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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, which 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 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 allowed women born up until 1951 to retire early at age 60. After the reform, women born since 1952 onward cannot retire early and must wait at least until they are 63 years old. The reform effectively increases the ERA from 60 to 63 years, and is especially suitable to answer 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 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 regression discontinuity difference-in-differences (RD-DD) framework to wash out any unobserved correlations between birth year and savings behaviors. Our analyses show that households with women younger than

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

age 60 and who were born since 1952 adjust their savings rates downwards by approximately 0.6 percentage points due to the rising ERA. Furthermore, we investigate the mechanisms of the reduction in savings rates by investigating the response in terms of disposable income and consumption expenditures. We find that the treated households increase their consumption expenditures while maintaining an unchanged disposable income. Using the Survey of Health, Ageing and Retirement in Europe (SHARE), we show suggestive evidence of treated women increasing their expected retirement age. Our finding suggests 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. The drop in savings rate is driven by married households. We find a 1.5 percentage points reduction in savings rates due to the reform, while single women do not change their savings rate. 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. Households with homeownership are also more likely to reduce their savings rates. To validate concerns about causal identification, we provide results from several robustness checks.

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 labour 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 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 fine-grained microdata source with detailed household savings and expenditure information also allows us to go beyond labor supply changes and focus on savings and consumption expenditures 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 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 age at retirement. 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 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 the 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.

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 savings to smooth income fluctuations. Our paper shows evidence of adjustments of savings and expenditures 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 finding provides empirical evidence that households can achieve a smooth 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 and expected impacts. Data and the empirical setup are discussed in Sections 3 and 4. Section 5 describes the results, while Section 6 discusses the findings and concludes.

2. Institutional Background

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 longer contribution years or higher relative wage incomes will receive higher pension benefits.

The statutory retirement age for a regular old-age pension remained at 65 years of age throughout our sample period.[2] 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.[3] However, retirement before the NRA renders a 3.6% benefit deduction for each year of early claiming (see Engels et al. (2017a) 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, women could claim their pension earlier via the pathway of old-age pension for women. Before the 1999 pension reform, the old-age pension for women provided eligible women with an option to retire early at age 60. The eligibility requirements for this pathway were: first, at least 15 years of waiting periods;[4] and second, at least 10 years of contribution periods 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.

2.1. Abolishment of the Old-age Pension for Women Pathway

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. [6] While women born before 1952 have the option to claim the pension at age 60 via the women's pension, women born in and after 1952 no longer have this option. The only other possible

[2] 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.

[3] The four alternative pathways to retirement are old-age pensions for long-term insured, old-age pensions for women, old-age pensions due to unemployment (and, later, part-time work) and the invalidity pension. See (Börsch-Supan et al. 2004) for more details.

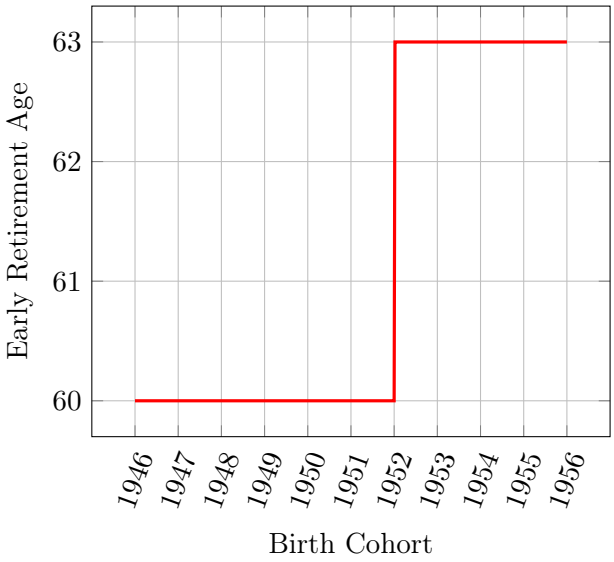
[4] Waiting periods are years of employment, unemployment, up to 10 years of child-rearing period and certain periods of education.

