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The Effect of Increasing Retirement Age on Households' Savings and Consumption Expenditures

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Abstract

This paper examines how households adjust their savings and consumption expenditure in response to an anticipated increase in the early retirement age (ERA). We examine the 1999 pension reform in Germany, which increased the ERA for women born after 1951 by at least three years. Using the German Income and Consumption Survey, we find a negative impact on private savings of 0.6 percentage points that is driven by married households. We show that households consisting of highly educated women and homeowners are more likely to reduce their savings rates. Furthermore, we find that the treated households increase their leisure spending while maintaining an unchanged level of disposable income. Our findings suggest that the treated households absorb the pension wealth shock without increasing their savings.

JEL-Classification: D14, J14, J26
Keywords: Pension Reform; Early Retirement Age; Savings; Pension Wealth; Consumption Expenditure

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1. Introduction

Due to the ageing population, many OECD countries have increased the statutory retirement age, aiming to prolong working lives and ensure the public pension system's solvency. Simultaneously, policymakers are seeking to incentivize households to increasingly engage in other ways to provide old-age income, notably through private savings. While there has been an extensive literature studying the labor supply responses of pension reforms (e.g., Krueger and Pischke (1992); Coile and Gruber (2004); Staubli and Zweimüller (2013); Manoli and Weber (2016)), there is relatively little knowledge about how households' savings plans respond to changes in the pension system. Theoretically, Feldstein (1974) stresses that the overall effect of public pension wealth on private savings relies on the magnitude of the employment effect. In anticipation of prolonged employment and a shortened retirement duration, households may dissave. In this paper, we ask the question: how do households' private savings change when facing an increase in the early retirement age?

This paper exploits a sizable increase in the early retirement age (ERA) for German women to estimate the response of private savings. In 1999, Germany abolished the old-age pension for women, which provided women an option of retiring early at age 60. After the reform, women born since 1952 onward cannot retire early and must wait until they are at least 63 years old. Only women who were born before 1952 still can retire at age 60 via the old-age pension for women. The reform effectively increases the ERA from 60 to 63 years and is particularly pertinent in answering the question posted by our paper for the following reasons. First, the sharp and large discontinuity in ERA based on birthdates allows us to credibly identify causal effects. Second, in contrast to reforms studied in other empirical studies on the displacement effects of public pension wealth on private savings (Attanasio and Brugiavini (2003); Attanasio and Rohwedder (2003); Feng et al. (2011); Delavande and Rohwedder (2017); Lachowska and Myck (2018)), the abolishment of the old-age pension for women has a relatively large effect on labor supply, hence on lifetime labor earnings.[1] This feature allows us to show direct evidence of dissaving when the adjustment in labor supply absorbs the loss in pension wealth.

To empirically address this question, we use four waves of the repeated cross-sectional data: German Income and Consumption Survey data (1993, 1998, 2003, and 2008). We observe detailed savings, consumption expenditure and income information. We first apply a sharp regression discontinuity (RD) design to estimate the changes at the cohort cutoff using the post-reform waves (2003 and 2008). Subsequently, we use the pre-reform waves (1993 and 1998) and a

[1] Geyer and Welteke (2019) find a sizeable increase in retirement age and large positive employment effects of this reform.

regression discontinuity difference-in-differences (RD-DD) framework to wash out any unobserved correlations between birth year and savings behavior. Our analyses show that households with women younger than age 60 and who were born since 1952 adjust their savings rates downwards by approximately 0.6 percentage points due to the rising ERA. We build a simple economic model which suggests that effects are driven by reform induced changes to the expected life-time income. These differ greatly along several factors. We therefore analyse effects by household composition and other socio-economic variables. We find that the drop in savings rates is driven by married households, who experience a 1.5 percentage points reduction in savings rates due to the reform. While single women do not change their savings rates. Moreover, we find that households with highly educated female members, who have better employment prospects and are also more likely to be financially literate, are more likely to reduce their savings rates. We also find that households with homeownership are more likely to reduce their savings rates, which suggests that income security matters. We further examine how joint retirement plays a role in couples' responses. We find that couples with older husbands and couples where men are the primary earners reduce their savings rates more. This suggests that the changes in expected future household labor earnings are exacerbated for married households due to the spillover effects.

Furthermore, we investigate the mechanisms of the reduction in savings rates by investigating the response in terms of reform induced changes to the expected retirement age, disposable income and consumption expenditure. Using the Survey of Health, Ageing and Retirement in Europe (SHARE), we show suggestive evidence of treated women increasing their expected retirement age and married women increase their expected retirement age more than single women. Additionally, we find that the treated households increase their consumption expenditure while maintaining an unchanged disposable income. Our findings suggest that the expected increase in future labor earnings offsets the anticipated loss of forgone pension benefits due to the reform. Therefore, the treated households absorb the pension wealth shock without increasing their savings.

To validate the causal relationship, we provide results from several robustness checks, including varying model specifications, such as choice of controls, bandwidths, and polynomial orders, and by using an alternative empirical method. We also establish the causality of our estimates by performing a number of placebo tests using placebo samples, including samples of older cohorts with the same age composition and a sample of men born between 1948 and 1955, and using placebo cutoffs.

This paper contributes and relates to three different strands of literature. First, it speaks directly to the studies on the implications of pension reforms that raise the statutory retirement age, including employment responses at the individual level (Mastrobuoni (2009); Staubli and Zweimüller (2013); Manoli and Weber (2016); Geyer and Welteke (2019)), retirement behavior in the household context (Cribb et al. (2016); Lalive and Parrotta (2017); Geyer et al. (2020); Fischer and Müller (2020)) and the labor supply and health behavior response of middle-aged individuals (Hairault et al. (2010); De Grip et al. (2013); Bertoni et al. (2018); Carta and De Philippis (2019)). However, very few studies investigate the impact of raising ERA on private savings, especially on middle-aged households' savings responses. Based on the empirical evidence of strong employment responses to an increase in the ERA, we expect the impact on private savings to differ from the impacts of pension reforms of other formats, such as changing the pension benefit formula and replacement rate. The richly detailed microdata source with comprehensive household savings and expenditure information also allows us to go beyond labor supply changes and focus on savings and consumption expenditure responses.

Second, our paper belongs to the literature on the substitution between public pension wealth and private savings using quasi-experiments. The standard life cycle model predicts whether the public pension benefits crowd out private savings depending on how much labor earnings increase. In theory, workers can postpone their labor market exit, and the additional future labor earnings can fully compensate for the loss in pension wealth. Existing studies commonly find that households increase their private savings rates when facing a reduction in the public pension replacement rate (Attanasio and Brugiavini (2003); Attanasio and Rohwedder (2003); Feng et al. (2011); Delavande and Rohwedder (2017)). A common feature of the exogenous variations explored in these studies is that they do not explicitly change the statutory retirement age and typically have a smaller impact on retirement age. For example, Lachowska and Myck (2018) study a reduction in pension wealth induced by a pension reform in Poland, which had a small effect on retirement age. They find a sizeable degree of substitution between pension wealth and savings. In contrast, our paper explores a setting in which the expected future labor earnings increase significantly due to the rise in ERA. We show that the treated middle-aged households reduce their savings rates in anticipation of a longer working horizon. This implies that they expect to have a higher level of overall lifetime wealth and so smooth their consumption by spending more and saving less.

Our paper is the closest to Lindeboom and Montizaan (2018) who analyze a Dutch pension reform, which changed many aspects of the pension system in the Netherlands. Importantly, political debates at the time emphasized a prolonged working life as a consequence of the reform.

They estimate the reform effects on households' retirement expectations and private savings and find that individuals mainly compensate for the reduction in pension wealth by prolonging employment. Private savings increase moderately. Their finding is consistent with ours and suggests that when the increase in the working horizon is salient, workers tend to cope with the loss in public pension wealth by working longer instead of saving more.[2]

Last, our paper relates to studies on the consumption response to anticipated permanent income changes (Hsieh (2003); also see Attanasio and Weber (2010) and Jappelli and Pistaferri (2010) for reviews). The permanent income hypothesis (PIH) predicts that consumers should not respond to predictable changes in their income because they use their savings to smooth income fluctuations. Our paper shows evidence of adjustments of savings and expenditure due to an anticipated permanent change in expected lifetime earnings. Consistent with the PIH, we find that treated households dissave and spend more in anticipation of an increase in future labor earnings. Our findings provide empirical evidence that households are forward-looking and can adjust their consumption when facing a change in their expected lifetime income.

The rest of the paper is organized as follows. Section 2 describes in detail the abolishment of the women's pension pathway and the German pension system. In Section 3 we describe a simple economic model of consumption and retirement timing to come to predictions. Data and the empirical setup are discussed in Sections 4 and 5. Section 6 describes the results, while Section 7 discusses the findings and concludes.

2. Institutional Background

Key Features of the Public Pension System in Germany The German Public Pension System is an earnings-related point system financed on a pay-as-you-go basis. Participation is mandatory, except for civil servants and the self-employed. On average, the public pension replaces around 50% of pre-retirement wage, net of income and payroll tax. The pension benefit levels are closely tied to the lifetime wage incomes. Aside from a few exceptions, workers with more contribution years or higher relative wage incomes will receive higher pension benefits.

[2] Note that Lindeboom and Montizaan (2018) finds that the highly educated can buffer the rise in early retirement age via a tax-advantaged saving scheme. However, the savings options are different in the Dutch reform. In the same year, when the Netherlands made early retirement less attractive for the cohorts born since 1950, the government also introduced a tax-facilitated saving option (the life-course savings program). This allows individuals to save tax-free up to 210 percent of their last wages earned, which equates to around two years of full income or two years with 70% of previous income. Moreover, the life-course savings program provides a slight advantage for those born since 1950 to save at a more rapid pace. Unfortunately, similar types of tax-favorable savings options were not introduced when the old-age pension for women was abolished in Germany in 1999. Therefore, highly educated Germans may not be able to finance their early retirement by saving more quickly.

The statutory retirement age for a regular old-age pension remained at 65 years of age throughout our sample period.[3] The only prerequisite of claiming a regular old-age pension is to have contributed for at least five years. Several alternate pathways make retiring before 65 years of age possible. Each pathway also has its own full retirement age (FRA), and an early retirement age (ERA). For example, age 60 is the early retirement age for the women's pension pathway; age 63 is the early retirement age for the long-term insured pathway.[4] However, retirement before the FRA renders a 3.6% benefit deduction for each year of early claiming (see Engels et al. (2017) for more details). Deductions of 3.6% are low by international standards (Queisser and Whitehouse 2006) and not actuarially fair (Börsch-Supan et al. 2004). As a consequence, many individuals prefer to retire as early as possible.

Notably, prior to the 1999 pension reform, eligible women could claim their pension early at age 60 via the pathway of the old-age pension for women. The eligibility requirements for this pathway were: first, at least 15-years of pension insurance contributions; and second, at least 10 of the 15 years of pension insurance contributions need to be acquired after age 40.[5] According to Geyer and Welteke (2019), 60% of women born in 1951 were eligible for the women's pension.

The old-age pension for women is an important pathway for women born until 1951 to retire. Among women born between 1948 and 1951, 35.38% retire via this pathway. Women who have retired through the old-age pension for women are more likely to be married, have around 13 years of education and are equally likely to be West or East German. They started working at age 18.50 and 60% of them have more than two children.[6]

Abolishment of the Old-age Pension for Women The 1999 reform eliminates the possibility of claiming a pension at age 60 for women born after 1951. This reform was announced in December 1997 and became effective in January 1999.[7] Prior to the reform, women born before 1952 had the option to claim the pension at age 60 via the women's pension, while women born in and

[3] Starting from 2012, the statutory retirement age for cohorts born after 1947 began increasing from 65, and this will reach age 67 for cohorts born after 1964.

[4] The four alternative pathways to retirement are old-age pensions for women, old-age pensions due to unemployment (and part-time work), old-age pensions for the long-term insured and old-age pensions for severely disabled persons. See (Börsch-Supan et al. 2004) and Appendix C.2 for more details.

[5] Contribution periods of employment periods, unemployment duration and up to three years of child-rearing periods and certain periods of education.

[6] Table A.3 uses information from SHARE-RV and the scientific use file of the Insurance Account Sample (VSKT) 2014 wave (administrative data from the German Pension insurance) to obtain the characteristics of women born between 1948-1951 who claimed old-age pension for women. VSKT2014 contains a random sample of individuals with an active public pension insurance account in Germany in 2014.

[7] Reform details can be found in the relevant law, *Rentenreformgesetz 1999* (RRG 1999), which was announced on December 16, 1997. In 1998, during the federal elections, the Green Party and the Social Democrats promised to change the already announced RRG 1999. However, although they won the election and modified many aspects of the pension scheme in 1999, they did not reverse the abolishment of the women's pension pathway. Therefore, the abolishment became effective in 1999. Even though the exact rules were announced in December 1997, there was political uncertainty about the actual implementation of the reform in 1998. See Appendix C.1 for a more in-depth discussion.

after 1952 no longer have this option after the reform. The only other possible way to leave the labor force before or at age 60 is via disability insurance due to severe health conditions.[8] Otherwise, the earliest possible age to claim a pension is at age 63, via the long-term insured pension pathway. The pension for the long-term insured is available for those with more than 35 years of contribution, including child-raising periods. Around 90% of women eligible for the women's pension also qualify for this pathway (Geyer and Welteke (2019)). Workers who are not eligible for the long-term insured pension can claim the regular old-age pension.[9] For example, women born in 1951 can claim the pension at age 60 via the women's pension with an 18% penalty for early claiming. For women born in 1952, unless they qualify for disability pension, the earliest possible retirement age is 63 with a 9% penalty via the pension for the long-term insured. Alternatively, they can retire at the regular retirement age without financial penalties, which is 65 years and five months.

Figure 1 plots the earliest possible retirement age for women as a function of the birth cohort. Women eligible for the women's pension face a sharp increase in their distance to retirement. The ERA effectively increases from age 60 to age 63 for the impacted cohorts. Indeed, Geyer and Welteke (2019) find that the reform increases the employment rates of 60-62-year-old women by 13.5 percentage points, which amounts to about a 30 percent increase compared to the pre-reform mean. It also shows that the increase in employment rate is mainly due to women remaining longer in their current jobs.[10] In this paper, we explore this sharp shift of the ERA between cohorts to estimate the causal impact on private household savings before retirement.

The reform was enacted in 1999, and the first cohort affected by the reform was cohort 1952, who turned age 60 in 2012. Affected individuals became aware of the changes in future pension wealth and future labor earnings a decade before the implementation of the income changes. Thus, they had considerable time to react to the forecastable income changes. Moreover, the reform was transparent and easy to understand. In this paper, we test changes in households' savings and spending in 2003 and 2008, four years and nine years after the reform's announcement. We expect to see the treated households incorporate the anticipated income changes into their savings and consumption decisions before retirement.

[8] Workers who have lost their earnings capacity can claim disability insurance, which is independent of age. The disability insurance is available for workers with at least five years of contribution, with at least three out the five years contributed before claiming. Workers who are officially recognized as having a low earnings capacity (which entails permanently not working more than three hours per day in any job) can claim disability insurance. Therefore, workers can leave the labor force via disability insurance.

[9] See Appendix C.2 for more details on different retirement pathways.

[10] Geyer and Welteke (2019) also looks at the reform impacts on the unemployment rate and disability pension participation rates. They find a small increase in the fraction of women who are unemployed, but program substation into the disability pension program. They find that about half of those women, who would have retired if they had the option, continue to work due to the reform.

3. Theoretical Predictions

In this section, we first use a standard dynamic consumption model with an endogenous retirement decision to illustrate the impact of the reform on optimal retirement. Then, we provide benchmark predictions for the savings responses, which depend on the changes in expected lifetime wealth, consisting of expected pension wealth and expected future labor earnings.

3.1. Model Prediction: Retirement Responses

A standard dynamic model of consumption with an endogenous retirement decision and no uncertainty as in Laitner and Silverman (2007) and Hurd et al. (2012) can assist us to understand how the increase in ERA affects retirement decisions, therefore private savings. Assume an individual starts with zero assets ($a_0 = 0$) and has a discount rate ρ, which equals the real interest rate r. The individual at age t decides how much to consume (c_t) and when to retire (R) if she has not done so already, by solving the following optimization problem:

$$\begin{aligned} \max_{c_t,R} U(c_t, R) = \quad & \max_{c_t,R} \int_0^R e^{-\rho t} u(c_t)\, dt + \Psi(a_R + B(R), R) \\ s.t. \quad & \dot{a} = r a_t + y_t - c_t, a_0 = 0 \end{aligned} \tag{1}$$

The post-retirement indirect utility Ψ is given by solving the following problem

$$\begin{aligned} \Psi\big(a_R + B(R), R\big) = \quad & \max_{c_t} \int_R^T e^{-\rho t}\big(u(c_t) + \Gamma\big) \\ s.t. \quad & \dot{a} = r a_t - c_t, a_T \geq 0 \end{aligned} \tag{2}$$

where the flow utility prior to retirement is $u(c_t) = c_t^{1-\theta} \Big/ (1-\theta)$ and after retirement is $u(c_t) + \Gamma$. Γ presents the utility of leisure in retirement. The inter-temporal elasticity of substitution $(1/\theta)$ is the coefficient of relative risk aversion. This formation of the utility function assumes that consumption and leisure are strongly separable. We assume an individual earns a constant (after tax) wage y per year and at retirement receives annual pension payments $b_t(R)$. The present value of the pension benefits $B(R)$ at retirement age R is given by $B(R) = \int_R^T e^{-rt} b_t(R) dt$, where r is the real interest rate.

