Discussion Paper Series – CRC TR 224

Discussion Paper No. 255
Project C 01

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

Stefan Etgeton [1]
Björn Fischer [2]
Han Ye [3]

November 2023
(Second version: January 2022)
(First Version: January 2021)

[1] DIW Berlin
[2] ZEW Mannheim
[3] University of Mannheim, IZA, ZEW

Support by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) through CRC TR 224 is gratefully acknowledged.

Collaborative Research Center Transregio 224 - www.crctr224.de
Rheinische Friedrich-Wilhelms-Universität Bonn - Universität Mannheim

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

Stefan Etgeton
DIW Berlin

Björn Fischer
ZEW Mannheim

Han Ye
University of Mannheim,
IZA, ZEW

First Draft: November 2019
Current version: February 2023

Click here for the latest version

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. First, we present suggestive evidence that women update their retirement planning in response to the reform. Using the German Income and Consumption Survey, we find a negative impact on private savings of 0.6 percentage points that is driven by households with married women. 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 households anticipate experiencing a lifetime income increase and reduce their savings rate to smooth consumption.

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

* We would like to thank the editor and three anonymous referees for their comments, which greatly improved the paper. We are grateful to Christian Dustmann, Johannes Geyer, Hans-Martin von Gaudecker, Peter Haan, Eckhard Janeba, Michèle Tertilt, Nicolas Ziebarth and participants at seminars in DIW Berlin, University of Mannheim, CReAM, EPSE 2019, EALE 2019 and University of South Florida for their helpful insights and suggestions. The views expressed here are solely those of the authors. The authors acknowledge funding from the Forschungsnetzwerk Alterssicherung of the German Statutory Pension Scheme and the German Federal Ministry of Labor and Social Affairs within the BMAS-FIS network "InterdisciplinarySocial Policy Research". Funding by the German Research Foundation (DFG) through CRC TR 224 (Project C01) is also gratefully acknowledged. All errors are our own.

Corresponding authors: Björn Fischer: bjoern.fischer@zew.de, Han Ye: han.ye@uni-mannheim.de

1. Introduction

Due to the aging 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., Coile and Gruber 2004; Krueger and Pischke 1992; Manoli and Weber 2016; Staubli and Zweimüller 2013), 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. The aim of the policy maker to prolong working lives is then in conflict with the aim of increasing household's private retirement savings. 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 could still retire at age 60 via the old-age pension for women. The reform effectively increased 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 the ERA based on birth dates allows us to credibly identify causal effects. Second, in contrast to reforms investigated in other empirical studies on the displacement effects of public pension wealth on private savings (Attanasio and Brugiavini 2003; Attanasio and Rohwedder 2003; Delavande and Rohwedder 2017; Feng et al. 2011; Lachowska and Myck 2018), the abolishment of the old-age pension for women has a relatively large and salient effect on labor supply, hence on lifetime labor earnings.[1] Using the Survey of Health, Aging and Retirement in Europe (SHARE), we show suggestive evidence of treated women increasing their expected retirement age. Married women react more than single women. We argue that the size in the reform induced adjustments in retirement planning leads to an overall increase in lifetime income. This feature allows us to show direct evidence of dissaving when the adjustment in labor supply absorbs the loss in pension wealth.

[1] Geyer and Welteke (2021) find a sizable increase in retirement age and substantial positive employment effects of this reform.

To empirically address this question, we use four waves of repeated cross-sectional data: The German Income and Consumption Survey (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 cut-off using the post-reform waves (2003 and 2008). Subsequently, we use the pre-reform waves (1993 and 1998) in a 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 (pp) due to the rising ERA.

We analyze effects by household composition and other socio-economic variables. We find that the drop in savings rates is driven by married women, who experience a 1.5 pp reduction in savings rates due to the reform. We do not find robust effects for the group of single women. Moreover, we find suggestive evidence 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. Our results show that households with homeownership are significantly more likely to reduce their savings rates. This suggests that income security matters. We further examine how joint retirement plays a role in couples' responses. We find suggestive evidence 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 studying the response in terms of disposable income and consumption expenditure. We find that the treated households increase their consumption expenditure while maintaining an unchanged disposable income. Our findings imply that the expected increase in future labor earnings offsets the reform-induced anticipated loss of forgone pension benefits. 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 order, and by using an alternative empirical method. We also establish the causality of our estimates by performing several 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 cut-offs.

We discuss the size of the main estimates along two dimensions: Using a micro-simulation exercise we study the impact of the reform on pre- and post-retirement available funds[2] in two scenarios. In scenario one married women only update their labor supply while in scenario two married women also update their savings according to the estimates. The exercise shows that women in couples understand that the reform-driven employment effect increases their overall lifetime income and use savings as a vehicle to redistribute the overall increase in lifetime income slightly toward pre-retirement periods in comparison to the scenario in which they only update their labor supply. We then show that while our results are difficult to compare to the existing literature, they are generally in line with other papers. For example, we compare our results with the literature under the marginal propensity to consume (MPC) framework. Our MPC of 0.14 implies that for an additional euro of lifetime wealth, a household consumes an additional 14 cents. The MPC of our estimate is similar to those found in consumption smoothing studies of labor income variations.

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 (Cribb et al. 2016; Eibich 2015; Fischer and Müller 2020; Geyer and Welteke 2021; Geyer et al. 2020; Lalive and Parrotta 2017; Manoli and Weber 2016; Mastrobuoni 2009; Staubli and Zweimüller 2013) and the labor supply and health behavior response of middle-aged individuals (Bertoni et al. 2018; Carta and De Philippis 2019; De Grip et al. 2013; Hairault et al. 2010). However, very few studies investigate the impact of raising the 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 that whether the public pension benefits crowd out private savings depends 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

[2] In this context "available funds" are defined as after-tax income minus savings in pre-retirement periods and pension-benefits plus average dissaving in post-retirement periods.

replacement rate (Attanasio and Brugiavini 2003; Attanasio and Rohwedder 2003; Delavande and Rohwedder 2017; Feng et al. 2011). 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 the 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 the retirement age. They find a sizable 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 the 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.

Closest to our paper are Lindeboom and Montizaan (2020) 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.[3]

Last, our paper relates to studies on the consumption response to anticipated permanent income changes (Hsieh 2003; also see Attanasio and Weber 2010 andJappelli 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.

[3] Note that Lindeboom and Montizaan (2020) find that the highly educated can buffer the rise in the ERA 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% 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 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 discuss the possible impacts of the abolishment of women's pension on households savings rates. Data and the empirical setup are discussed in Sections 4 and 5. Section 6 describes the results, before we discuss the findings in Section 7 and conclude in Section 8.

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 labor income. Aside from a few exceptions, workers with more contribution years or higher relative labor income 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.[4] The only prerequisite for 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.[5] 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 considered to be actuarial fair (Börsch-Supan et al. 2004). Consequently, 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 called '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 have been acquired after age 40.[6] According to Geyer and Welteke (2021), 60% of women born in 1951 were eligible for women's pension.

[4] 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.

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

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

The old-age pension for women was an important pathway for women born until 1951 to retire. Among women born between 1948 and 1951, 35.38% retired 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 had more than two children.[7]

Abolishment of the Old-age Pension for Women The 1999 reform eliminated 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.[8] Prior to the reform, women born before 1952 had the option to claim a pension at age 60 via the women's pension, while women born in and 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 after the reform is via disability insurance due to severe health conditions.[9] Otherwise, the earliest possible age to claim a pension is at age 63, via the old-age pension for the long-term insured. 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 2021). Workers who are not eligible for the long-term insured pension can claim the regular old-age pension.[10] Women born in 1951 could 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.

Women eligible for the women's pension faced a sharp increase in their distance to retirement benefit eligibility. The ERA effectively increased from age 60 to age 63 for the impacted cohorts. Indeed, Geyer and Welteke (2021) find that the reform increased the employment rates of 60-62-year-old women by 13.5 pp, which amounts to about a 30% increase compared to the

[7] 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.

[8] 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 Section C for a more in-depth discussion.

[9] 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.

[10] See Appendix Section B.2 for more details on different retirement pathways.

pre-reform mean. It also shows that the increase in the employment rate was mainly due to women remaining longer in their current jobs.[11] 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.

3. Savings Responses to the Reform

In this section, we discuss possible (heterogeneous) savings rate responses of households to the abolishment of women's pension. Theoretically, a pension reform impacts savings decisions via two channels: a decrease in pension wealth can induce higher savings rates as agents might want to smooth consumption over their life-cycle. Simultaneously, the reform can positively impact labor supply and delay retirement entry and employment exit. Ceteris paribus this reduces time spent in retirement and increases labor income. Higher labor earnings and pension benefits due to delayed claiming might outweigh forgone periods of pension benefits such that lifetime income might increase as a result. Agents might react to this by decreasing their savings rate while they are still employed. Which of the two channels will prevail is ultimately an empirical question and depends on the size of the anticipated employment effect of the reform (Feldstein 1974).

To understand the potential impacts of the reform on lifetime income by labor supply choices we depict four scenarios. In Figure 1, we compare the discounted lifetime income of otherwise identical women born on different sides of the reform cut-off. It illustrates four potential reactions to the reform in discounted lifetime income depending on the reform-induced change in retirement timing and labor market exit age. The reference (baseline) is the discounted lifetime income for a stylized woman who would retire and claim a pension at age 60. Her discounted lifetime income is the reference and is set to 100. On the right side of the vertical birth-year cut-off line,

[11] Geyer and Welteke (2021) also look at the reform impacts on the unemployment rate and disability pension participation rates. They find a small increase in the fraction of women who were unemployed, but no program substitution 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.

we depict the discounted lifetime income of four different scenarios of employment and pension claiming choices for women born since 1952 (treated cohorts). After the reform, the earliest possible claiming age is 63; however, treated women can exit the labor market earlier. Figure 1 shows four scenarios in which the employment exit ages are 60, 61, 62, and 63, respectively. The pension claiming age is always at 63 years.[12]

As argued above, heterogeneity in labor supply response to the reform can result in different lifetime income effects and thus lead to different savings consequences. Figure 1 illustrates how the magnitude of the lifetime income effect differs by labor supply response to the reform. In all scenarios a woman that had retired at age 60 before the reform foregoes at least three years of pension benefits through the reform. In scenarios where a treated woman works at least until age 61, her lifetime income increases due to delaying pension claim, which increases monthly pension benefits via a smaller financial penalty, more contributions, and a shorter retirement duration. In combination with additional future labor earnings, these factors may offset the forgone pension benefit due to later claiming, and thus increase her lifetime income. This extra income will be spread evenly over the lifetime, leading to fewer savings during the periods before retirement. If the reform does not impact her employment decision, she still retires at age 60 while she claims a pension at age 63. We show that in this case, she loses lifetime income, as compared to when she stops working and claims pension simultaneously at age 60.

