567)	(1,766)	(2,661)	(2,765)	(5,532)
	58,412	5,125	15,263	18,945	27,257	-15,977	109,024
D. Fiscal Externalities							
Net Taxes	2,188	4,419***	3,570***	-715	3,459*	153	13,223**
	(1,510)	(630)	(737)	(634)	(2,022)	(1,443)	(5,880)
	49,961	9,086	528	-4,237	-419	-20,996	33,923
Taxes	1,256	1,086***	919**	1,001***	971	875	6,168*
	(1,050)	(369)	(389)	(364)	(1,036)	(915)	(3,646)
	64,061	19,912	18,733	15,647	55,105	43,237	216,696
Transfers	-934	-3,311***	-2,638***	1,701***	-2,526*	796	-6,988**
	(802)	(390)	(477)	(391)	(1,325)	(948)	(3,347)
	13,944	10,783	18,188	19,856	55,435	64,088	182,295

Notes: The table reports the reform's estimated local treatment effects from the RDD specified in equation (4), where we implement the robust bias-corrected estimator with triangular weighting following Calonico et al. (2014). Outcomes are aggregated over different phases of adjustment period following the reform: upstream (2011-2013), midstream (2015,2017,2018), downstream (2020-2022), and the full period (2011-2022). For these periods, coefficients represent cumulative effects within indicated years. In contrast, the estimates reported for critical years (2014, 2016, and 2019) are point-in-time effects for those specified years and are not cumulative. Standard errors are shown in brackets. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Figure 14: Robustness of Overall Responses to Bandwidth Choice

Notes: This figure reports the reform's estimated local treatment effects from the RDD specified in equation (4) across alternative bandwidth choices around the January 1, 1954 cutoff. Estimates represent cumulative effects over the full period 2011–2022 (ages 57–68). The solid line reports the estimated coefficient and the shaded area the 95% confidence interval.