The first-order conditions to Equations 1 and 2 give us the following expression:

$$\frac{\dot{c}_t}{c_t} = \frac{r - \rho}{\theta} = 0 \tag{3}$$

Because we assume that the utility discount rate equals the interest rate, the individual smooths the marginal utility of consumption across periods to maximize utility over the life cycle. Therefore, $c_t^* = C/T$, where C is lifetime consumption.

The first-order condition for retirement age R takes the form

$$\big(y + B'(R)\big) \times u'(c_R) = \big(y + B'(R)\big) \times c_R^{-\theta} = \Gamma \tag{4}$$

where $y + B'(R)$ is the marginal benefit from delaying retirement by one period and Γ is the marginal cost of leisure forgone by retiring later. Figure 2 illustrates the stylized pension wealth $B(R)$ for individuals who face an ERA of 60 years of age (black dashed line) and an ERA of 63 years of age (blue solid line).[11] The non-linear relationship between pension wealth and retirement age indicates strong financial incentives to retire at the ERA. Therefore, a change in ERA induces large labor supply responses. Indeed, Geyer and Welteke (2019) and Geyer et al. (2020) show that the reform leads to increased labor supply of women in the ages 60-62 as they are affected by the reform.

3.2. Expected Savings Responses

Given the large labor supply responses, we can provide benchmark predictions for savings. Before the realization of retirement, individuals adjust their savings according to changes in expected lifetime wealth, which depends on changes in expected pension wealth and expected future labor earnings. The effect of raising the ERA on the savings rates is ambiguous and depends on the corresponding expected employment effect.

Figure 3 illustrates four scenarios of the changes in discounted lifetime income for a stylized individual who would retire and claim pension at age 60 in absence of the reform.[12] If the reform induces her to work until after age 61, her lifetime income increases. This is because delaying retirement incurs additional future labor earnings and delaying claiming the pension also increases monthly pension benefits via a smaller financial penalty and more contributions. Further, she has a shorter retirement duration. These factors may offset the forgone pension benefit due to later claiming and increase the lifetime earnings. Higher lifetime income indicates a higher per period consumption, as the extra income will be spread evenly over a lifetime, leading to less savings during the periods before retirement. However, if individuals do not expect to prolong their working lives (as per the scenario, they retire at age 60), they may increase their private savings to cushion the loss in lifetime income. The heterogeneity in expected employment responses can result in different savings consequences.[13] The differences in expected employment responses may occur due to fulfilment of eligibility criteria, knowledge of the institutional setting and other

[11] See Appendix D.2 for detailed steps to obtain the illustrated pension wealth.

[12] Figure A.2 illustrates different scenarios for a stylized individual who expect to retire and claim pension at age 60, 61, 62 and 63 in the absence of reform.

[13] In Section 6.4.1, we show that the affected cohorts' expected retirement age increases by around 1 year. Based on the illustration in Figure 3, we expect savings rates to decline due to the reform.

factors impacting retirement timing. In Appendix D.1, we build a simple three-period life cycle framework following Feldstein (1974, 1976) to illustrate the predictions on the optimal savings rates for two extreme cases: first, workers who do not prolong their lifetime working periods; and second, workers who prolong their working period for the full three years. In summary, the effect of an increase in the ERA on the savings rates is an empirical question. Whether the increase in expected future labor earnings is large enough to reduce the savings rates will be tested in this paper.

4. Data

We primarily use the German Income and Consumption Survey (*Erwerbs- und Verbrauchsstichprobe*, EVS) to analyze savings and consumption responses of the reform. In addition, to better understand the savings rate responses, we also employ the German part of the Survey of Health Ageing and Retirement in Europe (SHARE) to analyze changes in the expected retirement age.

4.1. Main Data and Sample

The main sample is from the German Income and Consumption Survey (*Erwerbs- und Verbrauchsstichprobe*, EVS).[14] The EVS is a representative repeated cross-sectional survey of 0.3% of all households in Germany, carried out every five years by the German Federal Statistical Office. The baseline sample consists of four waves of EVS: 1993, 1998, 2003, and 2008.[15] We keep households with female members born from 1948 to 1955: four years before and in 1951, and four years after 1951. We focus on households with female members younger than age 60 to ensure that pension wealth changes are not materialized, because claiming an old-age pension before age 60 is almost impossible.[16] In summary, we look at women aged 38-50 and born between 1948 and 1955 in the waves 1993 and 1998, and we look at women aged 48-60 and born between 1948 and 1955 in waves 2003 and 2008. We vary the birth cohort restrictions in the robustness analysis.

The EVS contains detailed information of household income, consumption expenditure and savings, that has been computed from diaries filled out by the households over the course of at least three months. Therefore, consumption and savings measures are precise and detailed. The

[14] For a short overview of the data set, see Statistische Ämter des Bundes und der Länder (2018).

[15] See Bundesamt (2005a,b, 2012) for the detailed data descriptions. Appendix B.1 also describes the representativeness, survey method, key variables, attrition and survey weights of the EVS in more details. There are two limitations of the EVS: first, limited representativeness at the very top end of the distribution; and second, underestimated income from self-employment or capital income. See Appendix B.1 for further discussion. Overall, we do not expect our estimates to be sensitive to these two constraints.

[16] We do not use wave 2013 because the cohorts born around 1951 are older than 60 in 2013. Thus, we do not observe anyone in the control group (women born before 1952) in 2013. Table A.1 shows the number of observations by birth cohorts and by age for women in the 1993, 1998, 2003, and 2008 waves.

EVS has three features that make it well-suited for our analysis: first, it is the only available richly detailed microdata source for households' savings and consumption information in Germany. The advantage lies in its reliance on a consumption diary kept for three months in contrast to retrospective survey questions as posed in household surveys (such as the SOEP or SHARE).This continuous measurement over a relatively long period results in higher data accuracy (Dustmann et al. (2018)).[17] In fact, the consumer price index for Germany is compiled in accordance with the consumption patterns in the EVS. Besides investigating the overall savings and consumption responses, we can also examine the changes in the subcategories of savings and consumption expenditure. Second, the sample size is large. Each wave contains individuals from around 60,000 households and is the largest data source of its kind in Europe. Third, the EVS has the socio-demographic characteristics of all household members. This feature allows us to examine the heterogeneous impacts by marital status and control for partners' characteristics.

4.2. Summary Statistics

The final sample comprises 14,987 households in the control waves (1993 and 1998; 6,774 born before 1952 and 8,213 born thereafter) and 12,765 households in the reform waves (2003 and 2008; 5,921 born before 1952 and 6,844 born thereafter).

Table 1 shows the summary statistics of sample characteristics and the main outcome variables for households with women born before and after 1952 in the reform waves (columns 1 and 2) and control waves (columns 3 and 4). Savings, income and consumption expenditure is measured at the household level. We use equivalized individual values, which are adjusted for household size. We divide household-level values by the number of equivalent adults and assign the outcome equally to all household members.[18] All monetary variables are adjusted to 2003 euro values. Table 1 shows that households in the control waves have higher equivalized net-income and disposable income, and their savings rates are slightly higher. This difference stems from the fact that we observe the sample when households are younger in the control waves. Besides, the 1993 wave has a slightly different way of categorizing savings and expenditure. We, therefore, control for wave fixed effect in our regression analysis.

The main outcome variable is the households' savings rates, which is defined as monthly household net savings divided by the monthly net disposable income. In our sample, households

[17] Dustmann et al. (2018) highlights that EVS differs from other household surveys (e.g.,the SOEP (Socio-Economic Panel Study)) in its reliance on a consumption diary kept for at least three months rather than on retrospective survey questions. Moreover, the EVS records a diary kept for three months, which is much longer than the diary in other consumption surveys, such as the Consumer Expenditure (CE) Survey in the US and the Living Costs and Food Survey (LCF) in the UK.

[18] We use the OECD equivalence scale, which assigns a weight of 1 for the first adult in the household, 0.5 for each additional household member aged 14 and above, and 0.3 for each additional household member under 14. The same scale is used, for example, in Biewen and Juhasz (2012) and Dustmann et al. (2018).

save on average 433 euros per month in the control waves (a savings rate of 13%), and 239 euros per month in the reform waves (a savings rate of 11%). We also look at three categories of savings rates by types of savings vehicle. These are the monetary savings rates (deposits to bank accounts, buying stocks), the property savings rates (buying gold, houses, etc.) and the loan payback rate (mortgage and interest payments or the redemption of credits, etc.). We find the savings rates for monetary values of 6%, a 3% savings rate for property values and a 2% savings rate for loan payback in the reform waves.[19]

We further check several subcategories of household consumption: basic consumption, leisure consumption, and the probability of owning a private pension insurance. We define basic consumption as the expenditure on clothes, food at home, education, rent, public transportation, etc. Leisure consumption includes expenditure on leisure activities, such as attending concerts, taking up hobbies, buying sports equipment, and holiday accommodation costs. In our sample, households spend on average 1,600 euros per month in the reform waves, and 2,000 euros per month in the control waves.

4.3. Data on Expectations

To show some suggestive evidence on the impact of the abolishment of the women's pension pathway on the expected retirement age, we utilize an auxiliary sample: the Survey of Health, Ageing and Retirement in Europe (SHARE). SHARE collects data on a representative sample of individuals aged 50 and over. We take the following waves: wave 1 (interview years 2004 2005), wave 2 (2006 and 2007), wave 4 (2011 and 2012), wave 5 (2013) and wave 6 (2015).[20] We construct a sample with all women younger than 60 and born between 1947 and 1956 (five years before and after the cut-off). The outcome variable of interest is the expected retirement age, which is asked directly in the survey. The survey question is: *"At what age do you yourself expect to start collecting this pension payment for the first time?"* This question is asked in all waves. See Appendix B.2 for more details about the SHARE sample.

5. Empirical Strategy

First, we explore the discontinuous jump in the ERA and use a regression discontinuity design to estimate the causal effect of the increase in ERA on monthly savings rates and consumption

[19] An observational period of three months is susceptible to producing extreme outliers due to durable good purchases and sales. Therefore, we trim the savings (total savings and savings rates) and drop the bottom and top 1%.

[20] See SHARE website and Börsch-Supan (2017), Malter and Börsch-Supan (2017) for further information on SHARE. We do not use wave 3 because it is a retrospective survey and has a different structure from the other waves.

expenditure. Because only women eligible for the women's pension are affected by the reform, the RD estimate captures an Intention-to-Treat (ITT) effect. Second, we augment our RD model with a difference-in-difference (DD) setup. We use the discontinuity by birth cohort to capture the reform effect and use the non-reform years to reveal any mechanical correlation between savings rate and birth year.

5.1. Regression Discontinuity Design

The estimation equation for RD design is the following:

$$Y_i = \alpha + \beta X_i + \gamma D_i + \delta_l f_l(S_i - c) + \delta_r D_i * f_r(S_i - c) + \epsilon_i \tag{5}$$

The running variable S_i is defined as the birth cohort. The reform cutoff c is set to 1951. The birth year is centered around 1951. The treatment indicator D is defined as $D = \mathbb{1}(S > c)$. f_l and f_r are unknown functions with the parameters δ_l and δ_r capturing diverging cohort trends in the outcome variables by treatment status. γ estimates the discontinuity in savings rates for cohorts born before and in 1995 and after 1951. X contains the demographic characteristics, including age, partner's age, being born in Germany, marital status (married, widowed, and divorced), number of household members, homeownership, education level, and living in East Germany. We include the year fixed effect and allow a differential cohort trend to the left and right of the cutoff to remove the age effect.[21] In further robustness analysis, we include a quadratic age trend and a quadratic cohort trend. For the baseline analysis, we use a bandwidth of four years and a linear specification.

One complication with the RD setup in our context is that we only know the birth information at the yearly level. Therefore, we have to compare individuals born a few years apart. We may capture some functional form correlation between birth cohort and the outcomes. To address this issue, we augment our RD design with a difference-in-differences model using non-reform years to reveal and control for any potential mechanical correlation between birth year and savings rates. This approach is valid under a common trend assumption whereby the underlying savings rate trends are comparable between reform and non-reform years in the absence of the reform. Specifically, we extend (1), using waves 1993, 1998, 2003 and 2008, by additionally specifying a "reform year" indicator $Post_{it} = 0, 1$, equal to one for waves after 1999 and zero otherwise, interacted with Equation (1):

[21] Some of the covariates are time-invariant and therefore redundant after the inclusion of year fixed effects.

$$Y_{it} = \alpha + \sum_{\tau=0}^{1} \mathbb{1}[Post_{it} = \tau] \times \{\gamma_\tau D_i + \delta_{l\tau} f_l(S_i - c) + \delta_{r\tau} * D_i f_r(S_i - c) + \theta Post_{it}\} + \tau_t + \beta X_{it} + \epsilon_{it} \tag{6}$$

γ_1 estimates the discontinuity in savings rates for cohorts born before and in 1951 and just after 1951 conditional on any secular cohort trends in the outcome variables. Equation (2) fully interacts Equation (1), with separate effects for reform and non-reform waves. τ_t is a wave fixed effect. Our preferred specification is the RD-DD specification with year fixed effect and a list of controls.

5.2. RD Assumptions

Smoothness in density: For a RD design to be valid, individuals must not manipulate the assignment variable, which in our case, is the birth year. This assumption is by construction true.[22] Nevertheless, we still check for the balancing density and predetermined variables in our sample. Figure A.1 shows the number of households per birth year of the female in the reform waves. We see no apparent discontinuity at the cut-off. There is a discrete increase for women born since 1949 because women born before 1948 are older than 60 in 2003 and are therefore not in the baseline sample.

Smoothness in covariates: Table A.2 reports estimated changes (from Equation (1), reform waves) for a set of covariates (age, age gap with the partner, homeownership, East German, household size, German citizens, the share of married women, the share of widowed or divorced women and the share with higher education) at the cut-off under different specifications: with a linear cohort trend (column (1), with a linear age trend (column (2), with a quadratic age trend (column (3)) and a quadratic cohort trend (column (4)). We find significant zero differences between the treated and un-treated for age in all specifications. Otherwise, the inclusion of different trends does not impact the estimators to any great degree. Pre-determined variables seem to be smooth around the cut-off in the sample.

6. Results

In this section, we first present graphical evidence and estimation results for savings responses to the reform. We further show heterogeneous effects and robustness tests. Moreover, because

[22] Geyer and Welteke (2019) provide detailed evidence that the RD identifying assumptions are satisfied.

family types can have important influences on household labor supply and financial decisions,[23] we present all effects for the full sample and the subgroups of couple households and single women.[24] Moreover, we examine other reform responses to better understand the savings rate responses, including expected retirement age, disposable income, and consumption expenditure responses.

6.1. Savings Responses

Figure 4 presents some graphical evidence on the relationship between birth year and the residualized savings rates in reform waves. The residualized savings rates are the difference between the actual values and the predicted savings rates using estimated coefficients from estimating Equation 5 using control wave observations. This partials out some of the concavity in life-cycle savings rates. We show the patterns for the full sample, couples and singles. The solid lines are the linear fitted lines, and the shaded areas indicate the 95 percent confidence interval. Overall, we see a small drop at the cut-off for the full sample. For the couples, we find an upward trend before the cut-off, which breaks at the cut-off. For singles, we observe a jump at the cutoff instead. Figure A.4 shows the relationship between birth year and residualized savings level. We observe a drop at the cut-off for both full sample and the couple households, while no obvious changes at the cut-off for the singles. Because we measure the cohort at a yearly level, other covariates may reduce the precision in the graphical analysis; we thus move on to show the regression results.

The first two columns of Table 2 report our basic RD estimates of γ from estimating Equation 5 in the reform waves (column (1)) and the control waves (column (2)). Column 3 reports the point estimate of γ_1 in the preferred RD-DD model from Equation 6 including both reform and non-reform years. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level.

The point estimate from column 1 suggests that the treated cohorts reduce their household savings rates by 1 percentage point in the reform waves, corresponding to a reduction of around 9 percent. We notice a mechanical effect of being born after 1951 in the non-reform years when no policy variations occurred at the cohort cut-off. Under the assumption that the underlying relationship between birth cohort and savings rates are comparable between reform and non-reform years in the absence of the reform, we take the impact of non-reform years into account in column 3. The effect is reduced to a more moderate 0.6 percentage points reduction in the

[23] There is a large literature study the interaction of marital status and household savings behavior (e.g. ,Borella et al. (2018), Fehr et al. (2016), Mazzocco et al. (2014), and Nelson (1988) and De Nardi et al. (2021)).

[24] In principle, it is possible that the reform also impacts marital status. We are less concerned by this in our setting because we have shown that the probability of being married is not affected by the reform in Table A.2.

savings rates. The RD-DD point estimate is, however, not significantly different from zero for the full sample.

How does marital status affect savings rate responses to the reform? Theoretically, the impact is ambiguous. First, married women tend to match their retirement timing to that of their partner, who is generally two or three years older in our sample. This creates an additional incentive for them to extend their working life when facing an increase in ERA. The descriptive statistics using the SHARE data in Table 7 show that married women increase their expected retirement age more than single women.[25] Thus, we expect married households to save less due to a higher expected lifetime income. Second, married women may rely more on their partners' income and may therefore be able to afford not to change their retirement decisions. For example, Geyer et al. (2020) explore the realized employment responses by marital status. They find that married women tend to move into inactivity, while singles rely more on other social welfare programs, such as unemployment insurance,[26] hence, they do not expect to prolong their working life and experience a larger decline in their lifetime income. Thus, we expect married women to save more in response to the reform. Last, single women are less likely to be the compliers of the reform. Without additional income from a partner, single women are unlikely to use the early retirement option in the absence of the reform. For example, Table A.3 shows that women who claimed the old-age pension for women are less likely to be single. Thus, the increase in ERA has a smaller impact on single women. We find that the couples dissave while singles are not responsive to the reform. This result infers that couples expect to have more labor income while singles do not have the same expectation.