How large is the impact of abolishing women's pension on optimal retirement timing and employment exit? Figure 2 illustrates the stylized pension wealth for individuals who face an ERA of 60 (dashed black line) and an ERA of 63 (solid blue line).[13] The non-linear relationship between pension wealth and retirement age indicates strong financial incentives to retire at the ERA. Hence, theoretically, when the ERA increases by three years, the retirement age will shift accordingly. The induced labor supply response in such a setting is predicted to be larger than those induced by other reforms, such as changes in pension replacement rates or increased financial penalties for claiming pensions early. Importantly, abolishing women's pension could induce a large enough labor supply response, which could outweigh the reform-induced losses in pension wealth. Indeed, Geyer and Welteke (2021) and Geyer et al. (2020) show that the reform leads to a sizeable increase in women's labor supply in the ages 60-62.

In this study, we are interested in reform-induced household savings responses at ages before retirement (before age 60). Therefore, the reform effects on savings rates depend on the difference in retirement planning between the cohorts (women born before 1952 vs. women born since 1952). Individual expectations toward the retirement age play a key role. Previous literature has

[12] Appendix Section E.2 describes the details of how we calculated the discounted lifetime income.
[13] See Appendix E.1 for detailed steps to obtain the illustrated pension wealth.

shown the importance of expectations in decisions related to pension and retirement planning (Bissonnette and Van Soest 2015; Bottazzi et al. 2006; Ciani et al. 2023). As argued above, the reform and its consequences for retirement timing are very salient. If the cohorts internalize the reform's incentives and plan to delay retirement by a large enough size, we hypothesize a decrease in savings rates of affected households.

To better understand the magnitude of the changes in the expected retirement age, we explore information from the German respondents to the Survey of Health Aging and Retirement in Europe (SHARE).[14] We investigate the impacts of the reform on self-reported retirement planning. Figure 3 shows an overlaid histogram of expected retirement ages for cohorts born before and since 1952. We see a clear shift in the expected retirement age from 60 to later ages for the treated cohorts. Table 1 compares the expected retirement ages for women born around the cut-off. Columns one and two show the sample means of expected retirement ages for women born before and since 1952. Columns three and four show the estimated treatment effect from a simple first-difference OLS regression with and without controls (age, East Germany, and education).[15] 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 and close to one year. The results provide suggestive evidence that the reform increases women's expected retirement age and could possibly induce a reduction in savings.

Variations in employment responses can result in heterogeneous savings consequences. The differences in expected employment responses may occur due to fulfillment of eligibility criteria, knowledge of the institutional setting, and other factors impacting retirement timing. One important factor influencing retirement timing is marital status. Marital status is linked to several aspects: women's pension eligibility, being a complier, and the role partner's income plays. All these aspects affect their labor supply response to the reform, thus saving responses.

Theoretically, the heterogeneous labor supply response by marital status 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 the ERA. Second, married women may rely more on their partners' income and can therefore afford not to update 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. At the same time singles rely more on other social welfare programs, such as unemployment insurance[16], hence, they do not expect to

[14] See Section 4.3 for a description of the SHARE data and sample construction.
[15] We only control for age and East Germany when showing heterogeneity by education attainment.
[16] Using household-level information from the German Census data, Geyer et al. (2020) find that the employment rate between age 60 to 62 increases by 8.72 pp for women in couple households, while the employment rate

prolong their working life and experience a larger decline in their lifetime income. Last, single women are less likely to be the compliers of the reform. Table A.3 shows that 85% of women who claimed women's pension are married and Table A.4 shows that even though among single women the probability to be eligible for women's pension is higher, the probability to claim women's pension is higher among married women. Without additional income from a partner, single women are unlikely to use the early retirement option in the absence of the reform.

Table 1 shows that in SHARE data married women report a 1.5 year higher expected retirement age due to the reform while single women show smaller impacts on their expected retirement age. This would mean that married women should also react more regarding savings than single women. Due to the fundamental differences between couples and single women, we perform the main analysis separately by marital status.

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.[17]

4.1. Main Data and Sample

The main sample is from the German Income and Consumption Survey (*Erwerbs- und Verbrauchsstichprobe*, EVS).[18] 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.[19] 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

increases by 7.45 pp 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 ages 60 and 62 for women in couple households.

[17] See Section 3 above.

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

[19] 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.

60 is almost impossible.[20] 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 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).[21] 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 contains 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 2 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 one and two) and control waves (columns three and four). 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

[20] 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.

[21] 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.

assign the outcome equally to all household members.[22] All monetary variables are adjusted to 2003 euro values. Table 2 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 save on average €433 per month in the control waves (a savings rate of 13%), and €239 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.[23]

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 €1600 per month in the reform waves, and €2000 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 and 2005), wave 2 (2006 and 2007), wave 4 (2011 and 2012), wave 5 (2013) and wave 6 (2015).[24] We

[22] We use the OECD equivalence scale, which assigns a weight of one 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).

[23] 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%.

[24] 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.

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 the ERA on monthly savings rates and consumption 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:

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

The running variable S_i is defined as the birth cohort. The reform cut-off 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 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 cut-off to remove the age effect.[25] 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 DD model using non-reform years to reveal and

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

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 use waves 1993, 1998, 2003 and 2008 and specify a "reform year" indicator $Post_{it} = 0, 1$, equal to one for waves after 1999 and zero otherwise, which we interact 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 rates for cohorts born before and in 1951 and just after 1951 conditional on any secular cohort trend 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.[26] 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 2, reform waves) for a set of covariates (age, age gap with the partner, homeownership, East German, household size, German citizens, share of married women, share of widowed or divorced women and higher education) at the cut-off under different specifications: with a linear cohort trend (column one, with a linear age trend (column two, with a quadratic age trend (column three) and a quadratic cohort trend (column four). We find significant zero differences between the treated and untreated 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.

[26] Geyer and Welteke (2021) provide detailed evidence that the RD identifying assumptions are satisfied.

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 family types can have important influences on household labor supply and financial decisions[27], we present all effects for the full sample and the subgroups of couple households and single women.[28] Moreover, we examine other reform responses, including disposable income, and consumption expenditure responses to better understand the savings rate 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 1 using control wave observations. This compensates for 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% 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 cut-off instead. Figure A.3 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 we observe 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 3 report our basic RD estimates of γ from estimating Equation 1 in the reform waves (column one) and the control waves (column two)). Column three 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 one suggests that the treated cohorts reduce their household savings rates by 1 pp in the reform waves, corresponding to a reduction of around 9%. 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

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

[28] 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.

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 three. The effect is reduced to a more moderate 0.6 pp reduction in the savings rates. The RD-DD point estimate is, however, not significantly different from zero for the full sample.

Panels 2 and 3 of Table 3 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 savings rates. The treated married households reduce their savings rates by 1.5 pp in the reform waves, which corresponds to a reduction of around 13%. 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.

To capture the dynamics of savings rates updating 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 largest in 2008 for the full sample and couples. There are two potential explanations: first, 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, the reform was announced in 1999. It may take longer than four years for households to internalize the changed incentives. 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 3. However, due to the smaller sample size, we cannot interpret the pattern.

We also investigate the reform effect on the equivalized individual savings level in Table A.6.[29] We find that the treated households reduce their savings by €90 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, 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 difference in savings responses to the reform by marital status can be partly explained by heterogeneous retirement planning responses. While married women born since 1952 report significantly higher expected retirement ages, the reform does not lead to a significant update in singles retirement planning (see Section 3). However, the fewer observations of single women lead to uncertainty in response sizes. This is also represented in a much higher relative standard

[29] Table A.6 only uses 1998 as the control wave. Because the 1993 wave of EVS is very differently constructed, the measurements of savings levels 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 most of the inconsistent accounting.

deviation in the savings rate for single households (mean of 0.08 and standard deviation of 0.16). Couple households have a mean of 0.13 and a standard deviation of 0.16. Therefore, we cannot conclude a zero savings response of singles to the reform. Furthermore, there is evident heterogeneity within the group of single women. Divorced or widowed women might substantially differ from never-married women in savings behavior and retirement planning. In our sample of single women, 38% are never married, and 62% are divorced or widowed. The majority (86%) of divorced or widowed women are divorced. All these groups are, however, too small to estimate conclusive heterogeneous reform impacts.

6.2. Heterogeneous Effects

Besides marital status, we further look at the heterogeneous responses for subgroups by family composition, educational attainment, and homeownership. Table 4 shows the results. Table A.5 shows the p-values testing the hypothesis that the coefficients by subgroups are equal.[30]

As argued in Section 3, differences in responses by marital status may stem from joint-retirement decisions of married women.[31] 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. 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, 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. The relative earnings share indicates who the primary earner is, and thus has more influence in household savings decisions. A growing literature analyses and documents the effect of household bargaining on intra-household decisions (Browning and Chiappori 1998; Chiappori 1992; Michaud et al. 2020). 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, the changes in expected future household labor earnings might be exacerbated for couples where men are the primary earners due to joint retirement. As a result, we expect that households in which men are primary earners reduce their savings rates more.

[30] To test whether the coefficients by subgroups are significantly different, we follow Clogg et al. 1995 in performing the group-specific estimation simultaneously using seemingly unrelated regressions. We use STATA's in-built seemingly unrelated estimations command *suest*. This command provides a common variance-covariance matrix. We can therefore perform a Hausmann test for equality of the point estimates between groups.