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

[6] Reform details can be found in the relevant law, *Rentenreformgesetz 1999* (RRG 1999), 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, and did not reverse the abolishment of the women's pension pathway. Therefore, the abolishment became effective in 1999.

way to leave the labor force before or at age 60 is via disability insurance due to severe health conditions.[7] 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.[8] 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. Thus, the reform has created a strong negative shock to pension wealth as it reduces the length of time that women can receive pension benefits. This sharp shift of the ERA between cohorts allows us to estimate the causal impact on private household savings using a regression discontinuity method.

Figure 1: Early Retirement Age for the Old-age Pension for Women Pathway



Notes: Figure 1 plots the statutory retirement age and the earliest possible retirement age for women as a function of their birth years.

The reform was enacted in 1999, and the first cohort affected by the reform was cohort 1952, who turned age 60 in 2012. Individuals are aware of the changes in future pension wealth and

[7] 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 contributions, 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.

[8] 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 for early claiming via the pension for the long-term insured. Alternatively, they can retire at the regular retirement age, which is 65 years and five months.

future labor earnings a decade before the realization of the income changes. Thus, they have considerable time to react to the forecastable income changes. Moreover, the reform is 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.

2.2. Expected Impacts

According to economic theory, individuals smooth their consumption over the life cycle. Income is only earned during the working ages; individuals save adequately to ensure that retirement does not lead to an abrupt drop in consumption. In modern welfare states such as Germany, individuals' retirement income mostly relies on the mandatory public old-age pension. Therefore, public pension reforms alter the incentives to save during an individual's working life.

The effect of raising the ERA on the savings rate is ambiguous and depends on the corresponding employment effect. The underlying mechanism is straightforward. As public pensions become less generous, private savings increase. However, most pension reforms also impact labor supply decisions. If the insured reacts to reducing pension wealth by working longer, the effect on savings behavior is unclear. Because a prolonged working life incurs higher future labor earnings, which may offset the forgone pension benefit and increase lifetime earnings, individuals can save less. However, if individuals do not expect to prolong their working lives, they may increase their private savings to cushion the loss in pension wealth. The heterogeneity in employment responses can result in different savings consequences. Therefore, the effect of an increase in the ERA on the savings rate is an empirical question. Whether the increase in expected future labor earnings is large enough to reduce the savings rate will be tested in the data.

On the one hand, we anticipate a lifetime income decline due to the loss of three years of pension benefits. On the other hand, the longer working horizon is very salient: individuals expect to offset the negative shock as they forecast to have more years of labor earnings, obtain higher monthly pension benefits due to more contributions, and have a smaller financial penalty due to the delay in claiming. Further, they have a shorter retirement duration. Therefore, we expect to see that the reform has a small or even negative impact on the savings rate.

3. Data

3.1. Data and Sample

The main sample is from the German Income and Consumption Survey (*Erwerbs- und Verbrauchsstichprobe*, EVS).[9] 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. 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 members younger than age 60 to avoid capturing the drop in consumption and savings around the retirement age. Moreover, we drop women older than age 60 to ensure that pension wealth changes are not materialized because claiming an old-age pension before age 60 is almost impossible.[10] 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 expenditures and savings, computed from diaries filled out by the household over the course of at least three months. Therefore, consumption and savings measures are precise and detailed. The EVS has three features that make it well-suited for our analysis: first, it is the only available richly detailed microdata source for household savings and consumption information in Germany. 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 subcategories of savings and consumption expenditures. 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.

3.2. 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).

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

[10] 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.

Table 1: Summary statistics

	Reform waves (2003, 2008)		Control waves (1993, 1998)	
	Born since 1952	Born before 1952	Born since 1952	Born 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	422.35	437.27	537.97	573.40
	(268.07)	(282.70)	(309.85)	(318.65)
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.

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). The savings, income and consumption expenditures are 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.[11] 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.

[11] 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).

Besides, the 1993 wave has a slightly different way of categorizing expenditures and savings. We, therefore, control for wave fixed effect in our regression analysis.