Panels 2 and 3 of Table 2 separate the sample into subgroups by marital status; that is, married households and single households. In line with graphical evidence, we find that the couples drive the drop in the savings rates. The treated married households reduce their savings rates by 1.5 percentage points in the reform waves, which corresponds to a reduction of around 13 percent. By comparison, the mechanical impact in the control years is zero. For single households, we find an insignificant positive effect due to the reform, which is a combination of a small positive impact with high standard errors in the reform years and a large negative significant impact in the control years.

[25] In section 6.4.1, we show suggestive evidence of the reform effects on expected retirement age using the SHARE sample.

[26] Using household level information from the German Census data, Geyer et al. (2020) find that employment rate between age 60 to 62 increases by 8.72 percentage points for women in couple households, while the employment rate increases by 7.45 percentage points for single women. Moreover, because married women are more likely to be inactive before the reform, Geyer et al. (2020) finds a larger impact on being inactive between age 60 to 62 for women in couple households.

To capture the potentially heterogeneous effects on savings rates by survey waves, we show the RD estimates for each sampling wave for three groups (full sample, couples, singles) in Figure 5. Both the RD estimates and the 95% confidence interval are displayed in the figure. We find that the magnitude of the negative impact grows over time and is the largest in 2008 for the full sample and couples. There are two potential explanations: first, as the retirement planning decision is more salient for older workers, treated households are therefore more responsive in 2008 when they are between 52 and 56 years of age; second, because the reform was announced in 1999, it may take longer than four years for households to internalize the incentives' changes. Therefore, we observe a more considerable impact in 2008, which is nine years after the reform announcement. We do not see any effects of the reform for single households, as suggested by Table 2. However, due to the smaller sample size, we cannot interpret the pattern.

We also investigate the reform effect on equivalized individual savings level in Table A.5.[27] We find that the treated households reduce their savings by 90 euros per month in the RD-DD. Again, the impact is driven by couples. Treated married women reduce their equivalized monthly individual savings level by 121 euros, while single women's savings are not responsive to the reform. We do not observe any statistically significant impacts in the non-reform years.

6.2. Heterogeneous Effects

Besides marital status, we further look at the heterogeneous responses for subgroups by education attainment and homeownership. Education matters for three reasons. First, households consisting of highly educated women are more likely to know about the pension system and thus the changing incentives. For example, both Bottazzi et al. (2006) and Hess (2017) show that education is an important indicator for knowledge of the pension reform. Households with knowledge about the pension system adjust their expectations of retirement age and wealth accumulation decisions. Second, highly educated women are likely to be more strongly attached to the labor force, working in an environment where extending the employment duration may be easier. Therefore, they would expect to have a higher level of future labor earnings. Last, differences in eligibility and claiming shares between the high and low education groups could also cause the heterogeneous outcomes. We explore SHARE-RV to investigate this possibility. We find that the share of women eligible and who claimed the women's pension are similar among the control cohorts (Table A.4). We also show that for the treated women, eligibility shares are similar for high and low education groups. Hence, we rule out this possibility.

[27] Table A.5 only uses 1998 as the control wave. Because the 1993 wave of EVS is very differently constructed, the measurements of savings level in the 1993 wave are not comparable with other waves. We can use the 1993 wave for our main analysis on savings rates because the ratio measurement takes away some of the inconsistent accounting.

Table 3 shows the estimation results. We find that households consisting of highly educated women reduce their savings rates by 2.4 percentage points, which drives the overall impact. This finding suggests the importance of both financial literacy and possibilities to extend the working life.

We also investigate the heterogeneous effects of homeownership. On the one hand, we expect that households with more assets can better buffer the reform shock. They can still afford to exit the labor market at age 60 and finance the gap between ages 60 and 63 from their housing assets. However, because the housing asset is relatively illiquid, we expect the buffer stock impact to be small. On the other hand, in the absence of the reform, we expect that women who are not homeowners may need to work longer to finance their retirement and may prefer to work beyond age 60 already. Therefore, they are not the compilers of the reform; that is, the reform would not affect their expected future labor earnings. Consequently, they will not update their savings plan. The overall effect is an empirical question.

Table 3 shows an insignificant impact on savings rates for the non-homeowners, while the homeowners, regardless of their marital status, reduce their savings rates in response to the reform. Single women who are homeowners also reduce their savings rate. This finding suggests that income security matters. Women with other income sources (such as their husband's income) and homeownership are more likely to adjust their savings and consumption behavior. As the sole earner in a household, single women, even if they face an increase in future expected lifetime income, may be more reluctant to spend more and save less in their 50s.

There is a large literature that documents the existence of joint-retirement (see, for example, Atalay et al. (2019), Coile (2015), Hurd (1990), and Stancanelli (2017)). Joint-retirement decisions could affect married couples' reactions to the rise in ERA. To understand how the need to retire together affects savings, we explore the differences across two characteristics: age differences within couples and relative earnings. First, we study whether the age gap within couples matters. Geyer et al. (2020) show that the increase in the ERA for women has a negative effect on the retirement of their partners.[28] This suggests that the older spouses tend to work longer to wait for their younger wives to reach the ERA, so that they can retire together. Therefore, we expect the changes in expected future household labor earnings are exacerbated for married households with older partners due to the spillover effects. Hence, we expect households with older spouses to reduce their savings rates more.

Second, we study the role of relative earnings within a couple. Relative earnings share indicates who the primary earner is, and thus has more influence in household savings decisions. A

[28] This type of spousal spillover effects are also documented in several papers (Atalay et al. (2019), Banks et al. (2007), Coile (2015), Hurd (1990), and Stancanelli (2017)).

growing literature analyses and documents the effect of household bargaining on intra-household decisions (Browning and Chiappori (1998) and Chiappori (1992)). Gustman and Steinmeier (2000), Browning et al. (2021) and García-Miralles and Leganza (2021) show that couples, where men are the primary earners, are more likely to retire jointly. Therefore, we expect the changes in expected future household labor earnings are exacerbated for couples where men are the primary earners due to joint retirement. As a result, households with men who are primary earners reduce their savings rates more.

Table 3 shows that married households with older male partners reduce their savings rates by 1.9 percentage points at the five percent significant level, while the families with younger male partners reduce their savings rates by only 0.2 percentage points, and the impact is insignificant. We also find similar patterns by looking at couples with the female income shares above and below the 50% mark. Married households where men are primary earners reduce their savings rates by 2 percentage points. The impact on married households, where females are primary earners, is 1 percentage points. These two exercises suggest the importance of considering joint-retirement in the savings responses for married couples.

6.3. Robustness Checks and Placebo Tests

Several exercises further establish the robustness of the estimates. In this section, we test the robustness of the estimation results by varying model specifications, including choice of controls, bandwidths, and polynomial orders, and by using an alternative empirical method. We also establish the causality of our estimates by performing a number of placebo tests using placebo samples, including samples of older cohorts with the same age composition, men born between 1948 and 1955, and by using placebo cutoffs.

6.3.1. Robustness: Alternative Bandwidths, Specifications and Sample Restrictions

Table A.6 shows how the RD-DD estimator (γ_1) changes for the full sample, couples and singles if we do not add any controls (columns 1), introduce year fixed effects (columns 2) and introduce the full number of control variables and year fixed effects (columns 3). The estimates are stable by varying the choices of controls. Table A.7 shows results by various bandwidths. The impacts are stable between samples with three and four years of bandwidth. However, when increasing the bandwidth to five years, the effect becomes insignificant. The results using a five-year bandwidth can be problematic due to an unbalanced sample around the cut-off. In the 2008 wave, we have only four years to the left of the cut-off because women born in 1947 are older than 60 and are therefore dropped from our sample.

We show the results with a quadratic age trend (Table A.8) and with a quadratic cohort trend (Table A.9). The estimates are not sensitive to quadratic age controls. However, introducing a quadratic cohort trend causes the estimates to be insignificant. We find close to zero and much smaller insignificant negative impact for couples with a quadratic cohort trend. We believe that given we have so few numbers of bins around the cut-offs, it may be a stretch to introduce quadratic cohort trends.

In the baseline sample, we only drop households with women older than age 60 to ensure that pension wealth changes are not materialized. To check if the estimates on couples are sensitive to this restriction, we perform robustness tests by using a sample of households with husbands who are not retired (Table A.10). The estimates are not sensitive to this restriction.

6.3.2. Robustness: Alternative Empirical Method

Because we only know the birth information at the yearly level, we compare individuals born a few years apart around the cutoff in the RD setup. Furthermore, the RD-DD specification washes out any potential mechanical correlation between birth year and the savings rates by using the relationship at younger ages in the pre-reform years. However, the savings profiles at younger ages might not be a good counterfactual for the savings profiles of the same cohorts at older ages. Moreover, the estimated discontinuous drop of savings rates may be driven by the life-cycle profile in savings rates, even after controlling for age, age squared and cohort profile. Therefore, we perform a robustness exercise by exploring an event study design. We compare the treated and control cohorts over the survey waves when they are of comparable ages. The regression equation follows the standard difference-in-differences (DID) setup:

$$Y_{it} = \theta_0 + \theta_1 D_i \times Post_{it} + \theta_2 D_i + \theta_3 Post_{it} + \beta X_{it} + \tau_t + \epsilon_{it} \tag{7}$$

We control for the same set of demographic characteristics and year fixed effect. Table A.11 shows the DID estimates by marital status and by bandwidth choices. Except for the sample using two years around the cutoff, the DID estimates show a similar pattern as the RD-DD results. Facing an increases in ERA, couple households do not increase their savings rates in their 50s. When we only take households with women born in 1951 and 1952, we find the treated married households reduce their savings rate by 1.4 percentage points after the reform. Table A.12 shows the corresponding event-study estimates. Figure A.3 displays the event-study plots using the baseline sample of cohorts from 1948 to 1955.

Even though not all post reform estimates are statistically significant, we see that the drop in savings rates widens in the 2003 and 2008 waves (except for the sample using two years around

the cutoff). However, as there are only two waves before and after the reform, we can never formally test for the parallel trend assumption. Nevertheless, the DID estimates suggest that the RD-DD estimator does not pick up the life-cycle profile in savings rates; rather, it captures the causal reform impacts.

6.3.3. Placebo Tests: Older Cohorts

To directly test if the observed discontinuous drop in savings rates in the reform year is driven by some structural break in savings rates at the cutoff age (age 51 in 2003, age 56 in 2008), we run a placebo test by using samples of older cohorts with the same age composition.

We compare the RD estimates obtained by the baseline sample (cohorts 1948-1955) in 2003 with the placebo estimates by using a pooled sample of older cohorts in 1993 (cohorts 1938-1945) and 1998 (cohorts 1943-1950).[29] The pooled placebo sample has the same age composition as the baseline sample and the same cut-off age at 51. Panel 1 of Table A.13 shows the effects using the pooled placebo sample for the full sample, couples, and singles. Panels 2 and 3 of Table A.13 display the RD estimates by using older cohorts in 1993 and by using older cohorts in 1998, respectively. We find no significant differences: all point estimates have magnitudes close to zero. Therefore, we can be confident that the estimated discontinuous decline in savings rates between cohorts 1951 and 1952 in 2003 is not driven by structural differences in savings rates along the age dimension.

We do the same analysis for the RD estimates in 2008. We compare the RD estimate obtained by the baseline sample (cohorts 1948-1955, aged from 53 to 60) in 2008 with the placebo estimate by using a pooled sample of older cohorts in 1993 (cohorts 1932-1940) and 1998 (cohorts 1938-1945). The pooled placebo sample has the same age composition as the baseline sample and the same cut-off age (younger than age 57). Table A.14 measures the discontinuous change in savings rates between ages 56 and 57 in the placebo sample. The RD estimate is not significant by using a placebo sample in 1993, however, the impacts are negative and significant when we use the placebo sample in 1998. One potential explanation is that women born earlier than 1941 face some financial penalties in claiming the old-age pension at age 60.[30] This can mean that the households with women younger than 57 in 1998 are less likely to leave the labor force, and

[29] We only perform the placebo tests for the RD estimates in reforms year and did not do so for the non-reform years because we would need to use earlier waves to obtain values of the outcome variables for placebo samples when they were younger. Unfortunately, earlier waves of EVS are very differently constructed and only contain information for West Germany.

[30] The 1992 pension reform in Germany introduced financial penalties for the early retirement for women born after 1939. Women born before January 1940 could retire without deduction from age 60 onwards, while for women born in subsequent months until December 1944, deductions were introduced at a monthly frequency. See Engels et al. (2017) for the labor supply impact of this reform.

hence have a higher disposable income. This could also be the reason for seeing lower savings rates in 1998 for the cohorts born after 1941.

Table A.13 and Table A.14 suggest that the RD estimate in the reform years is not driven by a structural break in the savings rates along the life-cycle profile.

6.3.4. Placebo Tests: Men

Furthermore, we take households with men born between 1948 and 1955 as a placebo group. We perform the RD-DD analysis using households with men born since 1952 as the instrument. Table A.15 shows that no significant changes in savings rates for the full sample, couples, and singles.[31] It is worth noting that there is a confounding cohort-based reform for men, which might also impact households with men born since 1952. For men born before 1952, the earliest age to claim a pension is at age 63 via either an old-age pension for the unemployed or an old-age pension for the long-term insured. For cohorts born since 1952, the old-age pension for the unemployed was abolished. Even though this change could potentially affect savings behavior, we do not find any significant changes at the cutoff. One explanation is that the earliest possible age to claim a pension remains at age 63 because the option to claim a pension via the long-term insured pathway at age 63 is still available. Their situation is very different from that of households with women born since 1952.

6.3.5. Placebo Tests: Placebo Cutoffs

In addition, we show the RD and the RD-DD estimates using cohort 1950, 1953, and 1954 as the placebo cut-offs in Table A.18. We find virtually no effects on the savings rate in the full sample at these placebo cut-offs. The absolute values of point estimates at the 1950 and 1954 cut-offs are almost always lower than our estimated effects at the 1951 cutoff, as expected. The estimates are small and insignificant, except that the RD-DD estimate for the couples at the 1954 cut-off is positive with a value of 0.006. Because the sign is the opposite of our baseline results, we are not concerned that the estimated reduction in savings rates in the baseline analysis is spurious. Yet, we do find a similar sizeable negative impact for couples and positive effects for singles at the 1953 cut-off, which might be because 1953 is too close to the actual cut-off. Combined with the fact that we only observe the birth dates at the yearly level, it is not too surprising to find similar impacts at the 1953 cut-off.

[31] Table A.16 and Table A.17 show the DID and event study results using households with men born between 1948 and 1955. We also find no significant changes.

6.4. Other Responses: Expectations and Expenditure

In this section, we examine the responses in three dimensions in order to better understand the savings rate responses. First, we show changes in the expected retirement age using the Survey of Health, Ageing and Retirement in Europe (SHARE). Second, we decompose the savings rate responses by investigating the response in disposable income and consumption expenditure. Last, as some specific subcategories of savings may drive the savings rate reduction, we show the impact for three outcomes: the monetary savings rates, the property savings rates, and the loan payment rates. Because the information on subcategories of consumption expenditure and savings in the 1993 wave is not comparable with other waves, we only show the RD effects using the reform waves in this section. We will focus only lightly on the magnitude of the estimates but more so on the signs.[32]

6.4.1. Expected Retirement Age Responses

We investigate the savings behavior of households consisting of women who have not yet retired; that is, women younger than 60 years of age. Consequently, the reform's effects on savings behavior run through the channel of changes in expectations toward the individual retirement age, retirement benefits and future labor earnings. Pervious literature has shown the importance of expectations on decisions related to pension and retirement planning (Bissonnette and Van Soest (2015), Bottazzi et al. (2006), and Ciani et al. (2019).)

Using the SHARE sample, Figure 6 shows an overlaid histogram of expected retirement ages for cohorts born before and since 1952. We see a clear shift of the expected retirement age from 60 to later ages for the treated cohorts. Table 7 compares the expected retirement ages for women born around the cut-off. Columns 1 and 2 show the sample means of expected retirement ages for women born before and since 1952. Columns 3 and 4 show the estimated treatment effect from a simple first-difference OLS regression with and without controls (age, East Germany and education).[33]

We find that women born before 1952 show an expected retirement age of 62.39, while women born since 1952 expect to retire at age 63.42. The difference in the expected retirement age is significant. The results provide some suggestive evidence that the reform alters individuals' expected age of retirement We also find that married women increase their expectation of retirement age more than single women. This is consistent with our finding that married

[32] Tables A.5, A.19, A.20 and A.21 show the estimates for savings level, savings rates, household disposal income and consumption expenditure using the 1998 wave as the control wave. The findings convey a similar message to the RD estimates.

[33] We only control for age and East Germany when showing heterogeneity by education attainment. See Appendix B.2 for more details about the SHARE sample.

households reduce their savings rate. When we look at the changes in the expected retirement age by education level, we find that women with a high and low education update their expectation of retirement age at a similar magnitude.

6.4.2. Disposable Income and Consumption Expenditure Responses

We first present the RD effects from Equation 1 using the reform waves in this section. Table 4 depicts small insignificant effects on equivalized disposable income for the full sample and couples, while singles show a positive but insignificant impact. This finding is consistent with Geyer and Welteke (2019), who find that the employment rates before age 60 are unaffected by the reform. Therefore, the change in the savings rates is not due to a change in disposable income.

We then highlight the consumption expenditure responses in Table 5. Row 1 shows that, for couples, the monthly equivalized consumption expenditure of married households increases. Further, we investigate subcategories of consumption expenditure, including basic consumption, spending on leisure goods, and private insurance. We also show the impact on the probability of owning private insurance. We do not find any reform effects (small and insignificant), except for spending on leisure activities, which is again driven by couples' responses. These include expenditure on activities such as attending concerts, purchasing sports equipment and spending on hotel accommodation.