[31] 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)).

Table 4 provides suggestive evidence that effects are driven by couples with older male partners. The estimated response of married households with older male partner is larger, and the point estimate is significant at 5% level, while the estimated response of families with younger male partners is smaller and statistically insignificant. However, Table A.5 shows that the point estimates are not significantly different. Further, families with male primary earners (defined as families with female income share below the 50% mark) show larger reductions in savings. Yet again, the estimates by income shares are not significantly different.

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.

Table 4 shows the estimation results. We find that households consisting of highly educated women reduce their savings rates by 2.4 pp, which drives the overall impact. This finding suggests the importance of both financial literacy and possibilities to extend the working life. However, the difference in savings responses by education is statistically insignificant (Table A.5).

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 compliers 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 4 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. The difference between the two subgroups is significant at the 10% level in the full sample and at 1% level in the singles sample. 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.

6.3. Robustness Checks and Placebo Tests

Trying to circumvent several drawbacks of EVS data we perform a multitude of robustness and placebo checks. First, we test the robustness of the estimation results by varying model specifications, including choice of controls, bandwidths, and polynomial orders. We further establish the causality of our estimates by performing placebo tests. Finally, we use an alternative empirical model to test for the sensitivity of our estimates.

6.3.1. Robustness: Alternative Bandwidths, Specifications and Sample Restrictions

Table A.7 shows how the RD-DD estimator (γ_1) changes for the full sample, couples and singles if we do not add any controls (columns one), introduce year fixed effects (columns two) and introduce the full number of control variables and year fixed effects (columns three). The estimates are stable by varying the choices of controls. Rows one and two of Table A.13 show that the coefficients with varying control variables do not significantly differ from the baseline. Table A.8 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. Rows three and four of Table A.13 show that the estimates using the alternative bandwidths (three or five years) are not significantly different from the baseline estimate.

We show the results with a quadratic age trend (Table A.9) and with a quadratic cohort trend (Table A.10). The estimates are not sensitive to quadratic age controls. Row five of Table A.13 confirms that. 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. Row six of Table A.13 shows that the estimates using quadratic cohort controls are significantly different from the baseline results in the full sample and the couples sample. We believe that given we have so few numbers of bins around the cut-off, 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.11). The estimates are not sensitive to this restriction. Row seven of Table A.13 shows that the baseline estimate does not differ significantly from the estimates using the sample dropping retired husbands from the sample.

6.3.2. Placebo Tests

In addition, we show the RD and the RD-DD estimates using cohorts 1950, 1951, 1953 and 1954 as the placebo cut-offs in Table A.12. We find virtually no effects on the savings rate in the full sample at these placebo cut-offs. As expected, the absolute values of point estimates at the 1950 and 1954 cut-offs are almost always lower than our estimated effects at the 1952 cut-off, 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.[32]

Table A.13 depicts p-values for significance of the difference between the baseline specification (using 1952 as cut-off) and the respective RD-DD estimate using placebo cut-offs. We can reject the hypothesis that the placebo point estimates are identical to the baseline specification for various cut-offs and groups (1950: couples and singles, 1951: couples and singles, 1953: full sample and singles, 1954: couples). While for the remaining pairs, we cannot reject the hypothesis, partly due to big standard errors of the placebo estimates.

[32] Furthermore, we show placebo tests using men born between 1948 and 1955 in Section D.2. We find no significant changes in savings rates for the full sample, couples, and singles if we use households with men born since 1952 as the instrument. We also use unaffected cohorts with the same age composition as placebo samples in Section D.1. The results show that our estimated change in savings rate is not driven by a structural break along the age dimension.

6.3.3. 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 cut-off 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 (DD) setup:

$$Y_{it} = \theta_0 + \theta_1 D_i \times Post_{it} + \theta_2 D_i + \theta_3 Post_{it} + \beta X_{it} + \tau_t + \epsilon_{it} \tag{3}$$

We control for the same set of demographic characteristics and year fixed effect. Table A.14 shows the DD estimates by marital status and by bandwidth choices. Except for the sample using two years around the cut-off, the DD estimates show a similar pattern as the RD-DD results. Facing an increase in the 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 pp after the reform. Table A.15 shows the corresponding event-study estimates. Figure A.2 displays the event-study plots using the baseline sample of cohorts from 1948 to 1955. The main takeaways from the event-study results (Table A.15 and Figure A.2) are similar to that from Figure 5 which shows wave-by-wave RD estimates. Even though the underlying samples and specifications differ, the findings align. While women in couple households born since 1952 show lower savings rates in the reform waves, there is no difference in the control waves. This pattern is not apparent for singles. The full sample exhibits a similar pattern as the couples, although the standard errors are bigger.

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 cut-off). However, as there are only two waves before and after the reform, we can never formally test for the parallel trend assumption. Nevertheless, the DD 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.

All in all, our estimate on married women is robust to the standard RD robustness checks except for including quadratic cohort trends. The various robustness and placebo checks above establish that the estimated savings responses do not originate from age differences or life-cycle profiles in savings rates. Notably, the estimates for couples are not sensitive to most of the robustness tests. However, we still want to draw caution that our estimates suffer from several drawbacks of the EVS data, such as a small sample size for the singles and only observing birthdates at a yearly level. The bottom line is that our analysis shows non-positive savings responses to the reform for couple households (the 95% interval of responses is [-0.52 pp; -2.48 pp]).

6.4. Other Responses

In this section, we examine the responses in two dimensions to better understand the savings rate responses. First, we decompose the savings rate responses by investigating the response in disposable income and consumption expenditure. Then, 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.[33]

6.4.1. Disposable Income and Savings Categories

We first present the RD effects from Equation 1 using the reform waves in this section. Table 5 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 (2021), who find that the employment rates before age 60 are unaffected by the reform. Therefore, the change in the savings rates for couples is not due to a change in disposable income.

We then highlight the consumption expenditure responses in Table 6. Row one 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

[33] Tables A.6, A.21, A.22 and A.23 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.

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 accommodations.

6.4.2. Subcategories of Savings Rates Responses

Furthermore, we investigate three subcategories of savings in Table 7. 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 pp. 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 adjustments 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. Discussion

To understand the magnitude of the estimated savings response, we first provide a micro-simulation exercise. Then, we compare our results with existing literature.

In this exercise, we aim to understand the average impact of the labor supply and savings responses on available funds in pre- and post-retirement periods. In this context “available funds" are after-tax income minus savings in pre-retirement periods and pension-benefits plus dissaving in post-retirement periods. Using the anticipated changes in the retirement age obtained from SHARE data (see Section 3), the German pension formula and estimated savings rate responses for women in couple households, we compare the impacts of the reform on annual pre-retirement and post-retirement available funds for a representative woman in two scenarios.[34] In the first

[34] As described in Appendix Section E.3 we use the mean values of accumulated earnings points and other statistics of married women for this calculation.

scenario, the married woman reacts to the reform by only updating her retirement age (labor supply response), while in the second scenario she also updates her savings behavior according to our estimates. The comparison aims to see if the estimated savings response brings people closer to a pre-reform equilibrium and, if so, by how much.[35] If she only updates her labor supply in response to the reform, her annual pre-retirement available funds remain unchanged. In this scenario she gathers more pension points and faces lower deductions to her pension benefits. Consequently, her annual pension benefits increase. Further, the respresenatative household has more periods (while she is active on the labor market) to accumulate savings which are consumed in a lower number of retirement periods. In this scenario, available funds in a representative post-retirement period are 12.7% higher than in the pre-reform setting.[36] In scenario two, we introduce the estimated reduction in the savings rate of 1.5 pp for married women together with the reform-induced increase in labor supply. The decrease in savings increases available funds in representative pre-retirement periods by 1.7%. At the same time, lower savings rates ceteris paribus reduce the total amount of savings for retirement. However, the higher number of periods that households can build up savings together with the lower number of periods of dissaving outweighs the impacts of decreased savings rates. Available funds in a representative post-retirement period increase by 10.4% compared to the pre-reform setting in the second scenario, less than the 12.5% from scenario one. The difference in representative post-retirement available funds between the two scenarios is due to the updated savings behavior.

This calculation suggests that women in couples understand that the reform-driven employment effect increases their overall lifetime income and use savings as a vehicle to redistribute the overall increase in lifetime income slightly more toward pre-retirement periods in comparison to the scenario in which they only update their labor supply.[37] After the update in savings rates, however, couple households still experience a higher increase in post-retirement available funds than in pre-retirement available funds.

Furthermore, we compare our results with three types of studies. First, we compare our estimates with responses to income shocks in the consumption smoothing literature (e.g.: Browning et al. 2013; Ganong et al. 2020; Mian et al. 2013). In particular, we compare our estimates with two types of income shock in this literature: labor income change (Ganong et al. 2020; McGee 2021) and housing price change (Browning et al. 2013; Mian et al. 2013). The

[35] We calculate the average income of women in couple households. We assume constant survival probabilities, take the average savings rate of households from EVS data and assume that households dissave a fixed proportion of their savings during retirement. See Appendix Section E.3 for further details of the calculation.

[36] The overall lifetime available funds of average married women increase by 5.83% through the shift in retirement timing.

[37] It is beyond the scope of the paper to understand whether the savings rates updates represent optimal consumption smoothing of agents, given a certain utility and discounting factor and perfect foresight. It could also be that agents are cautious or, to a certain degree, ignorant of the lifetime-income effects.

marginal propensity to consume (MPC) in our setting is 0.14. This implies that for an additional euro of lifetime wealth, a household consumes an additional 14 cents.[38] The MPC of our estimate is similar to those found in studies of labor income variations. For example, Ganong et al. (2020) find a 21 cents MPC for nondurable goods and services by using exogenous variations in labor income. A recent meta-analysis (Havranek and Sokolova 2020) finds the mean MPC from 246 estimates in previous papers to be 29 cents. Our MPC is smaller than those found in studies of changes in housing prices. For example, Mian et al. (2013) find an average MPC of 5 to 7 cents for every dollar loss in housing wealth by exploring the 2007-09 housing collapse in the U.S. The comparison is consistent with previous studies showing that households respond differently to labor supply-related windfalls than to housing wealth windfalls (McGee 2021). One potential reason could be that housing wealth is less liquid, and downsizing incurs more adjustment costs.