The main outcome variable is the household savings rate, which is defined as monthly household net savings divided by the monthly net disposable income. In our sample, households save on average 433 euros per month in the control waves with a savings rate of 13%, and 239 euros per month in the reform waves with a savings rate of 11%. We also look at three categories of savings rates by types of savings vehicles. They are the monetary savings rate (deposits to bank accounts, buying stocks), the property savings rate (buying gold, houses, etc.) and the loan payback rate (mortgage and interest payments or the redemption of credits, etc.). We find a savings rate for monetary values of 6% , a 3% savings rate for property values and a 2% savings rate for loan payback in the reform waves.[12]

We further check several subcategories of household consumption: basic consumption, leisure consumption, and the probability of owning private pension insurance. We define basic consumption as expenditure on clothes, food at home, education, rent, public transportation, etc. Leisure consumption includes spending on leisure activities, such as attending concerts, taking up hobbies, buying sports equipment, and holiday accommodation costs. In our sample, households spend on average 1600 euros per month in the reform waves, and 2000 euros per month in the control waves.

4. 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 expenditures. 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.

4.1. Regression Discontinuity Design

The estimation equation for RD design is the following:

$$Y_i = \alpha + \beta X_i + \gamma D_i + \delta_l f_l(S_i - c) + \delta_r D_i * f_r(S_i - c) + \epsilon_i \quad (1)$$

[12] 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%.

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 rate 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, home-ownership, 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. [13] 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 handle this problem, 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 the savings rate. 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 $T = 0, 1$, equal to one for waves after 1999 and zero otherwise, interacted with Equation (1):

$$Y_{it} = \alpha + \sum_{\tau=0}^{1} \mathbb{1}[Post_{it} = \tau] \times \{\gamma_\tau D_i + \delta_{l\tau} f_l(S_i - c) + \delta_{r\tau} * D_i f_r(S_i - c) + \theta Post_{it}\} + \tau_t + \beta X_{it} + \epsilon_{it} \tag{2}$$

γ_1 estimates the discontinuity in savings rate 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.

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

4.2. RD Assumptions

Smoothness in density: For an 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 [14]. 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 jump 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 the partner, home-ownership, 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 not 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.

5. Main 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. Because married women may rely more on their partners' income, savings responses may differ somewhat by marital status. We therefore present all effects for the full sample and the subgroups of couple households and single women.[15]

5.1. Savings Responses

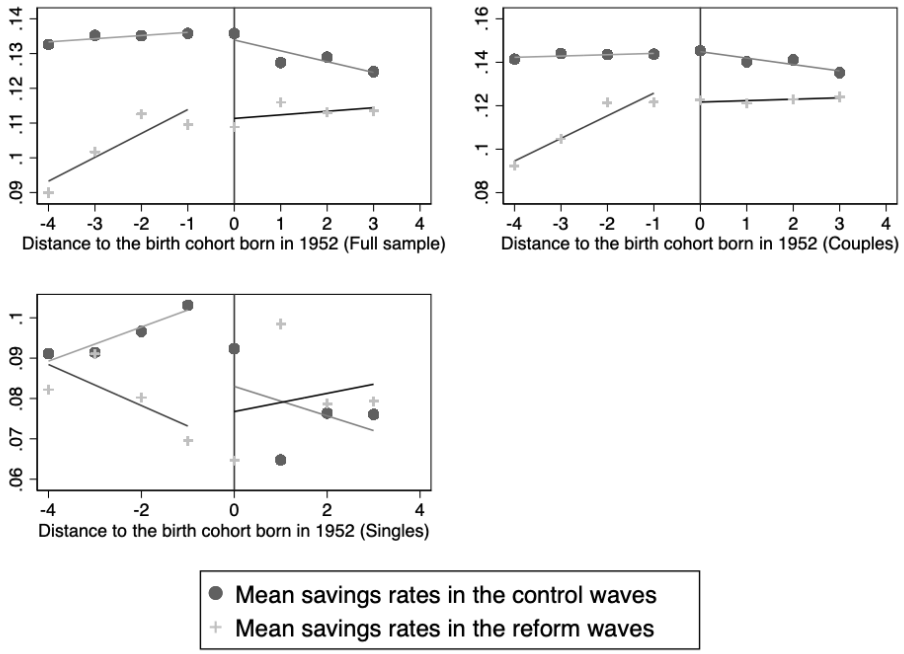
Figure 2 presents some graphic evidence on the relationship between birth year and savings rates in reform years (grey crosses) and in non-reform years (dark dots), for the full sample (upper left panel), couples (upper right panel) and singles (lower left panel). Overall, we notice that the savings rates are higher in the non-reform years. This is reasonable because in the non-reform years the sample is around 44 years old when the savings rate peaked in the life-cycle. We find a