6.4.3. Subcategories of Savings Rates Responses

Furthermore, we investigate three subcategories of savings in Table 6. Because the information on subcategories of savings and expenditure in the 1993 wave is not comparable with other waves, we only show the RD effects using the reform waves for this analysis. We find that savings in monetary assets (such as deposits in checking accounts and buying stock shares) are the most responsive. Both couples and singles reduce their savings rates in monetary assets by around 1.8 to 2 percentage points. We find that married households also have lower property savings, which are savings in the form of tangible assets, such as gold and real estate assets. On the contrary, singles increase their property savings. This suggests that even though the single households do not change their overall savings rate, they adjust their portfolio composition by increasing their property savings. The estimated impact on paying back loans is insignificant. The responses in the savings subcategories show that changes in overall savings rates are mostly driven by the adjustment in monetary assets and property ownership.

Overall, while middle-aged households' disposable income is not affected by the reform, savings are reduced and spending is increased. We find more spending on leisure goods, while spending

on other types of life insurance remains unchanged. Reductions in monetary savings drive the decrease in the overall savings rates.

7. Conclusion and Discussion

This paper analyzes the effect of raising the early retirement age on households' savings rates. We use an RD-DD design to examine the 1999 pension reform in Germany, which increased the early retirement age for women born after 1951 by at least three years. We show the reform effects on households' savings rates and consumption expenditure. Using the German Income and Consumption Survey, we find a negative impact on private savings of 0.6 percentage points, which is driven by married households. There is considerable heterogeneity in these effects. We show that households consisting of highly educated women and home owners are more likely to reduce their savings rates. Furthermore, we find that the treated households increase their leisure spending while maintaining an unchanged level of disposable household income. Our findings show that the treated households absorb the pension wealth shock without increasing their savings.

Our findings are interesting for two reasons. First, we show that individuals and households are aware of the pension system changes long before they reach retirement age. These households adjust their savings and expenditure accordingly. We show that groups whose expected retirement age is more affected by the reform (that is, the couples) are more likely to reduce savings and increase their leisure consumption. Our finding suggests that policy makers should incorporate these anticipatory adjustments when evaluating pension reforms, in particular, the role of consumption expenditure which is at the heart of welfare evaluation. As policy makers intend to strengthen private pension plans, raising the ERA incentivizes families to dissave when they are young.

Second, we show empirically that when the increase in the working horizon is salient and the increase in lifetime labor income outweighs the decrease in pension wealth, workers tend to cope with the loss in public pension wealth by working longer, rather than saving more. Our results suggest that women within in a couple expect a more substantial increase in their lifetime income, which leads them to reduce their savings. Our paper is one of the first studies to focus on the impact of raising the statutory retirement age on savings. Thus, more studies that examine the effect of increasing the statutory retirement age on household savings are called for.

One interesting extension of this paper will be to check the impact on realized lifetime income. Suppose that married women expect a higher lifetime income and accordingly save less during their 50s. Later, when they reach age 61 and 62, they may not be able to prolong their employment

due to unexpected constraints. They may regret over-consuming too soon. The possibility of misalignment in expected and realized retirement age may stem from overconfidence about their capacity to extend their working lives. For example, studies, such as Caliendo and Huang (2008) and Pagel (2017), have documented household overconfidence in their financial situations. However, this discussion is beyond the scope of this paper.

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8. Tables and Figures

Table 1: Summary statistics

	Reform waves (2003, 2008)		Control waves (1993, 1998)	
	Born		Born	
	since 1952	before 1952	since 1952	before 1952
Covariates				
Age	51.37	55.45	43.48	45.98
	(2.90)	(2.46)	(1.66)	(2.75)
Age Diff	3.24	3.28	3.07	3.21
	(4.23)	(4.31)	(4.06)	(4.18)
Birth year	1954.06	1949.75	1953.79	1949.59
	(1.41)	(1.01)	(1.50)	(1.11)
German	0.98	0.98	0.98	0.98
	(0.13)	(0.13)	(0.15)	(0.15)
East German	0.26	0.25	0.22	0.21
	(0.44)	(0.43)	(0.41)	(0.41)
Household size	2.49	2.15	3.39	3.13
	(1.07)	(0.82)	(1.24)	(1.24)
Income				
Household net income	3572.34	3287.61	5221.67	5271.49
	(2038.82)	(1917.78)	(2762.11)	(2768.77)
Household disposable income	3635.30	3343.68	5320.70	5338.39
	(2108.91)	(1971.71)	(2858.42)	(2817.35)
Consumption information				
Overall consumption	1520.96	1568.28	1955.60	2077.25
	(901.00)	(951.41)	(1061.03)	(1088.10)
Basic Goods	1316.77	1333.37	1562.92	1727.81
	(867.31)	(840.97)	(869.40)	(982.01)
Food, cloth and rent	747.56	775.31	966.06	1040.38
	(329.17)	(326.77)	(366.65)	(393.04)
Leisure activities	253.91	256.46	341.70	357.05
	(243.35)	(249.08)	(318.08)	(279.20)
Insurance consumption	143.02	145.57	193.59	199.45
	(210.00)	(166.66)	(191.33)	(195.11)
Probability of owning private insurance	0.92	0.91	0.93	0.95
	(0.27)	(0.29)	(0.25)	(0.21)
Savings information				
Overall savings	247.27	230.54	404.12	458.95
	(1169.81)	(948.33)	(1067.37)	(1014.77)
Savings Rate	0.11	0.11	0.13	0.13
	(0.15)	(0.16)	(0.16)	(0.17)
Property savings rate	0.03	0.03	0.06	0.06
	(0.51)	(0.39)	(0.58)	(0.46)
Monetary savings rate	0.06	0.06	0.05	0.06
	(0.26)	(0.25)	(0.22)	(0.35)
Paying back loans	0.02	0.02	0.02	0.01
	(0.49)	(0.34)	(0.55)	(0.35)
Observations	6844	5921	8213	6774

Notes: Table 1 reports Means and (standard deviations) of characteristics for households in reform years and control years, respectively. Note the values for consumption expenditure, disposable income and subcategories of savings rates in control waves are obtained using the 1998 wave only.

Table 2: Effects on household savings rates

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-0.010*	-0.005*	
	(0.005)	(0.002)	
Born after 1951=1 × post=1			-0.006
			(0.006)
Observations	11,239	13,604	24,843
R^2	0.019	0.017	0.022
Dependent Variable Mean	0.109	0.132	0.121
		Couples	
Born after 1951	-0.015**	-0.000	
	(0.006)	(0.001)	
Born after 1951=1 × post=1			-0.015**
			(0.005)
Observations	8,710	11,198	19,908
R^2	0.012	0.002	0.011
Dependent Variable Mean	0.117	0.142	0.131
		Singles	
Born after 1951	0.007	-0.025**	
	(0.015)	(0.010)	
Born after 1951=1 × post=1			0.033
			(0.025)
Observations	2,529	2,406	4,935
R^2	0.014	0.012	0.012
Dependent Variable Mean	0.080	0.086	0.083
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table2 reports the RD estimates in the reform waves (column 1) and the control waves (column 2), and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

Table 3: Heterogeneous effects: RD-DD

	Full sample	Couples	Singles
Low education	-0.003	-0.010	0.031
	(0.007)	(0.006)	(0.029)
Observations	13,891	11,259	2,632
Dependent Variable Mean	0.121	0.129	0.084
High education	-0.009	-0.024***	0.037
	(0.008)	(0.006)	(0.026)
Observations	10,952	8,649	2,303
Dependent Variable Mean	0.122	0.133	0.082
Not homeowner	0.002	-0.015	0.044
	(0.017)	(0.016)	(0.027)
Observations	12,175	8,785	3,390
Dependent Variable Mean	0.108	0.124	0.067
Homeowner	-0.017***	-0.016**	-0.030*
	(0.004)	(0.005)	(0.015)
Observations	12,668	11,123	1,545
Dependent Variable Mean	0.134	0.137	0.118
With younger partners		-0.002	
(age gap ≤ 0)		(0.011)	
Observations		4,574	
Dependent Variable Mean		0.135	
With older partners		-0.019**	
age gap > 0		(0.008)	
Observations		15,334	
Dependent Variable Mean		0.130	
Female primary earner		-0.010**	
		(0.004)	
Observations		4,777	
Dependent Variable Mean		0.133	
Male primary earner		-0.020*	
		(0.010)	
Observations		10,490	
Dependent Variable Mean		0.143	
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table 3 shows the heterogeneous responses for subgroups by education attainment and homeownership. It also shows the heterogeneous responses for couples by age gap between the couple and female income share. We show couples with larger age gaps and couples with females earning less are reducing the savings rates.

Table 4: Effects on monthly equivalized household disposable income

	Full sample	Couples	Singles
Born after 1951	6.937	-6.669	58.545
	(33.261)	(25.625)	(66.525)
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓
Observations	12,537	9,766	2,771
R^2	0.156	0.141	0.133
Dependent Variable Mean	2,115.388	2,235.853	1,698.372

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table 4 shows the estimated changes in the equivalized monthly disposable income using the RD method in the reform waves.

Table 5: Effects on monthly equivalized consumption expenditures

	Full sample	Couples	Singles
Total consumption expenditure	39.527	51.439*	2.437
	(26.076)	(24.234)	(64.312)
Dependent Variable Mean	1,556.203	1,615.878	1,349.625
Basic Goods	3.202	5.349	-2.960
	(6.463)	(6.597)	(19.682)
Dependent Variable Mean	432.375	446.760	382.581
Leisure Goods	29.921***	35.394***	11.317
	(7.190)	(7.844)	(8.114)
Dependent Variable Mean	259.077	275.599	201.883
Insurance consumption	6.560	4.301	12.743
	(5.408)	(5.182)	(8.607)
Dependent Variable Mean	143.227	155.676	100.134
Probability of owning a	0.000	-0.007	0.026
private insurance	(0.013)	(0.012)	(0.031)
Dependent Variable Mean	0.914	0.932	0.851
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓
Observations	12,537	9,766	2,771

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table 5 shows the estimated changes in the equivalized monthly consumption expenditure using the RD method in the reform waves.

Table 6: Effects on subcategories of savings rates

	Full sample	Couples	Singles
Monetary savings rate	-0.019***	-0.018***	-0.020*
	(0.002)	(0.005)	(0.010)
Dependent Variable Mean	0.057	0.062	0.040
Property savings rate	-0.006	-0.026**	0.063*
	(0.010)	(0.008)	(0.031)
Dependent Variable Mean	0.031	0.032	0.028
Loan payment rate	0.014	0.030	-0.036
	(0.016)	(0.016)	(0.040)
Dependent Variable Mean	0.021	0.024	0.012
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓
Observations	11,239	8,710	2,529

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table 6 shows the estimated changes in the subcategories of savings rates using the RD method in the reform waves.

Table 7: Expectations of retirement age in the SHARE data

	Mean of expected retirement age		Difference	
	born before 1952	born since 1952	without controls	with controls
	(1)	(2)	(3)	(4)
Full sample	62.39	63.42	1.03**	0.97*
	(6.39)	(6.48)	(0.37)	(0.38)
Observations	562	1,035	1,328	1,321
Married	62.07	63.66	1.59**	1.45**
	(7.61)	(3.93)	(0.46)	(0.47)
Observations	279	452	731	614
Non-married	62.73	63.22	0.49	0.41
	(4.83)	(8.02)	(0.57)	(0.59)
Observations	283	583	866	696
Low education	62.78	63.84	1.06***	0.63*
	(2.29)	(2.14)	(0.16)	(0.32)
Observations	257	489	746	746
High education	63.19	64.13	0.94***	0.51
	(2.03)	(1.70)	(0.18)	(0.36)
Observations	163	244	407	407

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table 7 shows the average expected retirement age for cohorts born before 1952 and cohorts born since 1952. Columns 1 and 2 show the sample means by treatment status. Columns 3 and 4 report the estimated treatment effect from a simple first-difference OLS regression without and with controls (age, education, East Germany) by treatment status.
Data Source: SHARE waves 1,2,4,5,6.

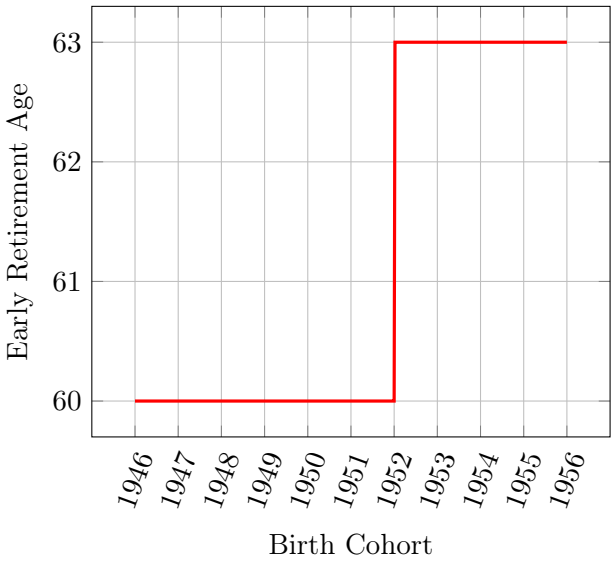


Figure 1: Early Retirement Age for the Old-age Pension for Women Pathway
Notes: Figure **??** plots the earliest possible retirement age for women as a function of their birth years.

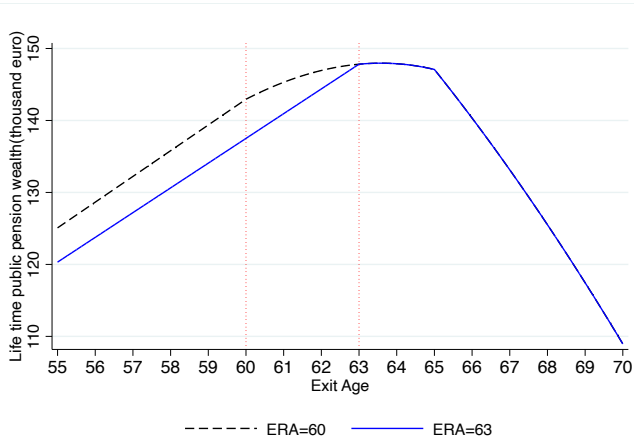


Figure 2: Illustration of the stylized pension wealth
Notes: Figure 2 illustrates the stylized pension wealth $B(R)$ for individuals face an ERA of 60 (black dashed line) and an ERA of 63 (blue solid line).

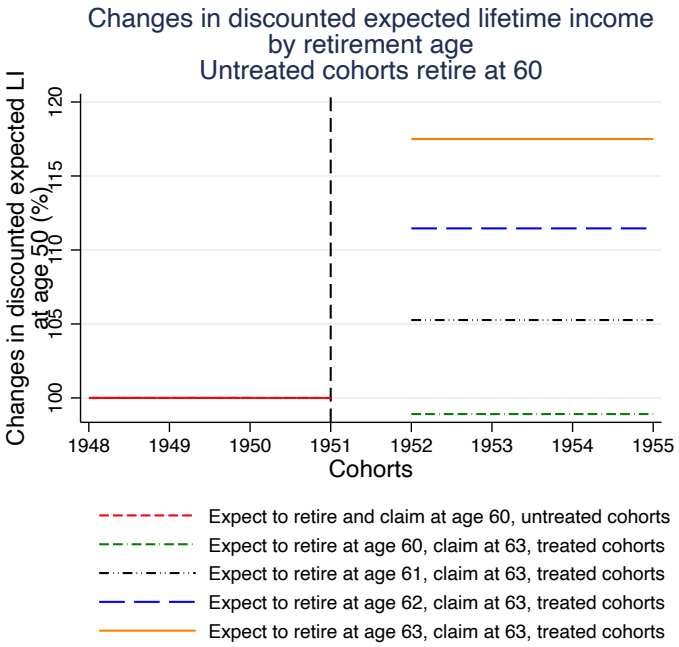
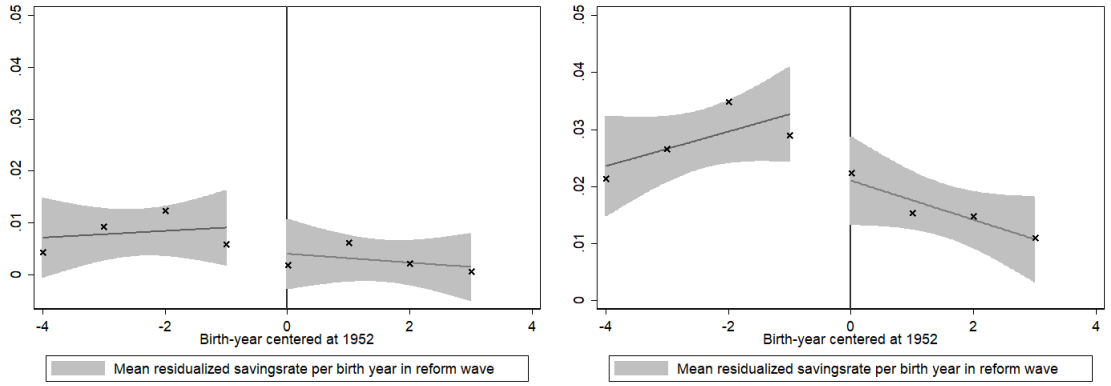


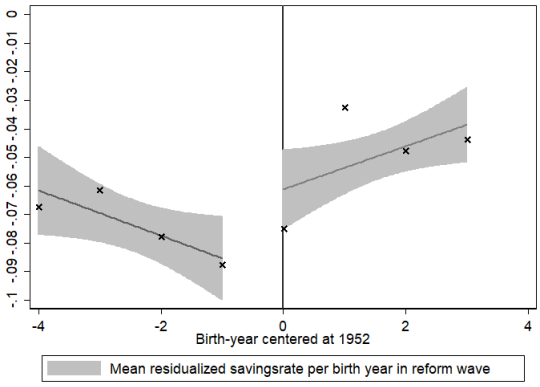
Figure 3: Illustration of changes in discounted lifetime income

Notes: Figure 3 shows four scenarios of the changes in discounted lifetime income for a stylized individual who retire and claim pension at age 60 in absence of the reform. The percentage changes in discounted lifetime income at age 50 are shown in four cases: 1) retire at age 60 and claim at 63 (green, dash dash dot line); 2) retire at age 61 and claim at 63 (black, dash dot dot line); 3) retire at 62 and claim at 63 (blue, dashed line); 4) retire at 63 and claim at 63 (orange solid line).