Second, we compare our estimates with studies of other pension reforms, such as a change in the generosity of social security benefits (e.g., Attanasio and Brugiavini 2003; Attanasio and Rohwedder 2003; Feng et al. 2011; Lachowska and Myck 2018). While most of these studies identify an increase in private savings as a result of a decline in pension wealth, we document a decline in private savings. As discussed above, it is because the labor supply responses are different. The salient and sizeable increase in future labor earnings implies that the lifetime income will increase on average in our setting rather than decrease. However, the size of our estimates is similar. For example, Lachowska and Myck (2018) show that the substitution between pension wealth and savings is 0.3 in Poland. Attanasio and Rohwedder (2003) show that a £1 increase in pension wealth leads to a £0.65 decrease in private savings for ages 43 to 53. In our setting, an increase in expected lifetime wealth of €1 leads to a decrease of €0.43 in households' savings.[39]

Lastly, we compare our findings with the impacts of other policy interventions on private savings, such as pension notification letter (Dolls et al. 2018) and abolishment of a pension supplement (Tyros and Van Ewijk 2022). Dolls et al. (2018) find that receiving pension notification letters in Germany increases annual household savings by 300 euros, a 15% increase. Our savings response is an order of magnitude larger. This is not surprising since abolishing women's pension pathway has a sizable impact on lifetime income rather than a pure information treatment. One study in

[38] The lifetime income of an average earner, who plans to delay retirement by 1.5 years, will increase by around €30000 (based on calculation in Appendix Section E.1). In Table 6, we find that such a household consumes €30 per month more in leisure goods. Therefore, total lifetime consumption increases roughly by €4320 (30*12*12 years). The 12-year is the period between ages 48 and 60.

[39] The lifetime income of an average earner, who plans to delay retirement by 1.5 years, will increase by around €30000 (based on calculation in Appendix Section E.1). In Table A.6, we show that households save €90 per month less. Therefore, overall savings decrease roughly by €12960 (90*12*12 years). The 12-year is the period between ages 48 and 60. Therefore, we divide the €12960 decrease in savings by €30000 to compare with the estimates in the literature.

the Dutch context investigates the effect of a change in future income by exploring the abolishment of the state pension partner supplement (Tyros and Van Ewijk 2022). It documents no savings and consumption responses. The salience of the reform could account for the discrepancy between ours and their findings. Tyros and Van Ewijk (2022) state that the lack of response in their paper is due to a lack of awareness. They explain that people were not fully aware of the existence and discontinuation of the supplement. In our setting, the existence and abolishment of women's pension pathway are very salient, and people have a relatively long time to respond to the change.

8. Conclusion

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 pp, which is driven by households with married women. There is considerable heterogeneity in these effects. 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 household income. Our findings show that the treated households absorb the pension wealth shock without increasing their savings.

Several takeaways emerge from our findings: First, our analysis shows a non-positive response of private savings to abolishing the early retirement pathway for women. The 95% interval of responses in savings rates is [-0.52pp;-2.48pp] for women in couples. This documents direct evidence of dissaving when the adjustment in labor supply absorbs the loss in pension wealth. It supports Feldstein (1974)'s notion that the overall effect of public pension wealth on private savings relies on the magnitude of the employment effect.

Second, a limitation of this study is the data quality. For example, we only observe birth dates at a yearly level, which makes the RD design less robust. Although EVS is the largest data source of its kind in Europe, our estimates still suffer from a small sample size. Results for singles are inconclusive partly due to the small sample size.

Third, the findings of this paper have timely and direct policy implications. We show that individuals and households are aware of the pension system changes long before they reach their retirement age. These households adjust their savings and consumption expenditures accordingly. This finding suggests that policymakers should incorporate these anticipatory adjustments when evaluating pension reforms, particularly the role of consumption expenditure, which is at the heart of welfare evaluation.

Lastly, the external validity of the results could be questioned given that we focus on a reform that affects only women and is very salient, announced more than a decade before realization. We show that when the increase in the working horizon is salient and sizable, the increase in lifetime labor income could outweigh the decrease in pension wealth. Under this situation, workers dissave to smooth consumption. Whether the increase in future labor earnings outweighs the reduction in pension wealth depends on a few parameters, such as labor supply elasticity, salience, the size of the increase in early retirement age, etc. However, the feature of a discontinued increase in early retirement age is similar to those carried out in today's policy world, where a common theme has been to induce workers to retire later by closing early retirement routes. 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 ages 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.

References

Atalay, Kadir, Garry F Barrett, and Peter Siminski (2019). "Pension incentives and the joint retirement of couples: Evidence from two natural experiments". In: *Journal of Population Economics* 32.3, pp. 735–767.

Attanasio, Orazio P. and Agar Brugiavini (2003). "Social Security and Households' Saving". In: *The Quarterly Journal of Economics* 118.3, pp. 1075–1119.

Attanasio, Orazio P. and Susann Rohwedder (2003). "Pension Wealth and Household Saving: Evidence from Pension Reforms in the United Kingdom". In: *American Economic Review* 93.5, pp. 1499–1521.

Attanasio, Orazio P and Guglielmo Weber (2010). "Consumption and saving: models of intertemporal allocation and their implications for public policy". In: *Journal of Economic literature* 48.3, pp. 693–751.

Becker, Irene (2014). "EVS und SOEP: methodische Aspekte bei Verteilungsanalysen". In: *soeb-Working-Paper*.

Becker, Irene, Joachim R Frick, Markus M Grabka, Richard Hauser, Peter Krause, and Gert G Wagner (2003). "A comparison of the main household income surveys for Germany: EVS and SOEP". In: *Reporting on Income Distribution and Poverty*. Springer, pp. 55–90.

Bertoni, Marco, Giorgio Brunello, and Gianluca Mazzarella (2018). "Does postponing minimum retirement age improve healthy behaviors before retirement? Evidence from middle-aged Italian workers". In: *Journal of Health Economics* 58, pp. 215–227.

Biewen, Martin and Andos Juhasz (2012). "Understanding rising income inequality in Germany, 1999/2000–2005/2006". In: *Review of Income and Wealth* 58.4, pp. 622–647.

Bissonnette, Luc and Arthur Van Soest (2015). "Heterogeneity in consumers' income and pension expectations". In: *Journal of Pension Economics & Finance* 14.4, pp. 439–465.

Borella, Margherita, Mariacristina De Nardi, and Fang Yang (2018). "The aggregate implications of gender and marriage". In: *The Journal of the Economics of Ageing* 11, pp. 6–26.

Börsch-Supan, Axel (2017). *Survey of health, ageing and retirement in Europe (SHARE) wave 5*. Tech. rep. 0.

Bottazzi, Renata, Tullio pelli, and Mario Padula (2006). "Retirement expectations, pension reforms, and their impact on private wealth accumulation". In: *Journal of Public Economics* 90.12, pp. 2187–2212.

Browning, Martin and Pierre-André Chiappori (1998). "Efficient intra-household allocations: A general characterization and empirical tests". In: *Econometrica*, pp. 1241–1278.

Browning, Martin, Mette Gørtz, and Søren Leth-Petersen (2013). "Housing wealth and consumption: a micro panel study". In: *The Economic Journal* 123.568, pp. 401–428.

Browning, Martin, Olivier Donni, and Mette Gørtz (2021). "Do you have time to take a walk together? Private and joint time within the household". In: *The Economic Journal* 131.635, pp. 1051–1080.

Bulmahn, Thomas (1998). *Rette sich, wer kann? Die Krise der gesetzlichen Rentenversicherung und die Privatisierung der Altersvorsorge.* Tech. rep. WZB Discussion Paper.

Bundesamt, Statistisches (2005a). *Qualitätsbericht Einkommens- und Verbrauchsstichprobe 1998.* Tech. rep.

Bundesamt, Statistisches (2005b). *Qualitätsbericht Einkommens- und Verbrauchsstichprobe 2003.* Tech. rep.

Bundesamt, Statistisches (2012). *Qualitätsbericht Einkommens- und Verbrauchsstichprobe 2008.* Tech. rep.

Börsch-Supan, Axel, Reinhold Schnabel, Simone Kohnz, and Giovanni Mastrobuoni (2004). "Micro-modeling of retirement decisions in Germany". In: *Social Security Programs and Retirement around the World: Micro-Estimation.* University of Chicago Press, pp. 285–344.

Caliendo, Frank and Kevin XD Huang (2008). "Overconfidence and consumption over the life cycle". In: *Journal of Macroeconomics* 30.4, pp. 1347–1369.

Carta, Francesca and Marta De Philippis (2019). "Working horizon and labour supply: the effect of raising minimum retirement age on middle-aged individuals". In: *Banca d'Italia, Temi di discussione* 26.

Chiappori, Pierre-André (1992). "Collective labor supply and welfare". In: *Journal of political Economy* 100.3, pp. 437–467.

Ciani, Emanuele, Adeline Delavande, Ben Etheridge, and Marco Francesconi (2023). "Policy uncertainty and information flows: Evidence from pension reform expectations". In: *The Economic Journal* 133.649, pp. 98–129.

Clogg, Clifford C, Eva Petkova, and Adamantios Haritou (1995). "Statistical methods for comparing regression coefficients between models". In: *American journal of sociology* 100.5, pp. 1261–1293.

Coile, Courtney and Jonathan Gruber (2004). “The Effect of Social Security on Retirement in the United States”. In: *Social Security Programs and Retirement around the World: Micro-Estimation*. NBER Chapters. National Bureau of Economic Research, Inc, pp. 691–730.

Coile, Courtney C (2015). “Economic Determinants of Workers’ Retirement Decisions”. In: *Journal of Economic Surveys* 29.4, pp. 830–853.