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

[15] 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 TableA.2.

smooth linear relationship between birth cohort and savings rates in the non-reform years for both the full sample and the couples, while in the reform years, we see an upward trend before the cut-off, which breaks at the cut-off. For singles (lower left panel), the relationships are mixed. 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.

Figure 2: Savings rates by marital status in reform and control years



Notes: Figure 2 presents some graphic evidence on the relationship between birth year and savings rates in reform year (grey crosses) and in non-reform years (dark dots) for the full sample, married households and single households.

The first two columns of Table 2 reports our basic RD estimates of γ from estimating Equation 1 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 2 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 rate by 10 percentage points 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 there were 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 in non-reform years into account in column 3. The effect is reduced to a more moderate 6 percentage points reduction in

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

Panel 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 rate by 15 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.

Table 2: Effects on household savings rates

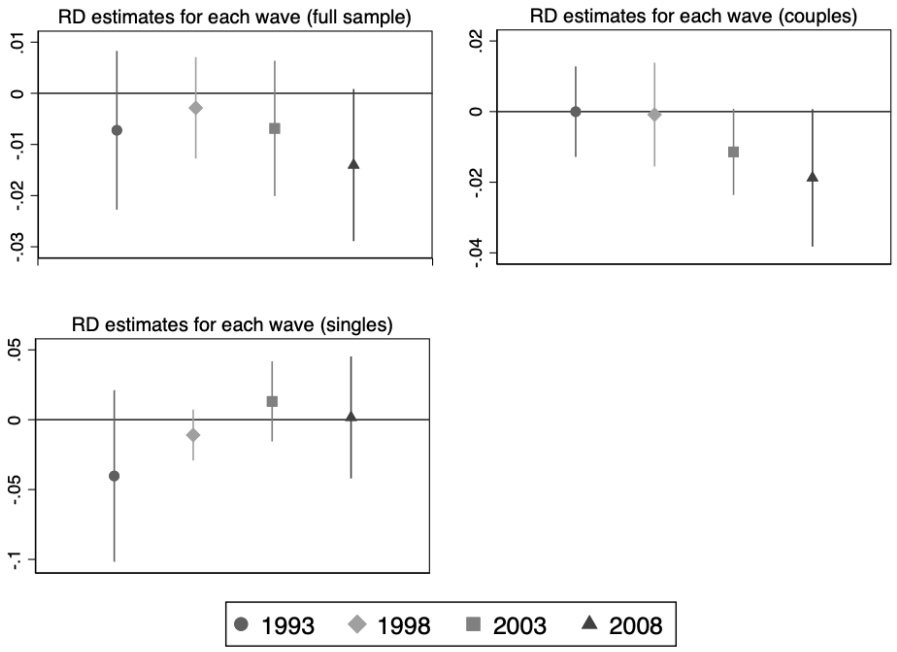
	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-0.010*	-0.005*	
	(0.005)	(0.002)	
Born after 1951=1 × post=1			-0.006
			(0.006)
Observations	11,239	13,604	24,843
R^2	0.019	0.017	0.022
Dependent Variable Mean	0.109	0.132	0.121
		Couples	
Born after 1951	-0.015**	-0.000	
	(0.006)	(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.

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

3. 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.

Figure 3: Wave-by-wave point estimates- savings rates



Notes: Figure 3 shows the RD estimates for each wave of EVS (1993, 1998, 2003 and 2013) for three groups (full sample, married and single households).

We also investigate the reform effect on equivalized individual savings level in Table A.3. We find the treated households reduce their savings by 75 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 108 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.

The impact of marital status on the savings rate is theoretically ambiguous. First, married women tend to match their retirement timing to that of their partner, who is in 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 A.11 (Section 5.4.1) show that married women increase their expected retirement age more than single women. Thus, we expect married household 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 to retire earlier. [16] 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. Thus, the increase in ERA has no impact on single women. [17] We find that the couples dissave while singles are not responsive to the reform. This results infers that couples expect to have more labor income while singles do not have the same expectation.