(a) Full sample

(b) Couples



(c) Singles

Figure 4: Residualized savings rates by marital status

Notes: Figure 4 presents graphic evidence on the relationship between birth year and the residualized savings rates in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform period are from an estimation model using 1993 and 1998 waves. The solid lines are the linear fitted lines. The shaded areas indicate 95 percent confidence interval.

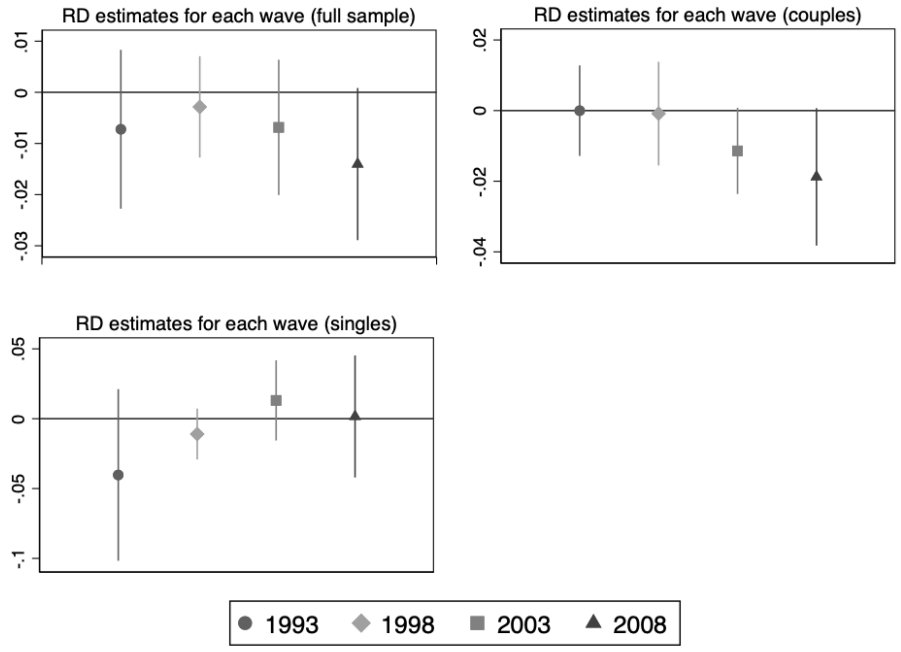


Figure 5: Wave-by-wave point estimates- savings rates

Notes: Figure 5 shows the RD estimates for each wave of EVS (1993, 1998, 2003 and 2013) for three groups (full sample, married and single households).

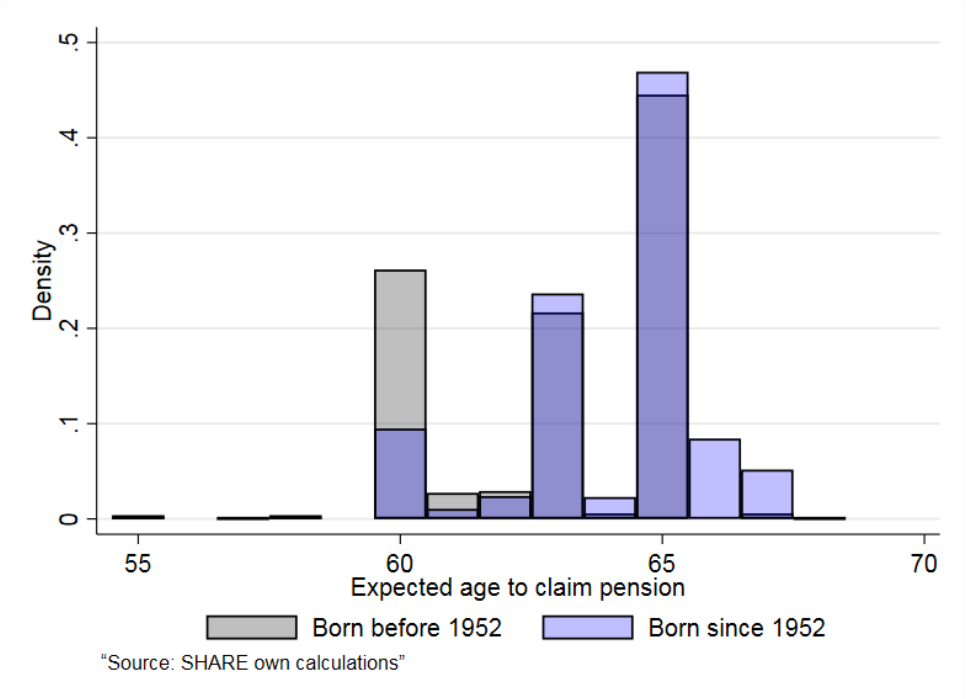


Figure 6: Expected retirement age by treatment status

Notes: Using the SHARE data, Figure 6 shows the distribution of expected retirement age for cohorts born before 1952 and cohorts born since 1952.

The Effect of Increasing the Early Retirement Age on Savings Behavior Before Retirement

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Online Appendix

A. Appendix Tables and Figures

Table A.1: Number of observations by cohort of the female and observation wave

Survey wave	1993	1998	2003	2008	Total
Birth year					N
1933	493	0	0	0	493
1934	683	0	0	0	683
1935	666	0	0	0	666
1936	730	0	0	0	730
1937	714	0	0	0	714
1938	728	826	0	0	1,554
1939	773	871	0	0	1,644
1940	833	903	0	0	1,736
1941	740	851	0	0	1,591
1942	576	689	0	0	1,265
1943	672	777	658	0	2,107
1944	646	753	708	0	2,107
1945	491	548	466	0	1,505
1946	585	627	570	0	1,782
1947	677	750	635	0	2,062
1948	696	791	673	697	2,857
1949	827	813	728	740	3,108
1950	846	945	762	799	3,352
1951	921	935	756	766	3,378
1952	944	995	808	820	3,567
1953	956	1,062	861	825	3,704
1954	993	1,137	884	861	3,875
1955	1,001	1,125	945	840	3,911
1956	1,001	1,219	969	879	4,068
1957	991	1,184	932	883	3,990
1958	1,001	1,259	991	930	4,181
1959	1,022	1,382	1,022	959	4,385
1960	985	1,356	1,096	975	4,412
Total	22,191	21,798	14,464	10,974	69,427

Notes: Table A.1 shows the number of observations for households with women younger than age 60 by survey wave and by cohort. In the baseline analysis, we keep households with female members born from 1948 to 1955.

Table A.2: Smoothness of the predetermined variables

	(1) Baseline	(2) Control for age	(3) Quadratic age control	(4) Quadratic cohort trend	N
Age female	-0.000***	-	0.000***	0.000***	12765
	(0.000)	-	(0.000)	(0.000)	
House ownership	0.020	0.020	0.018	0.025	12765
	(0.019)	(0.019)	(0.019)	(0.037)	
East	-0.025	-0.025	-0.023	-0.004	12765
	(0.016)	(0.016)	(0.016)	(0.033)	
Number of household	0.018	0.018	-0.003	-0.021	12765
members	(0.033)	(0.033)	(0.033)	(0.063)	
German	-0.005	-0.005	-0.004	-0.013	12537
	(0.005)	(0.005)	(0.005)	(0.010)	
Married	0.003	0.003	0.002	0.038	12765
	(0.016)	(0.016)	(0.017)	(0.033)	
High education	-0.020	-0.020	-0.020	-0.008	12765
	(0.018)	(0.018)	(0.018)	(0.037)	
Widowed	-0.008	-0.008	-0.008	-0.035**	12765
	(0.007)	(0.007)	(0.007)	(0.013)	
Divorced	0.012	0.012	0.012	0.027	12765
	(0.012)	(0.012)	(0.012)	(0.025)	
Age difference with	-0.265	-0.265	-0.247	-0.348	9714
the husband	(0.187)	(0.187)	(0.187)	(0.378)	

Notes: Standard errors in the parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.2 show smoothness for a set of predetermined variables at the cut-off under different specifications: with a cohort linear trend (column (1), with a age linear trend (column (2), with a quadratic age trend (column (3)) and a quadratic cohort trend (column (4)). Pre-determined variables seem to be smooth around the cut-off in the sample.

Table A.3: Characteristics of women claimed women's pension

Characteristics	mean	s.d.	Obs	Data source
High education	0.36	(0.48)	235	SHARE-RV
Years of education	13.0	(2.93)	235	SHARE-RV
Married	0.85	(0.35)	235	SHARE-RV
West German	0.55	(0.49)	3593	VSKT2014
Two and more children	0.59	(0.49)	3593	VSKT2014
Number of children	1.71	(1.04)	3593	VSKT2014
Age at first employment	18.50	(4.35)	3593	VSKT2014
Healthy (no sick spell before age 50)	0.45	(0.49)	3593	VSKT2014

Notes: Table A.3 shows the characteristics of women who claimed old-age pension fro women and born between 1948-1951 (control cohorts). In SHARE-RV data, we define women as retiring through women's pension if they are born before 1951 and are retired in the ages 60-62 while not retiring through disability pension (using old-age pension). In the scientific use file of Insurance Account Sample (VSKT) 2014, we observe the exact retirement pathway. Source: SHARE-RV and VSKT 2014.

Table A.4: Share eligible and claimed for the old-age pension for women pathway for different groups

Subgroups	Control cohorts (Born 1948-1951) share eligible	Treatment cohorts (Born 1952-1955) share eligible	Control cohorts (Born 1948-1951) share claimed
Full sample	54.3%	55.1%	35.38%
High education	61.11%	57.35%	19.44%
Low education	49.84%	53.85%	20.10%
Married	51.57%	52.86%	20.88%
Unmarried	54.66%	61.62%	18.35%
West German	44.34%	52.58%	39.92%
East German	79.66%	63.95%	71.00%

Notes: Columns 1 and 2 of Table A.4 show the share of women born before and after 1951 who fulfil the eligibility criteria for the old-age pension for women at age 60. Columns 3 shows the share of women born before 1951 claimed old-age pension for women. We define eligibility for women's pension in SHARE-RV according to the law. Women are eligible if they have at least 15 pension years at age 60 and at least 10 years of the contribution periods to be acquired after age 40. Source: SHARE-RV.

Table A.5: Effects on the equivalized individual savings level using 1998 as control : RD-DD

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-55.701***	31.882	
	(14.465)	(43.871)	
Born after 1951=1 × post=1			-90.477
			(50.323)
Dependent Variable Mean	228.136	401.911	294.062
Observations	12,537	7699	20,236
R^2	0.007	0.015	0.015
		Couples	
Born after 1951	-75.568***	39.996	
	(12.443)	(30.212)	
Born after 1951=1 × post=1			-121.049**
			(35.920)
Observations	9,766	6,247	16,013
R^2	0.006	0.013	0.014
Dependent Variable Mean	251.901	437.947	3245.332
		Singles	
Born after 1951	18.480	-2.832	
	(68.879)	(109.460)	
Born after 1951=1 × post=1			27.646
			(141.014)
Observations	2,771	1,452	4,223
R^2	0.006	0.008	0.008
Dependent Variable Mean	145.871	249.106	181.208
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.5 reports the RD estimates in the reform waves (column 1) and the control waves (1998 wave only, column 2) and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

Table A.6: Effects on savings rate by varying controls, RD-DD estimates

	Savings rate	Savings rate	Savings rate
		Full Sample	
Born after 1951=1 × post=1	-0.006	-0.006	-0.006
	(0.007)	(0.007)	(0.006)
Observations	25,198	25,198	24,843
Dependent Variable Mean	0.121	0.121	0.121
		Couples	
Born after 1951=1 × post=1	-0.015**	-0.015**	-0.015**
	(0.005)	(0.005)	(0.005)
Observations	20,134	20,134	19,908
Dependent Variable Mean	0.131	0.131	0.131
		Singles	
Born after 1951=1 × post=1	0.032	0.033	0.033
	(0.023)	(0.023)	(0.025)
Observations	5,064	5,064	4,935
Dependent Variable Mean	0.083	0.083	0.083
Cluster at birth cohort	✓	✓	✓
Year fixed effects		✓	✓
Further control variables			✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.6 show the RD-DD estimator without controls (columns 1), introduce year fixed effects (columns 2) and introduce the full number of control variables and year fixed effects (columns 3). The estimates are stable by varying the choices of controls.

Table A.7: RD-DD estimates by bandwidth

Saving rates	BW=3	BW=4	BW=5
Full Sample	-0.003	-0.006	0.001
	(0.005)	(0.006)	(0.006)
Observations	18,808	24,843	30,251
Couple	-0.012*	-0.015***	-0.006
	(0.005)	(0.005)	(0.007)
Observations	15,083	19,908	24,312
Single	0.036	0.033	0.036
	(0.021)	(0.025)	(0.027)
Observations	3,726	4,935	5,939
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.7 show the RD-DD estimator by various bandwidth choices.

Table A.8: Effects on households' savings rates, with a quadratic age trend

	RD reform year	RD control years	RD-DD
		Full Sample	
Born after 1951	-0.009	-0.004	
	(0.005)	(0.002)	
Born after 1951=1 × post=1			-0.006
			(0.006)
Observations	11,239	13,604	24,843
Dependent Variable Mean	0.094	0.118	0.107
		Couples	
Born after 1951	-0.014**	0.001	
	(0.006)	(0.001)	
Born after 1951=1 × post=1			-0.015**
			(0.005)
Observations	8,710	11,198	19,908
Dependent Variable Mean	0.094	0.118	0.107
		Singles	
Born after 1951	0.010	-0.026**	
	(0.014)	(0.010)	
Born after 1951=1 × post=1			0.033
			(0.025)
Observations	2,529	2,406	4,935
Dependent Variable Mean	0.094	0.118	0.107
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.8 show the RD-DD estimator with a quadratic age trend.

Table A.9: Effects on households savings rates, with a quadratic cohort trend

	RD reform year	RD control years	RD-DD
		Full Sample	
Born after 1951	0.004	-0.002	
	(0.003)	(0.001)	
Born after 1951=1 × post=1			0.006
			(0.004)
Observations	11,239	13,604	24,843
Dependent Variable Mean	0.094	0.118	0.107
		Couples	
Born after 1951	0.001	0.003	
	(0.006)	(0.002)	
Born after 1951=1 × post=1			-0.002
			(0.006)
Observations	8,710	11,198	19,908
Dependent Variable Mean	0.094	0.118	0.107
		Singles	
Born after 1951	0.015*	-0.026***	
	(0.008)	(0.003)	
Born after 1951=1 × post=1			0.042***
			(0.011)
Observations	2,529	2,406	4,935
Dependent Variable Mean	0.094	0.118	0.107
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.9 show the RD-DD estimator with a quadratic cohort trend.

Table A.10: RD-DD, savings rate, robust by male restrictions

	Baseline			Male partners not retired		
	RD reform year	RD control year	RD-DD	RD reform year	RD control year	RD-DD
Born after 1951	-0.016**	0.001		-0.014***	0.002	
	(0.005)	(0.001)		(0.003)	(0.002)	
Born after 1951=1 × post=1			-0.015**			-0.015***
			(0.005)			(0.003)
Cluster at birth cohort	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Further control variables	✓	✓	✓	✓	✓	✓
Observations	6,577	11,015	17,592	6,673	10,736	17,409
R^2	0.008	0.003	0.006	0.007	0.003	0.006
Dependent Variable Mean	0.126	0.143	0.136	0.127	0.143	0.137

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.10 shows the robustness by varying restrictions made to the male partners. We compare the baseline impacts on couples with estimates using samples with male partners whom are not retired.

Table A.11: Effects on household savings rates - DID method

	Full sample	Couple	Single	Full sample	Couple	Single	Full sample	Couple	Single	Full sample	Couple	Single
Treated	-0.008	-0.012**	0.010	-0.004	-0.006**	0.002	-0.004	-0.002	-0.017	-0.006**	-0.014***	0.027**
	(0.004)	(0.005)	(0.023)	(0.005)	(0.002)	(0.027)	(0.005)	(0.002)	(0.026)	(0.000)	(0.000)	(0.002)
Year indicator (1 if > 1998)	-0.006	-0.003	-0.023	0.001	-0.002	0.007	0.019	0.040*	-0.080	0.003	-0.056**	0.220*
	(0.012)	(0.016)	(0.015)	(0.014)	(0.016)	(0.051)	(0.018)	(0.013)	(0.081)	(0.006)	(0.004)	(0.020)
Cohort indicator	-0.001	0.002	-0.014	-0.001	0.000	-0.009	0.002	0.003	-0.005	-0.001	0.010**	-0.045**
	(0.004)	(0.003)	(0.009)	(0.004)	(0.003)	(0.011)	(0.003)	(0.002)	(0.008)	(0.001)	(0.000)	(0.003)
Sample	1948 −1955, 4 bdw			1949−1954, 3 bdw			1950−1953, 2 bdw			1951−1952, 1 bdw		
Cluster at birth cohort	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Further control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	24,843	19,908	4,935	18,743	15,017	3,726	12,548	10,050	2,498	6,224	4,944	1,280
R^2	0.022	0.011	0.011	0.018	0.010	0.012	0.018	0.009	0.016	0.023	0.012	0.018
Dependent Variable Mean	0.132	0.142	0.086	0.133	0.143	0.087	0.133	0.143	0.089	0.136	0.145	0.098

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.11 shows the DID estimates using samples 4 years, 3 years, 2 years and 1 year to the left and right of the cutoff. The control group is defined as cohorts born after 1951 and the post period is after 1993 (waves 2003 and 2008).