Cribb, Jonathan, Carl Emmerson, and Gemma Tetlow (2016). “Signals matter? Large retirement responses to limited financial incentives”. In: *Labour Economics* 42, pp. 203–212.

De Grip, Andries, Didier Fouarge, and Raymond Montizaan (2013). “How sensitive are individual retirement expectations to raising the retirement age?” In: *De Economist* 161.3, pp. 225–251.

De Nardi, Mariacristina, Eric French, John Bailey Jones, and Rory McGee (2021). *Why Do Couples and Singles Save During Retirement?* Tech. rep. National Bureau of Economic Research.

Delavande, Adeline and Susann Rohwedder (2017). “Changes in spending and labor supply in response to a Social Security benefit cut: Evidence from stated choice data”. In: *The Journal of the Economics of Ageing* 10, pp. 34–50.

Dolls, Mathias, Philipp Doerrenberg, Andreas Peichl, and Holger Stichnoth (2018). “Do retirement savings increase in response to information about retirement and expected pensions?” In: *Journal of Public Economics* 158, pp. 168–179.

Dustmann, Christian, Bernd Fitzenberger, and Markus Zimmermann (2018). “Housing expenditures and income inequality”. In: *ZEW-Centre for European Economic Research Discussion Paper* 18-048.

Eibich, Peter (2015). “Understanding the effect of retirement on health: Mechanisms and heterogeneity”. In: *Journal of Health Economics* 43, pp. 1–12.

Engels, Barbara, Johannes Geyer, and Peter Haan (2017). “Pension incentives and early retirement”. In: *Labour Economics* 47, pp. 216–231.

Fehr, Hans, Manuel Kallweit, and Fabian Kindermann (2016). “Household Formation, Female Labor Supply, and Savings”. In: *The Scandinavian Journal of Economics* 118.4, pp. 868–911.

Feldstein, Martin (1974). “Social Security, Induced Retirement, and Aggregate Capital Accumulation”. In: *Journal of Political Economy* 82.5, pp. 905–926.

Feng, Jin, Lixin He, and Hiroshi Sato (2011). “Public pension and household saving: Evidence from urban China”. In: *Journal of Comparative Economics* 39.4, pp. 470 –485.

Fischer, Björn and Kai-Uwe Müller (2020). "Time to care? The effects of retirement on informal care provision". In: *Journal of Health Economics* 73, p. 102350.

Ganong, Peter, Damon Jones, Pascal J Noel, Fiona E Greig, Diana Farrell, and Chris Wheat (2020). *Wealth, Race, and Consumption Smoothing of typical Income Shocks*. Tech. rep. NBER.

García-Miralles, Esteban and Jonathan Leganza (2021). "Joint Retirement of Couples: Evidence from Discontinuities in Denmark". In: *Available at SSRN 3778885*.

Geyer, Johannes and Clara Welteke (2021). "Closing Routes to Retirement for Women How Do They Respond?" In: *Journal of Human Resources* 56.1, pp. 311–341.

Geyer, Johannes, Peter Haan, Anna Hammerschmid, and Michael Peters (2020). "Labor market and distributional effects of an increase in the retirement age". In: *Labour Economics*, p. 101817.

Goebel, Jan, Markus M Grabka, Stefan Liebig, Martin Kroh, David Richter, Carsten Schröder, and Jürgen Schupp (2019). "The German socio-economic panel (SOEP)". In: *Jahrbücher für Nationalökonomie und Statistik* 239.2, pp. 345–360.

Gustman, Alan L and Thomas L Steinmeier (2000). "Retirement in dual-career families: a structural model". In: *Journal of Labor economics* 18.3, pp. 503–545.

Hairault, Jean-Olivier, Thepthida Sopraseuth, and François Langot (2010). "Distance to retirement and older workers 'employment: The case for delaying the retirement age". In: *Journal of the European Economic association* 8.5, pp. 1034–1076.

Havranek, Tomas and Anna Sokolova (2020). "Do consumers really follow a rule of thumb? Three thousand estimates from 144 studies say "probably not"". In: *Review of Economic Dynamics* 35, pp. 97–122.

Hess, Moritz (2017). "Rising preferred retirement age in Europe: Are Europe's future pensioners adapting to pension system reforms?" In: *Journal of Aging & Social Policy* 29.3, pp. 245–261.

Hsieh, Chang-Tai (2003). "Do consumers react to anticipated income changes? Evidence from the Alaska permanent fund". In: *American Economic Review* 93.1, pp. 397–405.

Hurd, Michael D (1990). "The joint retirement decision of husbands and wives". In: *Issues in the Economics of Aging*. University of Chicago Press, 1990, pp. 231–258.

Jappelli, Tullio and Luigi Pistaferri (2010). "The consumption response to income changes". In: *Annu. Rev. Econ.* 2.1, pp. 479–506.

Krueger, Alan B. and Jörn-Steffen Pischke (1992). “The Effect of Social Security on Labor Supply: A Cohort Analysis of the Notch Generation”. In: *Journal of Labor Economics* 10.4, pp. 412–437.

Lachowska, Marta and Michał Myck (2018). “The effect of public pension wealth on saving and expenditure”. In: *American Economic Journal: Economic Policy* 10.3, pp. 284–308.

Lalive, Rafael and Pierpaolo Parrotta (2017). “How does pension eligibility affect labor supply in couples?” In: *Labour Economics* 46, pp. 177–188.

Lindeboom, Maarten and Raymond Montizaan (2020). “Disentangling retirement and savings responses”. In: *Journal of Public Economics* 192, p. 104297.

Malter, Frederic and Axel Börsch-Supan (2017). “SHARE wave 6: panel innovations and collecting dried blood spots”. In: *Munich: Munich Center for the Economics of Aging (MEA).*

Manoli, Day and Andrea Weber (2016). “Nonparametric evidence on the effects of financial incentives on retirement decisions”. In: *American Economic Journal: Economic Policy* 8.4, pp. 160–82.

Mastrobuoni, Giovanni (2009). “Labor supply effects of the recent social security benefit cuts: Empirical estimates using cohort discontinuities”. In: *Journal of Public Economics* 93.11-12, pp. 1224–1233.

Mazzocco, Maurizio, Claudia Ruiz, and Shintaro Yamaguchi (2014). “Labor supply and household dynamics”. In: *American Economic Review* 104.5, pp. 354–59.

McGee, Rory (2021). *Old age savings and house price shocks.* Tech. rep. Research Report.

Mian, Atif, Kamalesh Rao, and Amir Sufi (2013). “Household balance sheets, consumption, and the economic slump”. In: *The Quarterly Journal of Economics* 128.4, pp. 1687–1726.

Michaud, Pierre-Carl, Arthur Van Soest, and Luc Bissonnette (2020). “Understanding joint retirement”. In: *Journal of economic behavior & organization* 173, pp. 386–401.

Nelson, Julie A (1988). “Household economies of scale in consumption: theory and evidence”. In: *Econometrica: Journal of the Econometric Society*, pp. 1301–1314.

Pagel, Michaela (2017). “Expectations-based reference-dependent life-cycle consumption”. In: *The Review of Economic Studies* 84.2, pp. 885–934.

Queisser, Monika and Edward Whitehouse (12/2006). *Neutral or Fair?: Actuarial Concepts and Pension-System Design.* OECD Social, Employment and Migration Working Papers 40. OECD Publishing.

Stancanelli, Elena (2017). “Couples’ retirement under individual pension design: A regression discontinuity study for France”. In: *Labour Economics* 49, pp. 14–26.

Statistische Ämter des Bundes und der Länder (08/2018). *Einkommens- und Verbrauchsstichprobe*. Forschungsdatenzentren der Statistischen Ämter des Bundes und der Länder. URL: http://www.forschungsdatenzentrum.de/bestand/evs/index.asp.

Staubli, Stefan and Josef Zweimüller (2013). “Does raising the early retirement age increase employment of older workers?” In: *Journal of Public Economics* 108, pp. 17 –32.

Tyros, Stefanos, Casper Van Ewijk, et al. (2022). *Shocks to Future Income and (Lack of) Consumption-Smoothing: Evidence from the Abolition of the State Pension Partner-Supplement in the Netherlands*. Tech. rep.

9. Tables and Figures

Table 1: 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 1 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.

Table 2: Summary statistics

	Reform waves (2003, 2008) Born		Control waves (1993, 1998) 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 2 reports means and standard deviations (in parenthesis) 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 3: Effects of the reform on household savings rates

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951 (baseline)	-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. Table 3 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 4: Heterogeneous reform 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 4 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 A.5 shows p-values for significance of the difference of point estimates between the groups.

Table 5: Effects of the reform 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 5 shows the estimated changes in the equivalized monthly disposable income using the RD method in the reform waves.

Table 6: Effects of the reform 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 private insurance	0.000	-0.007	0.026
	(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 6 shows the estimated changes in the equivalized monthly consumption expenditure using the RD method in the reform waves.

Table 7: Effects of the reform on subcategories of savings rates

	Full sample	Couples	Singles
Overall Savings rate	-0.010*	-0.015**	0.007
	(0.005)	(0.006)	(0.015)
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 7 shows the estimated changes in the subcategories of savings rates using the RD method in the reform waves.

Figure 1: Illustration of life-time income effect of the reform by labor supply response

Notes: Figure 1 shows the potential lifetime income effects of the reform by labor supply reaction. The stylized woman retires at age 60 if born before 1952 (pre-reform scenario, solid black line). We depict the percentage changes in discounted lifetime income at age 50 for four scenarios depending on the labor market exit age for a stylized individual who retires at age 63 if born from 1952 onward: 1) retire at age 60 and claim at 63 (dashed black line); 2) retire at age 61 and claim at 63 (dotted black line); 3) retire at 62 and claim at 63 (dashed gray line); 4) retire at 63 and claim at 63 (solid gray line). Appendix Section E.2 describes the details of how we calculated the discounted lifetime income.