5.2. Heterogeneous Effects

Besides marital status, we further look at the heterogeneous responses for subgroups by education attainment and household ownership. Table 3 shows the estimation results. We find that households consisting of highly-educated women reduce their savings rate by 0.24 percentage points, which drives the overall impact. This is pertinent for two 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, because we do not know the pension contribution history in the EVS, we cannot calculate the group-specific eligibility rates. It is worth noting that the heterogeneous savings response could also be driven by the difference in eligibility shares across groups.

We also investigate the heterogeneous effects of home ownership. On the one hand, we expect households with more assets can better buffer the reform shock. They can still afford to exit at age 60 and finance the gap between 60 and 63 from their housing assets. However, because the housing asset is relatively illiquid, we expect that the buffer stock impact is small. On the other

[16] Geyer et al. (2020) explore the realized employment responses by marital status. They find that married women tend to go into inactivity, while singles rely more on other social welfare programs such as unemployment insurance.

[17] Geyer et al. (2020) finds that the retirement effects are slightly higher for women in couples, while single women show a slightly higher probability to stay in the labor force.

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
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Table 3 shows the heterogeneous responses for subgroups by education attainment and household ownership.

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 compliers of the reform; that is, the reform would not affect their expected future labor earnings. Therefore, they won't update their savings plan. The overall effect is an empirical question. We find 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.

5.3. Robustness Checks and Placebo Tests

Several exercises further establish the robustness of the estimates. We test the robustness of the estimation results by varying the choice of controls, bandwidth, and polynomial orders.

Tables A.4 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.5 shows results by various bandwidths.

The impacts are stable with three and four years of bandwidth. However, when reducing the bandwidth to five years, the effect becomes insignificant and changes sign. 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.6) and with a quadratic cohort trend (Table A.7). The estimates are not sensitive to quadratic age controls. However, introducing a quadratic cohort trends makes the estimates insignificant. We find close to zero and much smaller insignificant negative impact for couples with quadratic cohort trend. We believe that given that we have so few numbers of bins around the cut-offs, it may be a stretch to introduce quadratic cohort trends.

The identification of the RD design relies on the assumption of the local randomization of the running variable around the reform cut-off. Because the running variable is the birth cohort, we are not concerned about manipulating the treatment status. However, because we only observe the birth information at a yearly level, one potential concern is that the observed discontinuous drop in savings rate at the cut-off may be driven by some structural break in savings rate at the cutoff age (age 51 in 2003 , age 56 in 2008). Therefore, we run a placebo test by using samples of older cohorts with the same age composition as our sample in the reform years. We compare the RD estimate obtained by the baseline sample (cohorts 1948-1955) in 2003 with the placebo estimate by using a pooled sample 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 and the same age cut-off at 51. Panel 1 of TableA.8 shows the effects using the pooled placebo sample for the full sample, couples, and singles. Panels 2 and 3 of Table A.8 display the RD estimates by using older cohorts in 1993 (cohorts 1938-1945) only and by using older cohorts in 1998 (cohorts 1943-1950) only. 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 rate between cohorts 1951 and 1952 in 2003 is not driven by structural differences in savings rates along the age dimension.

We do the same to test for robustness of 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 age cut-off age (younger than age 57). Table A.9 measures the discontinuous change in savings rate 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[18]. This can mean the households with women younger than 57 in 1998 are less likely to leave the labor force, and 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.

Furthermore, we show the RD and the RD-DD estimates obtained using cohort 1950, 1953, and 1954 as the placebo cut-offs in Table A.10. 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 too concerned that the estimated reduction in the baseline analysis's savings rate is spurious. Yet, we do find a similar sizeable negative impact for couples and positive effects for singles at the 1953 cut-off, which we believe is because 1953 is too close to the actual cut-off. Combined with the fact that we only observe birthdate at the yearly level, it is not too surprising to find similar impacts at the 1953 cut-off.