Table A.12: Effects on household savings rates - event study

	Full sample	Couple	Single	Full sample	Couple	Single	Full sample	Couple	Single	Full sample	Couple	Single
Born after 1951× Year 1993	0.001	0.006	-0.019	-0.002	-0.003	-0.008	0.008	-0.001	0.044	-0.008**	-0.005***	0.006**
	(0.007)	(0.004)	(0.030)	(0.012)	(0.006)	(0.042)	(0.011)	(0.008)	(0.027)	(0.000)	(0.000)	(0.000)
Born after 1951×Year 1998	-0.001	0.002	-0.017**	0.003	0.007**	-0.016	-0.003	0.004	-0.032***	0.000	0.005**	-0.023**
	(0.003)	(0.004)	(0.007)	(0.003)	(0.003)	(0.011)	(0.004)	(0.003)	(0.005)	(0.000)	(0.000)	(0.001)
Born after 1951×Year 2003	-0.008	-0.008	-0.011	-0.001	0.002	-0.016	-0.007	0.002	-0.048	-0.004**	-0.004**	-0.013*
	(0.005)	(0.006)	(0.015)	(0.005)	(0.003)	(0.024)	(0.007)	(0.003)	(0.023)	(0.000)	(0.000)	(0.001)
Born after 1951×Year 2008	-0.011*	-0.016	0.010	-0.010	-0.015	0.006	0.000	0.004	-0.016	-0.009*	-0.004*	-0.020*
	(0.005)	(0.010)	(0.027)	(0.006)	(0.008)	(0.026)	(0.002)	(0.007)	(0.024)	(0.001)	(0.000)	(0.002)
Sample	1948 −1955, 4 bdw			1949−1954, 3 bdw			1950−1953, 2 bdw			1951−1952, 1 bdw		
Cluster at birth cohort	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Further control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	24,843	19,908	4,935	18,743	15,017	3,726	12,548	10,050	2,498	6,224	4,944	1,280
R^2	0.022	0.011	0.011	0.018	0.010	0.012	0.018	0.009	0.016	0.023	0.012	0.018
Dependent Variable Mean	0.132	0.142	0.086	0.133	0.143	0.087	0.133	0.143	0.089	0.136	0.145	0.098

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.12 shows the event study estimates using samples 4 years, 3 years, 2 years and 1 year to the left and right of the cutoff. The control group is defined as cohorts born after 1951 and the post periods start in 2003 wave.

Table A.13: Effects on households' savings rates using a placebo sample for wave 2003

	Full sample	Couples	Singles
Younger than 52 in non-reform waves	0.000	0.000	0.003
(pooled placebo sample)	(0.005)	(0.003)	(0.022)
Observations	10,079	8,206	1,873
R^2	0.019	0.005	0.032
Dependent Variable Mean	0.125	0.135	0.084
Born after 1941 (younger than 52) in 1993	0.009	0.005	0.023
	(0.009)	(0.006)	(0.027)
Observations	4,787	3,898	889
R^2	0.034	0.008	0.040
Dependent Variable Mean	0.126	0.139	0.069
Born after 1946 (younger than 52) in 1998	-0.001	-0.009	0.029
	(0.006)	(0.009)	(0.037)
Observations	5,292	4,308	984
R^2	0.012	0.004	0.023
Dependent Variable Mean	0.124	0.131	0.097
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.13 shows the RD estimates of being younger than age 52 in a pooled placebo sample, which consists of older cohorts in 1993 (cohorts 1938-1945) and 1998 (cohorts 1943-1950). The pooled placebo sample has the same age composition as the baseline sample in 2003 and the same age cutoff at 51.

Table A.14: Effects on households savings rates using a placebo sample for wave 2008

	Full sample	Couples	Singles
Younger than 57 in non-reform waves	-0.008**	-0.007	-0.012
(pooled placebo sample)	(0.003)	(0.004)	(0.012)
Observations	9,643	7,666	1,977
R^2	0.022	0.012	0.020
Dependent Variable Mean	0.106	0.114	0.072
Born after 1936 (Younger than 57 in 1993)	-0.007	-0.009	-0.001
	(0.007)	(0.008)	(0.018)
Observations	4,435	3,514	921
R^2	0.035	0.016	0.040
Dependent Variable Mean	0.109	0.119	0.066
Born after 1941 (Younger than 57 in 1998)	-0.008**	-0.004	-0.017***
	(0.003)	(0.004)	(0.005)
Observations	5,208	4,152	1,056
R^2	0.015	0.010	0.015
Dependent Variable Mean	0.103	0.109	0.078
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.14 shows the RD estimates of being younger than age 57 in a pooled placebo sample, which consists of older cohorts in 1993 (cohorts 1932-1940) and 1998 (cohorts 1938-1945). The pooled placebo sample has the same age composition as the baseline sample in 2008 and the same age cutoff at 56.

Table A.15: Impact on household savings rates using men as a placebo sample

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-3.255	-6.004*	
	(6.109)	(2.774)	
Born after 1951=1 × post=1			2.864
			(5.140)
Observations	10,466	13,233	23,699
R^2	0.007	0.001	0.005
Dependent Variable Mean	0.124	0.140	0.133
		Couples	
Born after 1951	-5.193	-6.949*	
	(6.463)	(3.051)	
Born after 1951=1 × post=1			1.797
			(6.648)
Observations	9,135	11,983	21,118
R^2	0.006	0.000	0.004
Dependent Variable Mean	0.128	0.142	0.136
		Singles	
Born after 1951	14.424	3.925	
	(10.748)	(5.211)	
Born after 1951=1 × post=1			8.674
			(13.931)
Observations	1,331	1,250	2,581
R^2	0.022	0.003	0.018
Dependent Variable Mean	0.094	0.123	0.108
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.15 reports the RD estimates in the reform waves (column 1) and the control wave (column 2), and the RD-DD estimates in column 3. The placebo sample consists of households with male members born between 1948 and 1955. An indicator for male member born since 1950 is the instrument. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

Table A.16: Effects on household savings rates - DID, men as a placebo sample

	Full sample	Couple	Single
Treated	-0.005	-0.003	-0.016
	(0.006)	(0.005)	(0.016)
Year indicator (1 if >1998)	-0.016	-0.014	-0.026
	(0.019)	(0.020)	(0.026)
Cohort indicator	-0.001	-0.001	0.003
	(0.007)	(0.008)	(0.008)
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓
Observations	23,699	21,118	2,581
R^2	0.005	0.004	0.018
Dependent Variable Mean	0.140	0.142	0.123

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.16 shows the DID estimates for a placebo sample consisting of households with male members born between 1948 and 1955.

Table A.17: Effects on household savings rates -Event study, men as a placebo sample

	Full sample	Couple	Single
Born after 1951*Year 1993	-0.014*	-0.007	-0.016
	(0.006)	(0.006)	(0.017)
Born after 1951*Year 1998	-0.006	0.001	0.131*
	(0.005)	(0.003)	(0.057)
Born after 1951*Year 2003	-0.005	-0.001	0.204***
	(0.007)	(0.005)	(0.007)
Born after 1951*Year 2008	0.003	0.006	0.050**
	(0.007)	(0.006)	(0.017)
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓
Observations	19,369	21,118	2,581
R^2	0.004	0.004	0.019
Dependent Variable Mean	0.140	0.142	0.123

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.17 shows the event study estimates for a placebo sample consisting of households with male members born between 1948 and 1955.

Table A.18: Effects on household savings rates at placebo cutoffs

	Full Sample		Couples		Single	
	RD reform year	RD-DD	RD reform year	RD-DD	RD reform year	RD-DD
Placebo cutoff 1950	-0.005	-0.004	-0.000	-0.000	-0.022	-0.018
	(0.006)	(0.007)	(0.010)	(0.010)	(0.013)	(0.011)
Observations	10,217	21,384	7,963	17,132	2,254	4,252
Placebo cutoff 1953	0.000	0.004	-0.012**	-0.010	0.042***	0.056***
	(0.004)	(0.003)	(0.005)	(0.006)	(0.005)	(0.012)
Observations	11,653	22,271	9,032	17,698	2,621	4,573
Placebo cutoff 1954	-0.001	-0.000	0.003	0.006**	-0.017	-0.027
	(0.003)	(0.007)	(0.002)	(0.002)	(0.019)	(0.030)
Observations	11,956	26,628	9,240	21,334	2,716	5,294
Cluster at birth cohort	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Further control variables	✓	✓	✓	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01 Table A.18 shows the RD-DD estimates at placebo cutoffs (born since 1950, 1952 and 1954).

Table A.19: Impact on household savings rates using 1998 as control : RD-DD

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-0.010*	-0.003	
	(0.005)	(0.004)	
Born after 1951=1 × post=1			-0.008**
			(0.003)
Observations	11,239	6,997	18,236
R^2	0.019	0.013	0.022
Dependent Variable Mean	0.109	0.132	0.117
		Couples	
Born after 1951	-0.015**	-0.001	
	(0.006)	(0.006)	
Born after 1951=1 × post=1			-0.015***
			(0.003)
Observations	8,710	5,663	14,373
R^2	0.012	0.002	0.012
Dependent Variable Mean	0.117	0.140	0.126
		Singles	
Born after 1951	0.007	-0.011	
	(0.015)	(0.008)	
Born after 1951=1 × post=1			0.019
			(0.015)
Observations	2,529	1,334	3,863
R^2	0.014	0.013	0.015
Dependent Variable Mean	0.080	0.097	0.086
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.19 reports the RD estimates in the reform waves (column 1) and the control wave (1998 wave only, column 2), and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

Table A.20: Impact on monthly equivalized household disposable income using 1998 as control: RD-DD

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	6.937	34.345	
	(33.261)	(37.5439)	
Born after 1951=1 × post=1			-35.336
			(32.209)
Dependent Variable Mean	2115	2757	2359
Observations	12,537	7,699	20,236
R^2	0.156	0.182	0.213
		Couples	
Born after 1951	-6.669	45.066	
	(25.625)	(53.223)	
Born after 1951=1 × post=1			-63.186
			(46.045)
Observations	9,766	6,247	16,013
R^2	0.141	0.172	0.202
Dependent Variable Mean	2236	2876	2485
		Singles	
Born after 1951	58.545	-57.121**	
	(66.525)	(21.691)	
Born after 1951=1 × post=1			120.369
			(76.471)
Observations	2,771	1,452	4,223
R^2	0.133	0.177	0.191
Dependent Variable Mean	1698	2252	1888
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.20 reports the RD estimates in the reform waves (column 1) and the control wave (1998 wave only, column 2), and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

Table A.21: Impact on monthly equivalized consumption expenditures using 1998 as control: RD-DD

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	39.527	31.163	
	(26.076)	(32.859)	
Born after 1951=1 × post=1			3.011
			(48.559)
Dependent Variable Mean	1556	2026	1734
Observations	12,537	7699	20236
R^2	0.085	0.096	0.130
		Couples	
Born after 1951	51.439*	35.393	
	(24.234)	(24.422)	
Born after 1951=1 × post=1			9.665
			(31.790)
Observations	9,766	6,247	16,013
R^2	0.080	0.095	0.127
Dependent Variable Mean	1616	2081	1797
		Singles	
Born after 1951	2.437	-13.188	
	(64.312)	(156.740)	
Born after 1951=1 × post=1			16.313
			(218.700)
Observations	2,771	1,452	4,223
R^2	0.082	0.096	0.126
Dependent Variable Mean	1350	1796	1502
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.19 reports the RD estimates in the reform waves (column 1) and the control wave (1998 wave only, column 2), and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables and cluster the standard error at the cohort level. The estimates are obtained from a linear specification with a four-year bandwidth.

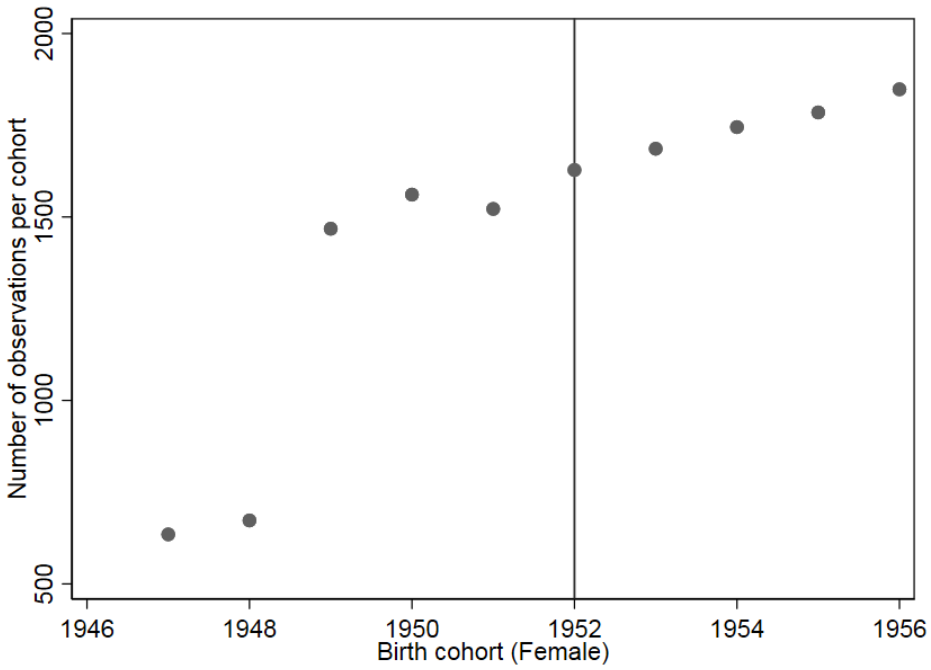


Figure A.1: Number of households by cohort of female

Notes: Figure A.1 shows the density by birth cohorts of our baseline sample.

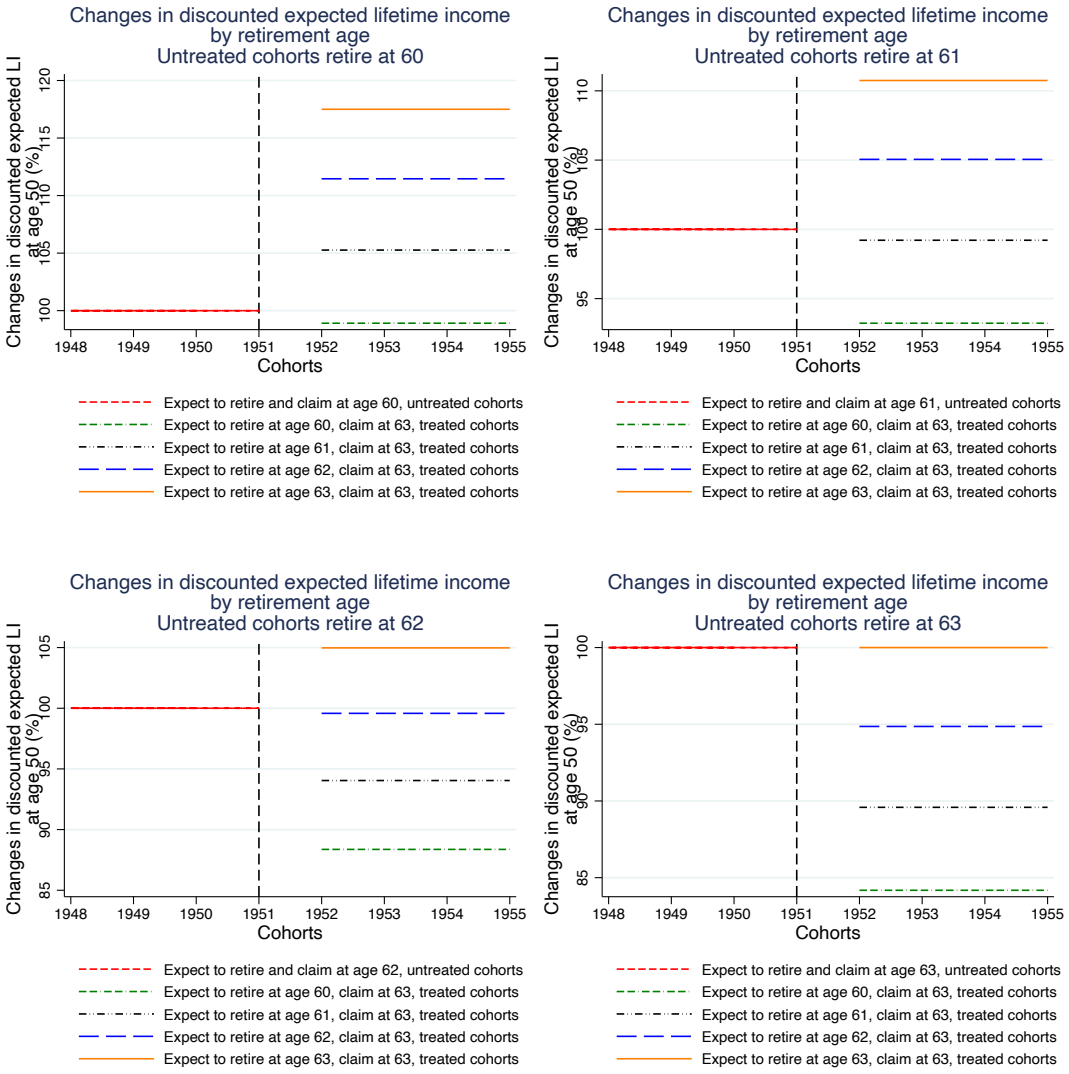
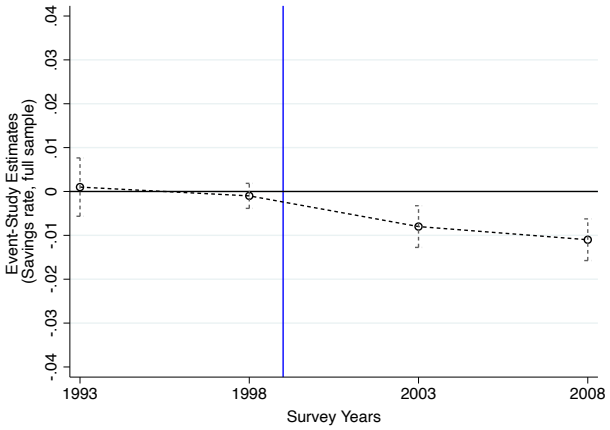
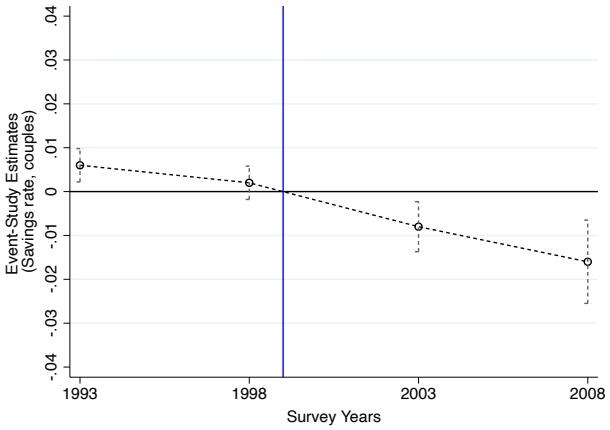