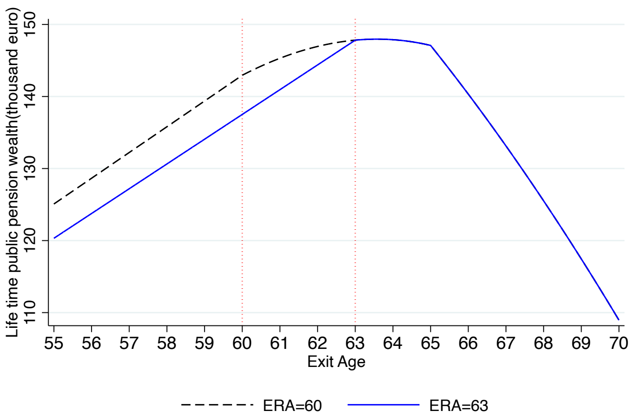


Figure 2: Illustration of the stylized pension wealth

Notes: Figure 2 illustrates the stylized pension wealth $B(R)$ depending on the individual labor market exit age (x-axis) for individuals who face an early retirement age (ERA) of 60 (dashed black line, pre-reform ERA for eligible women) and an ERA of 63 (solid blue line, post-reform ERA for all women).

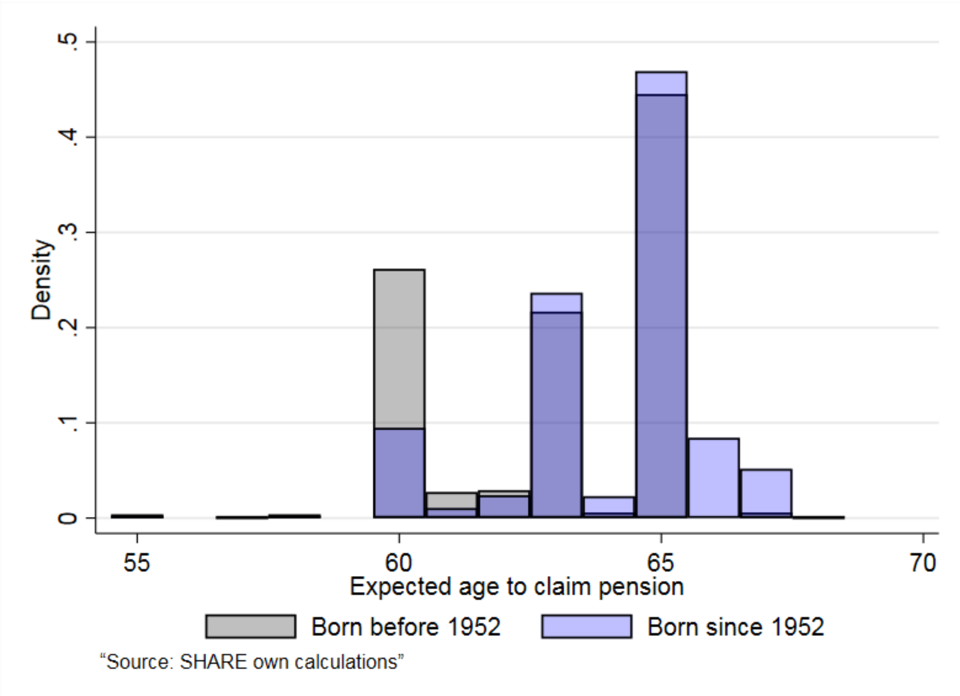
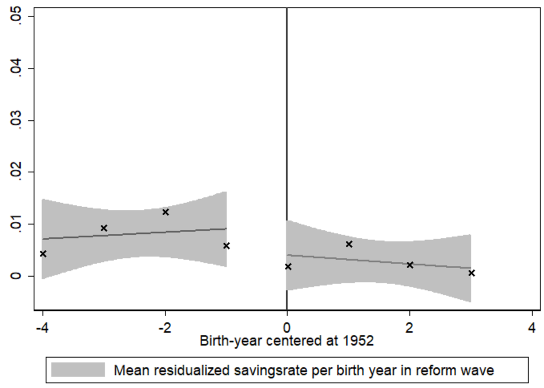
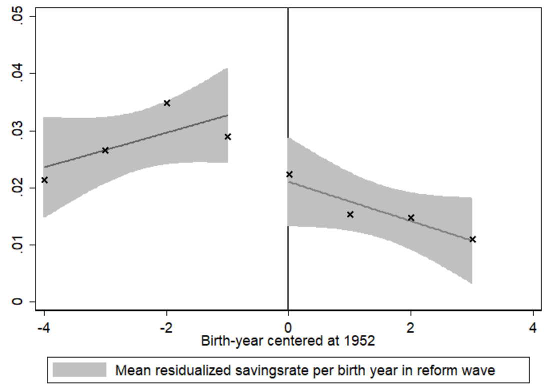


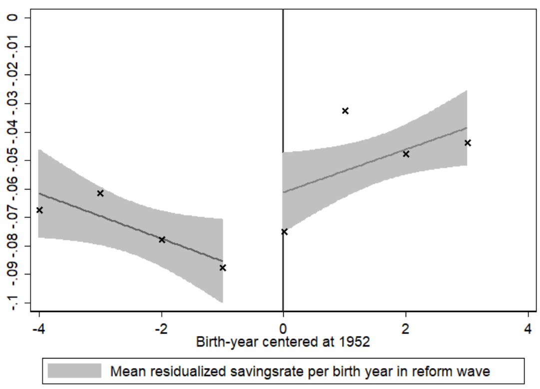
Figure 3: Expected retirement age by treatment status
Notes: Using the SHARE data, Figure 3 shows the distribution of expected retirement age for cohorts born before 1952 and cohorts born since 1952.



(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.

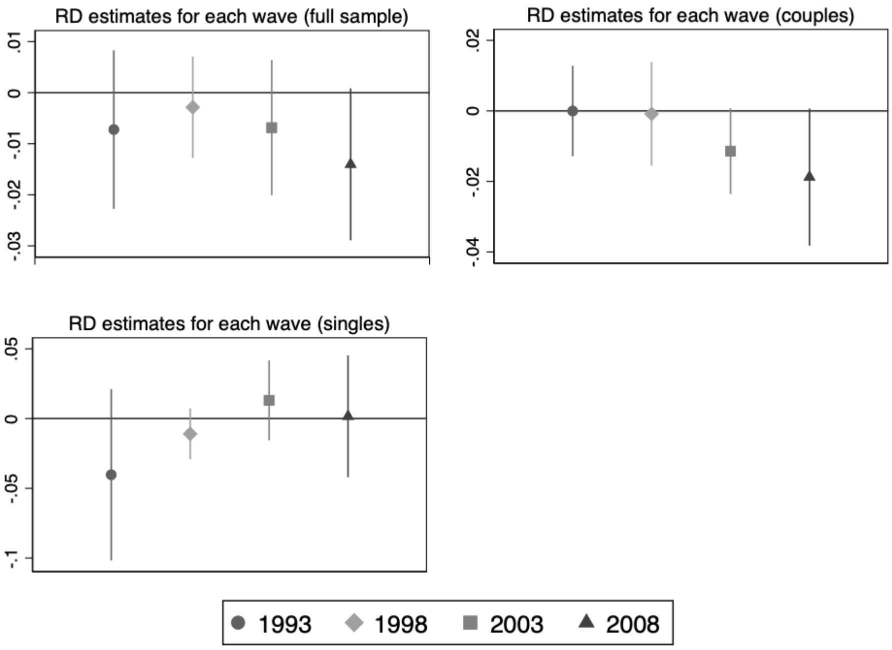


Figure 5: Wave-by-wave point estimates- savings rates

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).

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

Stefan Etgeton — DIW Berlin

Björn Fischer — ZEW Mannheim,

Han Ye — University of Mannheim, IZA, ZEW

Online Appendix

A. Appendix Tables and Figures

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<.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: P-value on significance in difference of point estimates for heterogeneous effects (Table 4)

	Full Sample	Couples	Singles
Education	0.4637	0.1121	0.6881
Homeownership	0.0643	0.6331	0.0081
Age difference to partner		0.3342	
Primary earner		0.4413	

Notes: Table A.5 shows the p-values on the significance of differences in point estimates in heterogeneous effects. The Null-Hypothesis is that the point estimates in subgroups (e.g. low vs. highly educated) are identical. P-values higher than 0.1 indicate that we cannot reject the H0 with a probability higher than 90%. The null hypothesis (H0) is that the point estimates from the heterogeneous groups are significantly different.

Table A.6: 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	-22.981***	-5.113	
	(5.308)	(12.916)	
Born after 1951=1 × post=1			-20.263*
			(10.478)
Observations	11,239	6,997	18,236
R^2	0.033	0.043	0.052
Dependent Variable Mean	266.251	393.879	315.116
		Couples	
Born after 1951	-32.650***	3.300	
	(7.902)	(15.141)	
Born after 1951=1 × post=1			-39.749*
			(19.272)
Observations	8,710	5,663	14,373
R^2	0.022	0.032	0.041
Dependent Variable Mean	294.858	427.422	346.991
		Singles	
Born after 1951	15.087	-49.644***	
	(30.362)	(8.506)	
Born after 1951=1 × post=1			66.387**
			(25.198)
Observations	2,529	1,334	3,863
R^2	0.030	0.051	0.048
Dependent Variable Mean	169.271	253.836	198.430
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 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.7: Effects on savings rate by varying controls, RD-DD estimates

	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.7 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 baseline estimates (see Table 3, column 3). The estimates are stable by varying the choices of controls. Column 3 corresponds to the baseline estimates from Table 3.

Table A.8: 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.8 show the RD-DD estimator by various bandwidth choices. The baseline is 4 years (column 2), corresponding to Table 3, column 3.