5.4. Other Responses

In this subsection, we examine the the responses in three dimensions in order to better understand the savings rate responses. First, we show changes in 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 expenditures. Last, as some specific subcategories of savings may drive the savings rate reduction, we show the impact for three outcomes: the monetary savings rate, the property savings rate, and the loan payment rate. Because the information on subcategories of expenditures 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 not focus too much on the magnitude of the estimates but rather the signs.

5.4.1. 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

[18] 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. (2017b) for the labor supply impact of this reform.

behavior run through the channel of changes in expectations toward the individual retirement age, retirement benefits and future labor earnings, however, we have no expectation information in the main dataset (EVS). 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).[19] 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), wave 6 (2015) and wave 7 (2017).[20] We drop individuals older than age 60 at the survey year and only look at Germany and cohorts born between 1948 and 1955. The outcome variable of interest is the age at which women expect to collect pension benefits. We find that women born before 1952 show an expected retirement age of 62.37, and 34% of women in the sample state an expected retirement age lower than 63. In the group of women born from 1952 onward, we find an expected mean retirement age of 63.21, and only 12% of the women in the group state an expected retirement age lower than 63. The difference in expected retirement age is significant. The results provide some suggestive evidence that the reform alters individuals' expected age of retirement (see Table A.11 for results). We also find that married women increase their expectation of retirement age more than single women. This is consistent with our finding that married households reduce their savings rates.

5.4.2. Disposable Income and Consumption Expenditure Responses

We present the RD effects from Equation 1 using the reform waves in this section. Table A.12 depicts small insignificant effects on equivalized disposable income for the full sample and couples, while singles show a positive but insignificant impact. Therefore, the change in the savings rate is not due to a change in disposable income. Rather, it is in line with Geyer and Welteke (2019), who show no anticipated labor market responses to the reform.

We then highlight the consumption expenditure responses in Table A.13. Row 1 shows that, for couples, the monthly equivalized consumption expenditure of married households increases. Further, we investigate subcategories of consumption expenditures, 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 are expenditures

[19] see DOIs: 10.6103/SHARE.w1.700, 10.6103/SHARE.w2.700, 10.6103/SHARE.w4.700, 10.6103/SHARE.w5.700, 10.6103/SHARE.w6.700, 10.6103/SHARE.w7.700 for information on the SHARE data. See Börsch-Supan and Jürges (2005) and Börsch-Supan et al. (2013) for methodological information.

[20] We don not use wave 3 because it's a retrospective survey and has a different structure as the other waves.

on activities such as attending concerts, purchasing sports equipment and spending on hotel accommodation.

5.4.3. Subcategories of Savings Rates Responses

Furthermore, we investigate three subcategories of savings in Table A.14. 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 the savings rate in monetary assets by around 18 to 20 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 rates, 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 rate.

6. 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 the households' savings rates and consumption expenditures. 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 their retirement ages. These households adjust their savings and expenditures accordingly. We show that groups whose

expected retirement age is more affected by the reform (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 expenditures 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 in couples 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 middle-ages. Later, when they reach age 61 and 62, they may not be able to prolong their employment due to unexpected constraints. They may regret to 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. However, this discussion is beyond the scope of this paper.

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Online Appendix

A. Appendix Tables

List of Tables

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

Survey wave Birth year	1993	1998	2003	2008	Total 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 members	0.018	0.018	-0.003	-0.021	12765
	(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 the husband	-0.265	-0.265	-0.247	-0.348	9714
	(0.187)	(0.187)	(0.187)	(0.378)	

Notes: Standard errors in the parentheses. * p<0.10, ** p<0.05, *** p<0.01. 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: Effects on the equivalized individual savings level

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-55.701***	15.175	
	(14.465)	(26.029)	
Born after 1951=1 × post=1			-74.714*
			(34.040)
Dependent Variable Mean	228.136	433.777	339.189
Observations	12,537	14,815	27,352
R^2	0.007	0.018	0.020
		Couples	
Born after 1951	-75.568***	26.727	
	(12.443)	(21.822)	
Born after 1951=1 × post=1			-108.035***
			(23.118)
Observations	9,766	12,219	21,985
R^2	0.006	0.012	0.017
Dependent Variable Mean	251.901	475.079	375.693
		Singles	
Born after 1951	18.480	-52.620	
	(68.879)	(57.927)	
Born after 1951=1 × post=1			73.469
			(114.934)
Observations	2,771	2,596	5,367
R^2	0.006	0.012	0.009
Dependent Variable Mean	145.871	241.582	191.903
Cluster at birth cohort	✓	✓	✓
Year fixed effects	✓	✓	✓
Further control variables	✓	✓	✓

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Table A.3 shows estimated changes in equivalized individual monthly savings amount from a linear specification with a four-year bandwidth.