Figure A.2: Illustration of changes in expected lifetime income by counterfactual retirement age

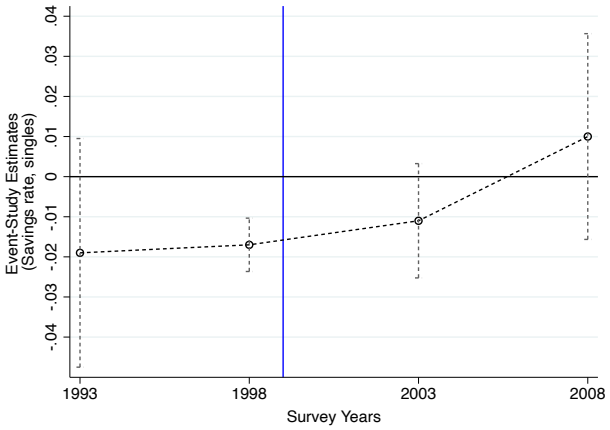
Notes: Figure A.2 illustrates the percentage changes in expected lifetime income when individuals retire age claim at age 60, 61, 62 and 63 in absence of the reform.



(a) Full sample



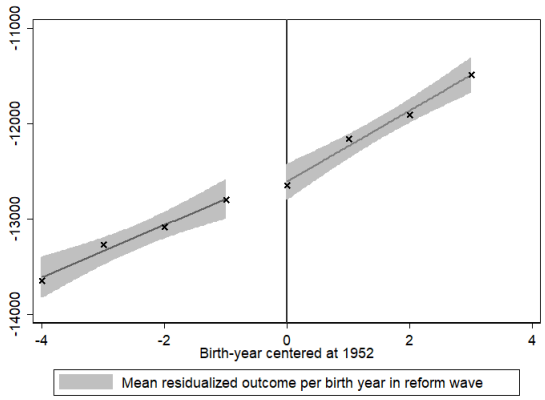
(b) Couples



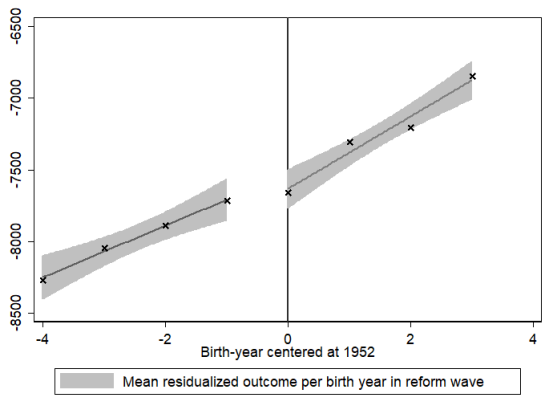
(c) Singles

Figure A.3: Event study plots: savings rate

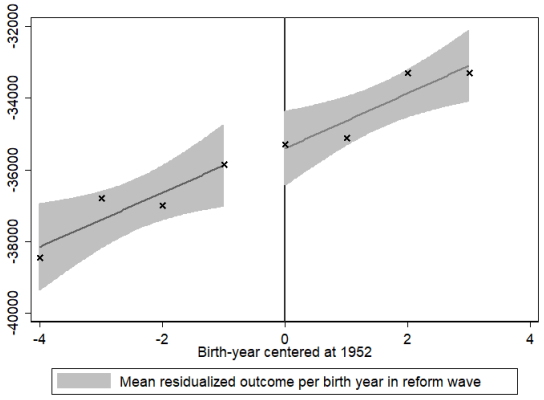
Notes: Figure A.3 presents the event study coefficients by survey year for the full sample, married households and single households. The results are obtained using the baseline sample of cohorts from 1948 to 1955.



(a) Full sample



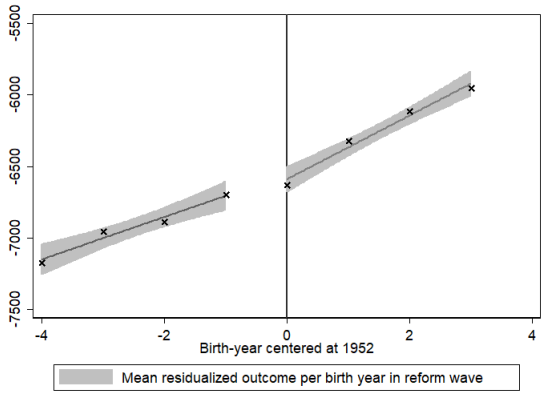
(b) Couples



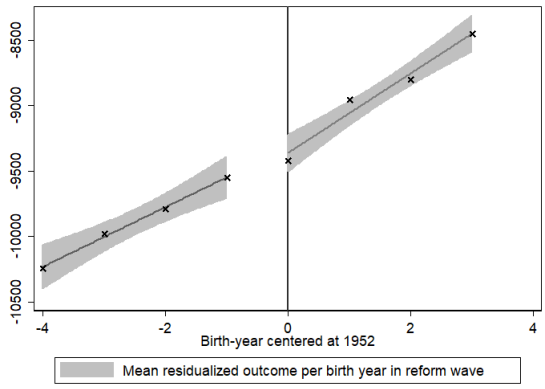
(c) Singles

Figure A.4: Residualized equivalized individual savings level by marital status

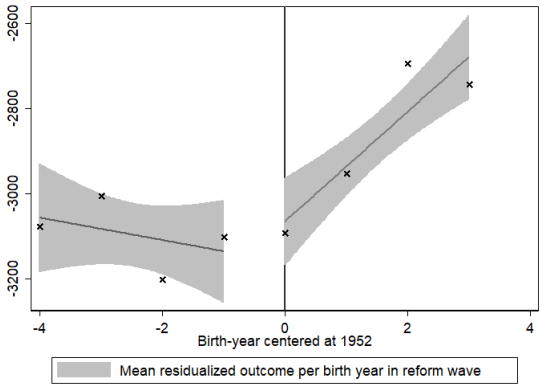
Notes: Figure A.4 presents graphic evidence on the relationship between birth year and the residualized individual monthly savings level in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform period are from an estimation model using 1998 wave only. The solid lines are the linear fitted lines. The shaded areas indicate 95 percent confidence interval.



(a) Full sample,level



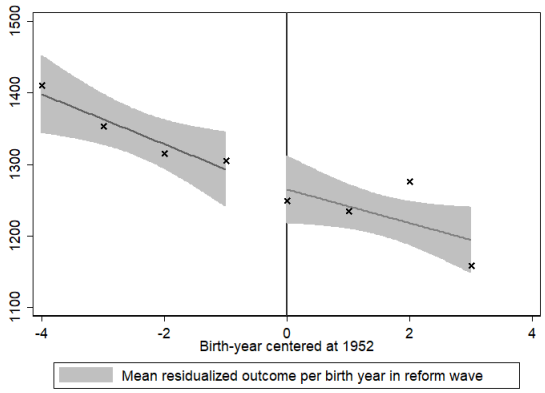
(b) Couples, level



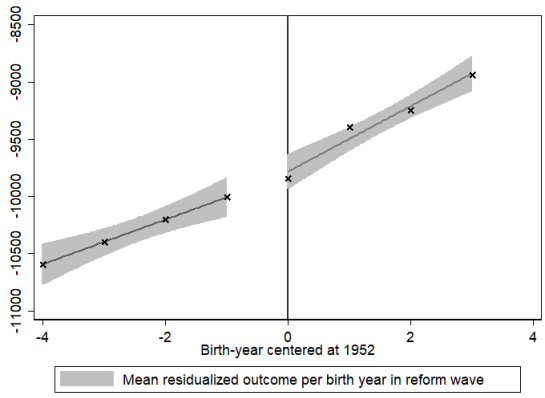
(c) Singles,level

Figure A.5: Residualized monthly equivalized household disposable income by marital status

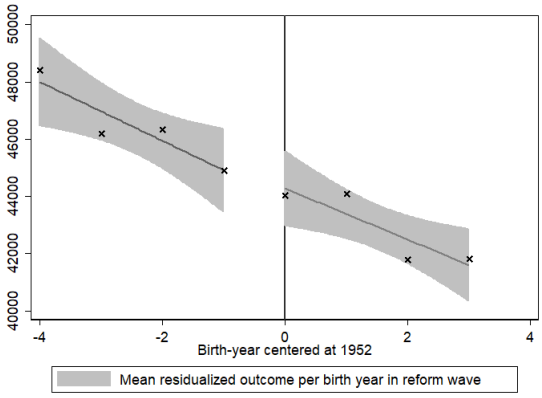
Notes: Figure A.5 presents graphic evidence on the relationship between birth year and the equivalized monthly disposable income in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform period are from an estimation model using 1998 wave only. The solid lines are the linear fitted lines. The shaded areas indicate 95 percent confidence interval.



(a) Full sample.level



(b) Couples,level



(c) Singles,level

Figure A.6: Residualized monthly equivalized consumption expenditures by marital status

Notes: Figure A.6 presents graphic evidence on the relationship between birth year and the equivalized monthly consumption expenditures in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform period are from an estimation model using 1998 wave only. The solid lines are the linear fitted lines. The shaded areas indicate 95 percent confidence interval.

B. Data Appendix

B.1. The Sample Survey of Household Income and Expenditure (EVS)

The Sample Survey of Household Income and Expenditure in Germany (Einkommens- und Verbrauchsstichprobe – EVS) is a large cross-sectional survey of about 40,000 households conducted by the German Federal Statistical Office. It takes place every five years.

Representativeness: Participation in the EVS is voluntary, therefore, there are two limitations related to external validity. First, there is limited representativeness at the very top end of the distribution because the participation rates of this group are low. , The income threshold amounted to a monthly net household income of 35,000 Deutschmark (17.895 €) in the 1993 and 1998 waves, and 18,000 € in the 2003, 2008 and 2013 waves (Dustmann et al. (2018)). Second, the EVS underestimates income from self-employment or capital income, which is a well-known problem of household surveys.

We do not think those two limitations will affect our estimates to a larger extent. First, households at the very top of the income distributions are less likely to be the compliers of the pension reform because their retirement decisions are less dependent on the availability of the public pension. Moreover, this restriction only affects less than 1% of all German households (Becker et al. (2003)) and only drops the top income earners whose responses to the reform are by and large muted. Second, the reform mainly affects lifetime labor income rather than income from self-employment or capital income. Underestimated income from self-employment or capital income might increase the measures of savings rates, yet it should affect treated and control cohorts similarly. Moreover, this underestimation is a common issue for household surveys. Becker et al. (2003) and Becker (2014) provide a comparison of household income data in EVS and SOEP (Socio-Economic Panel Study). They find that SOEP also underreports income from self-employment or capital income

Survey Method and Key Variables: The questionnaire behind the EVS dataset has three parts. First, at the beginning of each year, participants are asked about several important household and household-member characteristics (first part) as well as wealth and property (second part). The flow variables are collected in the third part in the diary. The flow variables used in our analysis are income, expenditure, and savings. Households are asked to fill in income and expenditure and payments in bank accounts etc. into a table. For example, below is a table asking about expenditure on "restaurants, canteens, hotels and boarding house". Participants need to fill in how much they spent on each activity during the three consecutive months. There

are no specific questions asked. For more information, see the description on the German statistics office about the EVS and the exact questionnaires for EVS2013.

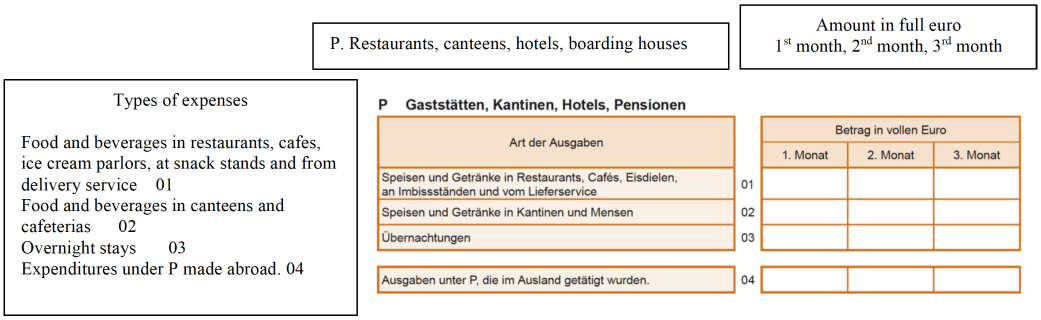


Figure B.1: Example of the EVS Dairy

Source: Economic accounts: Income and Consumption Survey Task, Method and Implementation 2013 (Wirtschaftsrechnungen: Einkommens und Verbrauchsstichprobe Aufgabe, Methode und Durchführung 2013)

Using the household income and expenditure diary, the German Statistical Office created a list of variables in the EVS. No specific questions are asked about the households about savings levels or savings rates. The variable "savings level" is provided by the EVS, which is constructed by the German Statistical Office using the following formula:

Savings level= expenses to create property values + expenses to create monetary values + expenses to paying back loans, paying interest – income from loans – income from interests

We reconstruct the variable "savings level" using the above formula and obtain the same value as provided by the EVS. We also define three main savings categories: monetary savings (paying into bank accounts, buying a stock), property savings (buying gold, a house, etc.), and loan payback (paying interest, etc.). Savings are then the sum of differences of these categories with their counterparts. For example, the counterpart of monetary savings is taking money from the bank, the counterpart of property savings is selling gold, the counterpart of loan payback is taking on new loans.

The variable "household net disposal income" is also provided by the EVS. We create the variable "savings rates" by dividing savings level by household net disposable income. In the baseline sample, savings level and savings rates are trimmed to drop the bottom and top 1%. Similarly, the measures of consumption expenditure are also provided by the EVS, which are generated from the diary.

The time frame over which the diary is kept has changed over the years. Specifically, the diary used to be an annual diary until 1993 and has been switched to a quarterly one since 1998. We

harmonize the dataset and convert all variables to a monthly level. All household characteristics are questioned at the beginning of the year and refer to the same year. We adjust for CPI and convert monetary variables in Euros and prices of 2003.

Attrition Between Surveys: The EVS is a repeated cross-sectional questionnaire. Households in the questionnaire change every 5 years (they might be the same person, however, there is no personal id to indicate the repeated questionnaire participants). Because the EVS is not a panel dataset, attrition between surveys, in this sense, is not a problem.

Attrition During Surveys: A During each wave, households are questioned at the beginning of the survey year (1st of January) about household and household member characteristics and holdings of properties. Then, households are given the diary, in which they fill in income and expenses for the three consecutive months. For the EVS 1998, the statistical Office reports that 10% of households that started the questionnaire did not complete the diary. In this sense, there are some attritions in terms of completing the diary. However, only those households that finished the questionnaire are included in the EVS. Therefore, the final dataset contains information on households that complete the main questionnaire at the beginning of the survey year and the diary.

Survey Weights To guarantee representativeness of the outcomes, EVS provides weights. These weights are produces with the aim to meet important population means of the German Microcensus. However, in our estimation, we do not use the weighting schemes in the RD analysis. Instead we control for household type, social status of the main earner and age.

B.2. The Survey on Health, Aging and Retirement in Europe (SHARE)

In order to asses the change in expected retirement ages due to the 1999 pension reform we make use of the German part of the Survey of Health, Ageing and Retirement in Europe (SHARE) . SHARE is a representative panel data set on European citizens aged 50 and older. SHARE offers specific information on the lives on elder individuals. In six consecutive waves respondents are asked about several relevant Socio-Economic variables as well as age specific information on an individual as well as an household level. [1] Among others, individuals are asked about their expected retirement age. The exact question is: *"At what age do you yourself expect to start collecting this pension payment for the first time?"* This question is asked in all waves. We therefore construct a data set using waves 1-6 with all women aged younger than 60 [2] and born between 1947 and 1956 (five years before after the cut-off). We use raw information

[1] Wave 3 only includes retrospect information without new information about respondents.