Table A.9: Effects of the reform on households' savings rates, with a quadratic age trend

	Baseline			Quadratic age controls		
	RD reform year	RD control years	RD-DD	RD reform year	RD control years	RD-DD
			Full Sample			
Born after 1951	-0.010*	-0.005*		-0.009	-0.004	
	(0.005)	(0.002)		(0.005)	(0.002)	
Born after 1951=1 × post=11			-0.006			-0.006
			(0.006)			(0.006)
Observations	11,239	13,604	24,843	11,239	13,604	24,843
Dependent Variable Mean	0.109	0.132	0.121	0.094	0.118	0.107
			Couples			
Born after 1951	-0.015**	-0.000		-0.014**	0.001	
	(0.006)	(0.001)		(0.006)	(0.001)	
Born after 1951=1 × post=1			-0.015**			-0.015**
			(0.005)			(0.005)
Observations	8,710	11,198	19,908	8,710	11,198	19,908
Dependent Variable Mean	0.117	0.142	0.131	0.094	0.118	0.107
			Singles			
Born after 1951	0.007	-0.025**		0.010	-0.026**	
	(0.015)	(0.010)		(0.014)	(0.010)	
Born after 1951=1 × post=1			0.033			0.033
			(0.025)			(0.025)
Observations	2,529	2,406	4,935	2,529	2,406	4,935
Dependent Variable Mean	0.080	0.086	0.083	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 estimators. Columns 2-4 show baseline estimates, (see Table 3) and columns 5-7 show estimates with a quadratic age trend.

Table A.10: Effects of the reform on households savings rates, with a quadratic cohort trend

	Baseline			Quadratic cohort controls		
	RD reform year	RD control years	RD-DD	RD reform year	RD control years	RD-DD
			Full Sample			
Born after 1951	-0.010*	-0.005*		0.004	-0.002	
	(0.005)	(0.002)		(0.003)	(0.001)	
Born after 1951=1 × post=11			-0.006			0.006
			(0.006)			(0.004)
Observations	11,239	13,604	24,843	11,239	13,604	24,843
Dependent Variable Mean	0.109	0.132	0.121	0.094	0.118	0.107
			Couples			
Born after 1951	-0.015**	-0.000		0.001	0.003	
	(0.006)	(0.001)		(0.006)	(0.002)	
Born after 1951=1 × post=1			-0.015**			-0.002
			(0.005)			(0.006)
Observations	8,710	11,198	19,908	8,710	11,198	19,908
Dependent Variable Mean	0.117	0.142	0.131	0.094	0.118	0.107
			Singles			
Born after 1951	0.007	-0.025**		0.015*	-0.026***	
	(0.015)	(0.010)		(0.008)	(0.003)	
Born after 1951=1 × post=1			0.033			0.042***
			(0.025)			(0.011)
Observations	2,529	2,406	4,935	2,529	2,406	4,935
Dependent Variable Mean	0.080	0.086	0.083	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.10 show the RD-DD estimators. Columns 2-4 show baseline estimates, (see Table 3) and columns 5-7 show estimates with a quadratic cohort trend.

Table A.11: RD-DD- Effects of the reform on couple households' savings rates, including restrictions on the partner

	Baseline			Male partners not retired		
	RD reform year	RD control year	RD-DD	RD reform year	RD control year	RD-DD
Born after 1951	-0.015**	0.000		-0.014***	0.002	
	(0.006)	(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	8,710	11,198	19,908	6,673	10,736	17,409
R^2	0.012	0.002	0.011	0.007	0.003	0.006
Dependent Variable Mean	0.117	0.142	0.131	0.127	0.143	0.137

Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.11 shows the robustness by varying restrictions made to the male partners. We compare the baseline impacts (Table 3) on couples with estimates using samples with male partners who are not retired.

Table A.12: Effects of the reform 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
Reform cut-off 1952	-0.010*	-0.006	-0.015**	-0.015**	0.007	0.033
(Baseline)	(0.005)	(0.006)	(0.006)	(0.005)	(0.015)	(0.025)
Observations	11,239	24,843	8,710	19,908	2,529	4,935
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 1951	0.009	-0.003	0.020*	0.001	-0.026	-0.016
	(0.009)	(0.009)	(0.010)	(0.010)	(0.018)	(0.021)
Observations	11,546	24,503	8,882	19,556	2,664	4,947
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.12 shows the RD-DD estimates at placebo cutoffs (born since 1950, 1952 and 1954).

Table A.13: P-value on significance in difference of point estimates for RD-DD robustness checks

	Full Sample	Couples	Singles
BL vs. no controls (Table A.7)	0.6466	0.9543	0.6953
BL vs. no controls and no year fixed effects (Table A.7)	0.6230	0.9541	0.9702
BL vs. BW 3 years (Table A.8)	0.4445	0.3245	0.7515
BL vs. BW 5 years (Table A.8)	0.1424	0.2436	0.6384
BL vs quadratic age control (Table A.9	0.1447	0.1591	0.6487
BL vs quadratic cohort control (Table A.10)	0.0010	0.0011	0.4274
BL vs. male restriction (Table A.11)		0.4567	
BL vs. 1950 cutoff (Table A.12)	0.1837	0.0000	0.0036
BL vs. 1951 cutoff (Table A.12)	0.5166	0.0603	0.0080
BL vs. 1953 cutoff (Table A.12)	0.0715	0.4026	0.0213
BL vs. 1954 cutoff (Table A.12)	0.3834	0.0036	0.2495

Notes: Table A.13 shows the p-values on the significance of differences in point estimates in robustness checks. The Null-Hypothesis is that the point estimates in the baseline and robustness check are identical. The baseline is the main RD-DD specification as given in Table 3, column 3. P-values higher than 0.1 indicate that we cannot reject the H0 with a probability higher than 90%. The null hypothesis (H0) is that the baseline value and the point estimate from the respective robustness check are significantly different.

Table A.14: Effects of the reform on household savings rates - DD 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.14 shows the DD 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.15: Effects of the reform 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.15 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.16: Effects on households' savings rates using a placebo sample for wave 2003

	Full sample	Couples	Singles
Born after 1951 (younger than 52) in 2003	-0.007	-0.011*	0.013
(Baseline)	(0.006)	(0.005)	(0.012)
Observations	5,674	4,524	1,150
R^2	0.007	0.004	0.012
Dependent Variable Mean	0.122	0.129	0.095
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.16 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.17: Effects on households savings rates using a placebo sample for wave 2008

	Full sample	Couples	Singles
Born after 1951 (younger than 57) in 2008	-0.014*	-0.019*	-0.000
(Baseline)	(0.007)	(0.008)	(0.018)
Observations	5,565	4,186	1,379
R^2	0.011	0.007	0.006
Dependent Variable Mean	0.095	0.105	0.068
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.17 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.18: Effects of the reform on household savings rates using men as a placebo sample

	RD reform year	RD control years	RD-DD
		Full sample	
Born after 1951	-0.007	0.002	
	(0.006)	(0.004)	
Born after 1951=1 × post=1			-0.009
			(0.005)
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	-0.005	0.002	
	(0.006)	(0.004)	
Born after 1951=1 × post=1			-0.007
			(0.007)
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	-0.020	0.007*	
	(0.015)	(0.004)	
Born after 1951=1 × post=1			-0.026
			(0.016)
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.18 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.19: Effects of the reform on household savings rates - DD, 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.19 shows the DD estimates for a placebo sample consisting of households with male members born between 1948 and 1955.

Table A.20: Effects of the reform 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.20 shows the event study estimates for a placebo sample consisting of households with male members born between 1948 and 1955.

Table A.21: Effects of the reform on household savings rates using 1998 as control

	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.21 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. The baseline (Table 3) uses 1998 and 1993 as control waves.

Table A.22: Effects of the reform on monthly equivalized household disposable income using 1998 as control

	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.22 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.

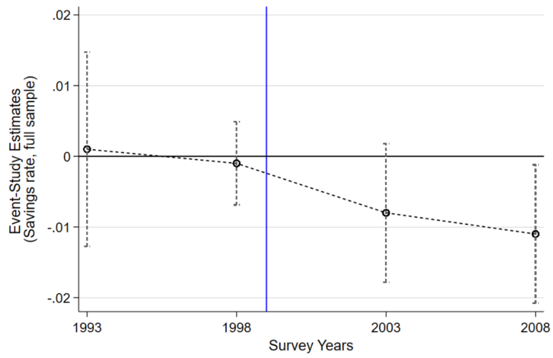
Table A.23: Effects of the reform on monthly equivalized consumption expenditures using 1998 as control: RD-DD

	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	✓	✓	✓

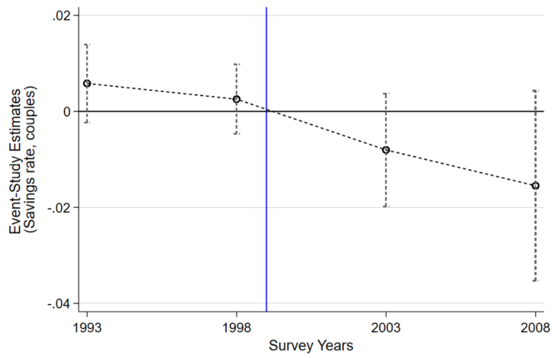
Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01. Table A.23 reports the RD estimates in the reform waves (column 1) and the control wave (1998 wave only, column 2), and the RD-DD estimates in column 3. All specifications control for wave-fixed effects and predetermined variables 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

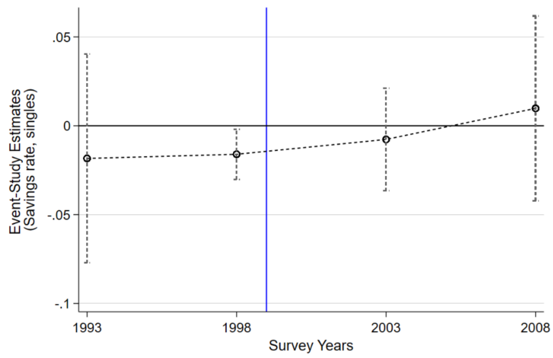
Notes: Figure A.1 shows the density by birth cohorts of our baseline sample.