Table A.4: 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<0.01. Table A.4 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.5: 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<0.01. Table A.5 show the RD-DD estimator by various bandwidth choices.

Table A.6: 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<0.01. Table A.6 show the RD-DD estimator with a quadratic age trend.

Table A.7: 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<0.01. Table A.7 show the RD-DD estimator with a quadratic cohort trend.

Table A.8: Effects on households' savings rates using the placebo sample for wave 2003

	Full sample	Couples	Singles
Younger than 52 in non-reform waves	0.000	0.000	0.003
	(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<0.01. Table A.8 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.9: Effects on households savings rates using placebo sample for wave 2008

	Full sample	Couples	Singles
Younger than 57 in non-reform waves	-0.008**	-0.007	-0.012
	(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.9 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.10: Effects on household savings rates at the 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<0.01. Table A.10 shows the RD-DD estimates at placebo cutoffs.

Table A.11: Expectations of retirement age in the SHARE data

	Mean of expected retirement age		Difference	
	born before 1952	born since 1952	without controls	with controls
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

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Table A.11 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 1,2,4,5,6,7.

Table A.12: Effects on households' 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<0.01. Table A.12 shows the estimated changes in the equivalized monthly disposable income using the RD method in the reform waves.

Table A.13: 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

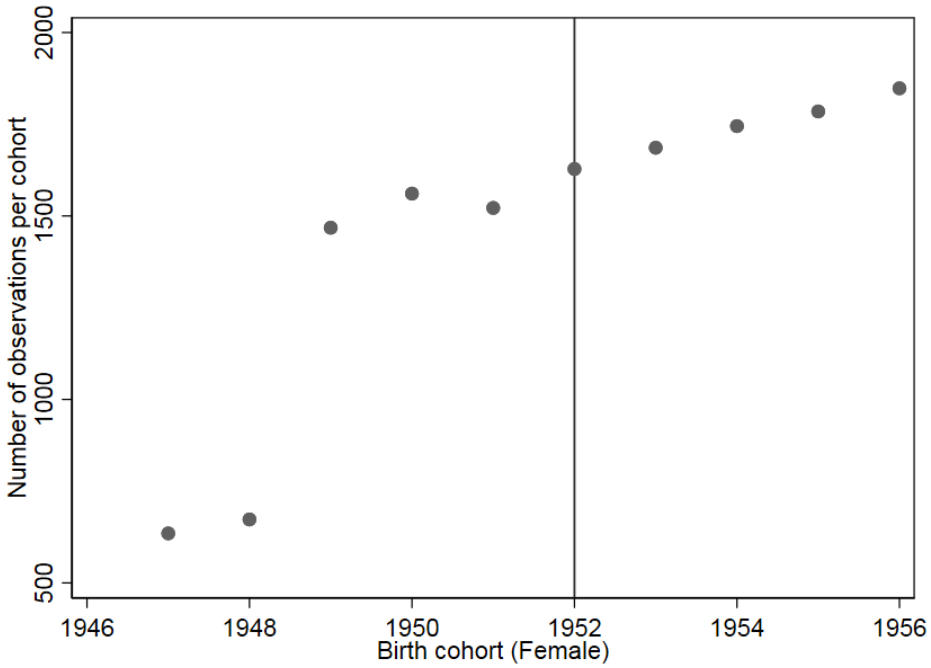
Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.13 shows the estimated changes in the equivalized monthly consumption expenditure using the RD method in the reform waves.

Table A.14: 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<0.01. Table A.14 shows the estimated changes in the subcategories of savings rates using the RD method in the reform waves.

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



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