[2] Women born before 1952 can retire as early as age 60, therefore the expected retirement age for this group is of no interest in the comparison. (what does this mean?)

given by respondents on their gender, birth-age, age per wave and expected retirement age. We then compare the expected retirement ages for women born around the cut-off. In Table 7, we show both the mean differences in expected retirement ages and the first difference in expected retirement ages with controlling for age and East Germany. In a last regression we include cohort trends that we allow to break at the cut-off point.

To analyze eligibility of women for women's pension we make use of the matched SHARE-RV data set. Some respondents except that their information from the official pension insurance records are linked to their SHARE information (see description on SHARE website). We therefore have exact information on the number of waiting years at age 60 as well as the exact number of waiting years acquired since age 40. Further, we make use of SHARE-RV information on the kind of pension used by any given retired woman. All women that are retired between the ages 60 and 62, born before 1952 and use old-age pension (in contrast to disability pension) then must be using women's pension.

C. Additional Background on German Pension System

C.1. Details on the legislations to abolish women's pension pathway

The laws implementing the pension reforms mentioned in this paper include the *Rentenreformgesetz 1992*[3], the *Wachstums- und Beschäftigungsförderungsgesetz 1996*[4], the *Rentenreformgesetz 1999*[5], and the *RV-Nachhaltigkeitsgesetz 2004*[6]. The reform was drafted in October 1997. Despite firm rejection by the upper house (Bundesrat; then dominated by social-democratic party SPD), which had little options to intervene, the law was passed with the votes of the then ruling conservative CDU/CSU/FDP coalition (Christian Democratic Union/ The Christian Social Union/ The Free Democratic Party). The law was published in the law Gazette (Bundesgesetzblatt) on December 17, 1997 and will become effective on January 1, 1999. Technically, the affected cohorts know about the exact rules of the implementation since December 17, 1997.

However, the following year 1998 was dominated by the upcoming federal election. The campaign created a lot of uncertainty about whether the reform will be revoked. The SPD and Greens were leading the conservative bloc by as much as 4-12% throughout the year 1998 according to all major pollsters (e.g. see polling results provided by the forsa Institute for Social Research and Statistical Analysis. The opposition to the recently passed but not yet effective

[3] Abbr. as RRG 1992, http://pdok.bundestag.de/extrakt/ba/WP11/1183/118320.html
[4] Abbr. as WFG 1996, http://pdok.bundestag.de/extrakt/ba/WP13/629/62941.html
[5] Abbr. as RRG 1999, http://pdok.bundestag.de/extrakt/ba/WP13/656/65676.html
[6] http://pdok.bundestag.de/extrakt/ba/WP15/380/38047.html

pension reform 1999 played a prominent role in the election programme of the SPD. The SPD and the Green Party coalition has won the election. However, even two months after they took power in Sept 1998, it still remained opaque, which of the elements of the 1999 reform were to be revoked and where reform would be going even further (Bulmahn (1998)). Therefore, it is reasonable to assume that in 1998 the run-up to the election, the affected households are uncertain whether the changes will become effective in 1999 and are unlikely to adapt to a pension reform of the old government that was unlikely to remain in place.

In the end, the SPD/Greens didn't revoke the abolishment of women's pension. In the following years (2000/2001), the SPD/Greens proceeded with their own major pension reform and made further adjustments. The biggest reform steps included the re-organization of the reduced earnings-capacity pensions, the introduction of a sustainability factors linked to demographics and the introduction of a private pension plan pillar.

In summary, even though the exact rules were announced in December 1997, there was political uncertainty about the actual implementation of the reform in 1998.

C.2. Retirement pathways

Several alternate pathways make retiring before the regular retirement age of 65 possible in Germany. There are four main early retirement pathways: old-age pensions for women, old-age pensions due to unemployment (and part-time work), old-age pensions for the long-term insured, and old-age pensions for severely disabled persons. Each pathway has its own eligibility conditions. Each pathway also has its own full retirement age (FRA) and early retirement age (ERA). For example, age 60 is the ERA for women's pension pathway. Age 63 is the ERA for the long-term insured pathway.

The table below highlights the changes in ERA, FRA and the corresponding deductions when claim at the ERA for cohorts 1948 to 1955. For example, the ERA via the pension for women stayed at 60 for cohorts born before 1951. Thus, the financial penalties for claiming a pension at age 60 via women's pension remained at 18% for cohorts from 1948 to 1951. The 1999 pension reform abolished the women's pension for cohorts born after 1951.

For women born before 1952, in addition, to claim the standard old-age pension at age 65, which requires 5 years of contribution, there are four alternative pathways into early retirement: old-age pension for women, old-age pension for long-term insured, old-age pension for the unemployed and old-age pension for severely disabled. Old-age pension for women and old-age pension for severely disabled allow eligible individuals to claim pension as early as age 60. Yet, old-age

Table C.1. Changes in pension parameters for cohorts 1948 to 1955

	1948	1949	1950	1951	1952	1953	1954	1955	Reform Year
Regular/statutory retirement age	$65\frac{2}{12}$	$65\frac{3}{12}$	$65\frac{4}{12}$	$65\frac{5}{12}$	$65\frac{6}{12}$	$65\frac{7}{12}$	$65\frac{8}{12}$	$65\frac{9}{12}$	2007
Pension for women (ERA^w)	60	60	60	60	-	-	-	-	1997
Pension for women (FRA^w)	65	65	65	65	-	-	-	-	1997
Deductions at ERA^w	18%	18%	18%	18%	-	-	-	-	1992
Pension for long-term insured (ERA^l)	63	63	63	63	63	63	63	63	
Pension for long-term insured (FRA^l)	65	$65\frac{3}{12}$	$65\frac{4}{12}$	$65\frac{5}{12}$	$65\frac{6}{12}$	$65\frac{7}{12}$	$65\frac{8}{12}$	$65\frac{9}{12}$	1992/2017
Deductions at ERA^l	7.2%	8.1%	8.4%	8.7%	9.0%	9.3%	9.6%	9.9%	1992
Pension for unemployed (ERA^u)	62	63	63	63	-	-	-	-	1997
Pension for unemployed (FRA^u)	65	65	65	65	-	-	-	-	1992
Deductions at ERA^u	10.8%	7.2%	7.2%	7.2%	-	-	-	-	
Pension for severely disabled(ERA^d)	60	60	60	60	$60\frac{6}{12}$	$60\frac{7}{12}$	$60\frac{8}{12}$	$60\frac{9}{12}$	2007
Pension for severely disabled (FRA^d)	63	63	63	63	$63\frac{6}{12}$	$63\frac{7}{12}$	$63\frac{8}{12}$	$63\frac{9}{12}$	1992/ 2007
Deductions at ERA^d	10.8%	10.8%	10.8%	10.8%	10.8%	10.8%	10.8%	10.8%	

Notes: Authors' own calculation according to the SBG VI. The ERA, FRA and deductions are those for cohorts born in December that year.

pension for the severely disabled is for people who have lost at least 50% of their earning capacity due to severe health conditions.

For women born since 1952, in addition to claiming the standard old-age pension at age 65, there are only two alternative pathways into early retirement: old-age pension for long-term insured and old-age pension for severely disabled. They can no longer retire through the women's pension pathway and the unemployment pathway. Unless they are qualified for a disability pension, the earliest possible retirement age is age 63 with a 9% penalty for early claiming via the pension for long-term insured. The ERA of the long-term insured pathway remained at age 63, while the FRA started to increase to 65 and 3 months for cohort 1949 and will increase at the same pace as the SRA for cohorts 1950 to 1964 and reaches age 67 in the year 2030. The eligibility condition for the old-age pension for the long-term insured is 35 years of contribution, including child-raising periods. These eligibility conditions remain unchanged.

D. Model

D.1. Three-period life cycle framework

We build a three-period life cylce model according to Feldstein (1974, 1976), which accommodates a loss in pension generosity caused by a life of ERA. In this life cycle framework, an individual lives for three periods, dies afterwards and has no children. Individuals have perfect foresight and smooth consumption over the lifecycle. We assume that an individual lives for three periods, dies afterwards and has no children. In the first period, she always works and in the third period, she is always retired. She has perfect foresight and smooths consumption over the life cycle. We

assume the individual is single to avoid intra-household transfer decisions. She maximize the following life time utility:

$$U(c_1, c_2, c_3) = \sum_{t=1}^{3} \rho^{t-1} \frac{c_t^{1-\theta}}{1-\theta}, \tag{A.1}$$

where c_t is consumption in period t, The inter-temporal elasticity of substitution is $\frac{1}{\theta}$, and ρ is the discount factor.

We start with a baseline case which is characterized by one period of employment followed by two periods of non-employment. Retirement at the ERA, after period one, is assumed to be individually optimal. This corresponds to the regime for cohorts born before 1952. In period 1 the individual works, she earns a wage w and makes mandatory retirement contributions τ and privately saves the amount s. We assume no bequests and all wealth is consumed by the last period.

$$c_1 = w_1 - \tau_1 - s_1 \tag{A.2}$$

The saving rate (sr_t) for periods 1 to 3 (t = 1, 2, 3) is as follows

$$sr_1 = \frac{w_1 - \tau_1 - c_1}{w_1 - \tau_1} \tag{A.3}$$

Her contributes τ to finance pension benefits. In the baseline case, the individual's total public pension benefits equal total contributions made to the pension scheme.

$$\frac{ss}{(1+r)} + \frac{ss}{(1+r)^2} = \tau_1, \tag{A.4}$$

where r is the interest rate and ss is the per period social security benefits. When retired, the individual spends all private savings and social security benefits (ss).

$$\frac{c_2}{(1+r)} + \frac{c_3}{(1+r)^2} = s_1 + \frac{ss}{(1+r)} + \frac{ss}{(1+r)^2} = \tau_1 + s_1$$

We assume leisure and consumption are non-complementary, and the utility function is concave. For simplicity, we also assume a discount factor $\rho = 1$ and zero interest rates $r = 0$. According to the life cycle hypothesis, the individual smooths the marginal utility of consumption across periods to maximize utility over the life cycle. Accordingly, she saves an optimal amount s^*

such that $c_1 = c_2 = c_3$. Using the implications of consumption smoothing, we have the optimal savings and consumption s^* and c^*:

$$s^* = \frac{2w_1 - 3\tau}{3} = \frac{2}{3}w_1 - \tau \tag{A.5}$$

$$\Leftrightarrow c^* = \frac{w_1}{3} \tag{A.6}$$

Now, let's consider the case where the earliest possible age to claim pension is increased by one period. This corresponds to the new regime for women born in and after 1952. In this simple model, this translates to restricting access to pension benefits to period three. An individual copes with this shock by choosing a new savings level and adjusting employment decisions. For simplicity, we distinguish two extreme scenarios to highlight the importance of changes in future labor earnings: first, she re-optimizes and does *not* prolong her career; second, she re-optimizes and works one period *longer*, denoted by n and l. The new optimal savings are denoted by s_n^* and s_l^*, respectively.

In the first scenario, the individual finds it optimal to *not* prolong her career. She still works only in period 1. In period 1 she earns a wage w_1, save s_n and pays contributions τ_1 – same as the baseline case. However, pension is no longer accessible in period 2 due to the lift of the ERA. She consumes a share ϕ of private savings in period 2. In period 3, she retires and consumes the remaining savings $(1-\phi)s_n$ and pension benefits. See Eqs. (A.7) to (A.9) for a formal notation.

We incorporate an adjustment factor $\gamma < 1$ to reflect the actuarial unfairness in the German pension system.[7] $\gamma < 1$ represents the situation that, if pension claiming is delayed, the sum of pension benefits is smaller than the sum of contributions. Early retirement is always preferred.[8] Therefore, $ss = \gamma\tau_1$ with the following per period consumption levels:

$$c_{1n} = w_1 - \tau_1 - s_n \tag{A.7}$$

$$c_{2n} = \phi s_n \tag{A.8}$$

$$c_{3n} = (1-\phi)s_n + \gamma\tau_1 \tag{A.9}$$

[7] Benefits are often adjusted to account for the duration of benefit receipt, but this adjustment usually is not actuarially fair. Therefore, the incentives to continue employment after reaching the ERA are limited.

[8] Under reasonable assumptions of interest rates, life expectancy and time preferences, early retirement is financially beneficial in a net present value perspective. If it was not preferable, the incentive to claim as early as possible is smaller.

In optimum, she chooses c_n^* to smooth consumption. Since she lives for 3 periods, earns a wage w_1 in period 1, and loses the share $(1-\gamma)$ of contributions τ_1 due to the actuarial unfairness of the pension benefits, the new per period consumption and optimal savings are the following:

$$c_n^* = \frac{w_1 - (1-\gamma)\tau_1}{3} \tag{A.10}$$

$$s_n^* = \frac{2w_1 - (2+\gamma)\tau_1}{3} \tag{A.11}$$

The share of savings consumed in period 2 $\phi = \frac{w-(1-\gamma)\tau_1}{2w_1-(2+\gamma)\tau_1}$. Because $\gamma < 1$, it holds that $s_n^* > s^*$. In absence of an employment effect, we expect the lift of the ERA to increase savings and decrease consumption in period 1.

It is important to note that $\gamma < 1$ is not introduced by the reform but already embedded in the baseline case. The individual in the baseline case prefers to claim benefits as early as possible because of the actuarial unfairness of the public pension system. In the German context, it is reasonable to assume $\gamma < 1$, because a delay of pension claiming by 3 years results in slightly higher per period pension benefits but accrues a substantial loss in the net present value of pension wealth of 5% to 7% due to shortened pension duration. [9,10]

In the second scenario, we assume the individual works longer. The consumption pattern in this scenario is the following:

$$c_{1l} = w_1 - \tau_1 - s_{1l} \tag{A.12}$$

$$c_{2l} = w_2 - \tau_2 - s_{2l} \tag{A.13}$$

$$c_{3l} = \gamma(\tau_1 + \tau_2) + (s_{1l} + s_{2l}) \tag{A.14}$$

Now the individual works one more period and has to finance one period of retirement less. The optimal consumption level changes substantially. Imposing the consumption smoothing condition $c_1 = c_2 = c_3$, and let $w_1 = w_2 = w$, $s_1 = s_2 = s$, $\tau_1 = \tau_2 = \tau$, we find that, in comparison to the baseline case, consumption rises and savings per period declines, see Eqs. (A.15) and (A.16).

$$c_l^* = \frac{(w_1 + w_2 - 2(1-\gamma)t}{3} > c^* \tag{A.15}$$

$$s_l^* = \frac{w - (1+2\gamma)t}{3} < s^* \tag{A.16}$$

[9] Calculations are based on an individual with 30 years of employment at the average wage level. We assume a 3% internal discount rate, account for the 3.6% per year correction factor for postponing claiming, use current life expectancy tables, and a reasonable range of the expected future growth rate of pension benefits.

[10] Introducing borrowing constraints or concepts of uncertainty into the model leads to similar model implications as does actuarial unfairness.

The need to save decreases because of additional wage income in period 2, higher per period pension benefits through a longer contribution period, and a shorter period of non-employment that needs to be financed.

This simple three-period life cycle framework illustrates the importance of employment responses. We highlight that the lift of the ERA can result in higher or lower savings rates depending on the individual's labor supply response.[11]

$$s_l^* < s^* < s_n^* \tag{A.17}$$

$$c_l^* > c^* > c_n^* \tag{A.18}$$

Therefore, the effect of a shift of the ERA on the savings rate is an empirical question. Whether the employment effect of a change to the eligibility age is large enough to reduce the savings rate will be tested in the empirical part of this study.

D.2. Parameters in the illustrated budget constraint

Here, we explain the parameters used to produce Figure 2. The taxable wage income is after social security contribution (SCC) and child allowance. Healthcare insurance is almost always 100% deductible during the sample period. Before 2005, pension contributions were 100% tax-free. As of 2005, to balance the changes in pension income tax, 60% of pension contributions were tax-free, and it increased by 2% each year. In 2025, 100% of contributions will be taxed. For simplicity, we assume all SCC are tax deductible.

The social security contribution (SSC) includes contributions to healthcare insurance, long-term care insurance, unemployment insurance and pension insurance. The average SSC is around 20% of gross wage income. The baseline budget set is constructed for the sample of the married female without dependent children. According to online tax calculator [12], the average tax rate of the married individual with average wage income and whose spouse makes zero income is 0.12.

The public pension benefits are calculated on a complex formula of individual career earnings, average pay, revaluation, and insurance periods. The main determinant of pension payments is the sum of individual accumulated earnings points. Some periods without contribution also count as insurance periods after the age of 17, such as years of further education, time spent in

[11] Taking the life-time perspective on savings and benefit streams, the theoretical model can easily be extended to focus on the substitutability between pension wealth and overall private savings. Under stricter assumptions concerning γ, it can be shown that the effect of pension wealth on overall private savings is ambiguous, as well.

[12] The tax rates are obtained from https://www.bmf-steuerrechner.de/ekst

military service, and time spent in raising children. The annual pension wealth of a worker who claims old age pension without financial adjustment and insured for $T^E - s$ years is the following:

$$PB_{gross} = \sum_{t=T^R}^{T} AR_t \times \sum_{\tau=s}^{T^E} \frac{w_\tau}{\bar{w}}$$

, where AR_t is aggregate pension base of year t, w is gross annual individual income τ , $\bar{w}$ is the average income of all insured people in the pension system. If we assume constant wage and take the mean of AR_t, the total pension wealth is

$$TotalPB_{gross} = (T - T^R)\frac{AR}{\bar{w}}(T^E - s) = pw(T^E - s)(T - T^R)$$

, where p is the gross pension replacement rate per year of the pension contribution. The interest portion of pension is subject to income tax. The taxable portion depends on retirement age. It is 27% if one retires at full retirement age 65. The taxable rate of pension is around 30%. Because the taxable portion of pension on average falls into the zero tax bracket, we assume that pension is not subject to income tax.