(a) Full sample



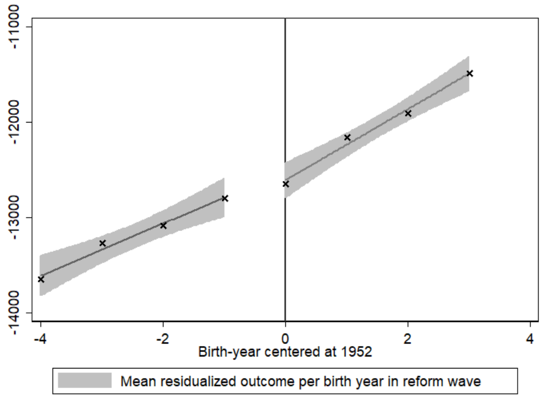
(b) Couples



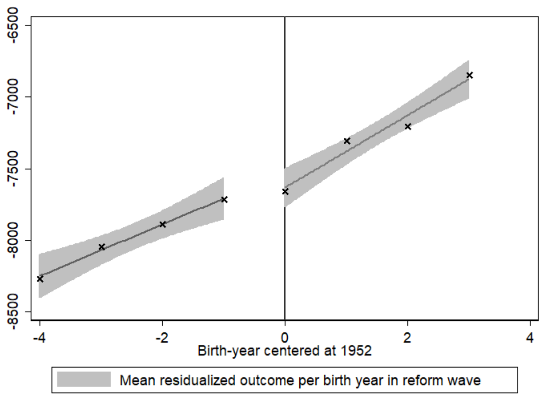
(c) Singles

Figure A.2: Event study plots: savings rate

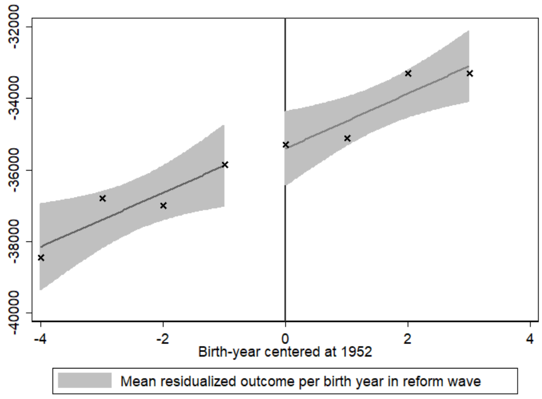
Notes: Figure A.2 presents the event study coefficients by survey year for the full sample, married households and single households. The results are obtained using the baseline sample of cohorts from 1948 to 1955.



(a) Full sample



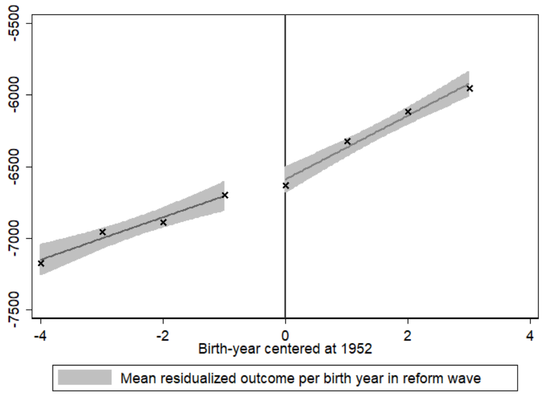
(b) Couples



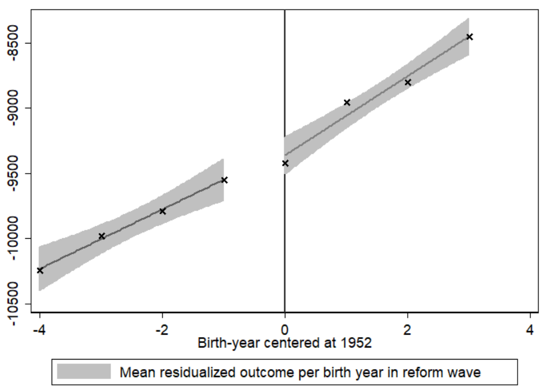
(c) Singles

Figure A.3: Residualized equivalized individual savings level by marital status

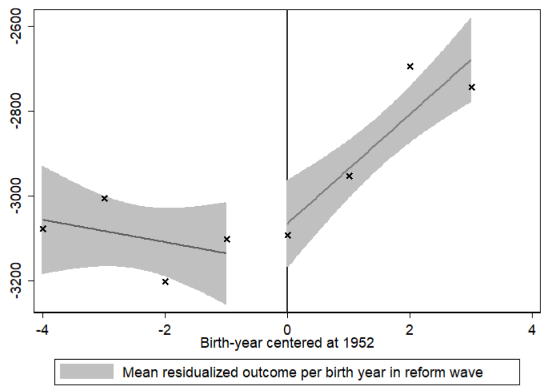
Notes: Figure A.3 presents graphic evidence on the relationship between birth year and the residualized individual monthly savings level in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform 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



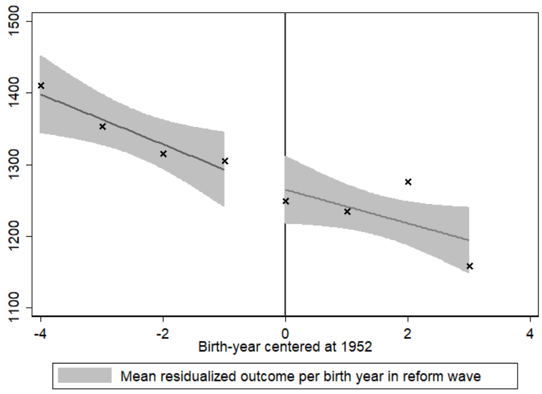
(b) Couples, level



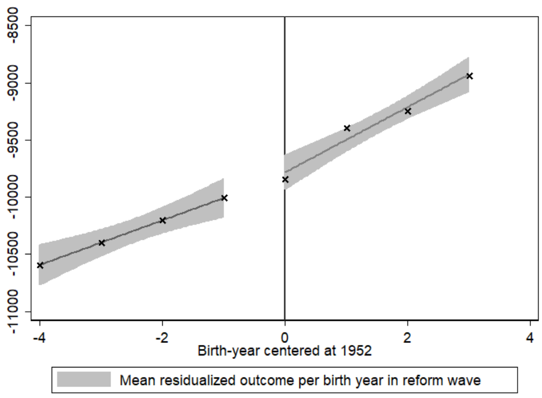
(c) Singles,level

Figure A.4: Residualized monthly equivalized household disposable income by marital status

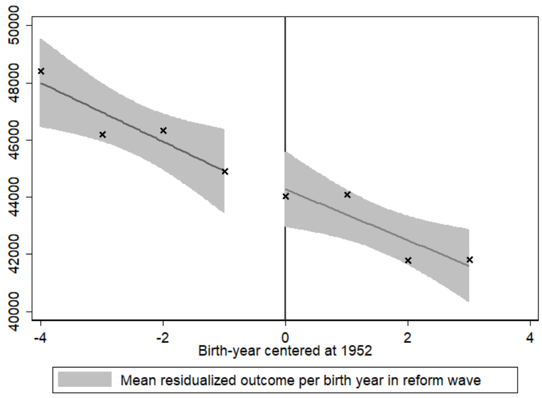
Notes: Figure A.4 presents graphic evidence on the relationship between birth year and the equivalized monthly disposable income in the post-reform periods for the full sample, couple households and single households. The estimated coefficients to obtain a residualized saving rate in the post-reform 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.5: Residualized monthly equivalized consumption expenditures by marital status

Notes: Figure A.5 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

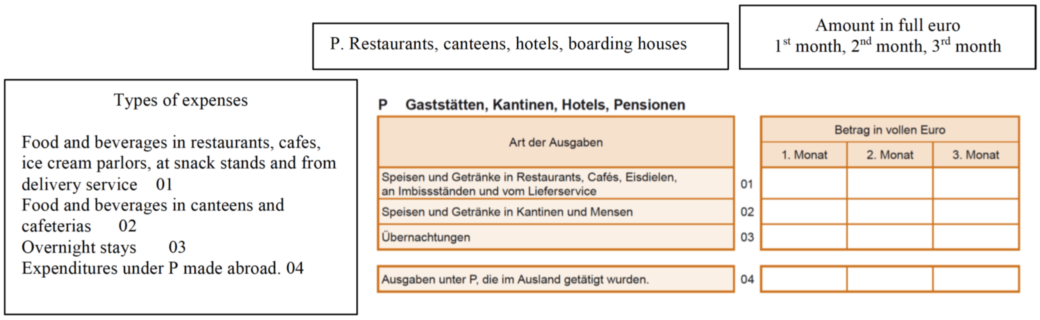
B.1. The Sample Survey of Household Income and Expenditure (EVS)

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.


P. Restaurants, canteens, hotels, boarding houses

Amount in full euro
1st month, 2nd month, 3rd month

Types of expenses

Food and beverages in restaurants, cafes, ice cream parlors, at snack stands and from delivery service 01
Food and beverages in canteens and cafeterias 02
Overnight stays 03
Expenditures under P made abroad. 04

P Gaststätten, Kantinen, Hotels, Pensionen

Art der Ausgaben		Betrag in vollen Euro		
		1. Monat	2. Monat	3. Monat
Speisen und Getränke in Restaurants, Cafés, Eisdielen, an Imbissständen und vom Lieferservice	01			
Speisen und Getränke in Kantinen und Mensen	02			
Übernachtungen	03			
Ausgaben unter P, die im Ausland getätigt wurden.	04			

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 assess 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 given by

[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?)

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.

SHARE offers specific information on the lives on elder individuals. 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 [3] and born between 1947 and 1956 (five years before after the cut-off). We use raw information 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 1, 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*[4], the *Wachstums- und Beschäftigungsförderungsgesetz 1996*[5], the *Rentenreformgesetz 1999*[6], and the *RV-Nachhaltigkeitsgesetz 2004*[7]. 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

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

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 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 did not 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 the 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.

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.

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 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. Additional Robustness Checks

Here we present further robustness checks performed to establish the causality of the point estimates from the main specification.

D.1. Placebo Tests: Older Cohorts

Theoretically, it is possible that the main effects we estimate in the baseline are not differences in savings rates between the cohorts in question but rather age-driven effects. Looking at the two reform waves (2003 and 2008) and given the main cohort bandwidth (women born between 1948 and 1955) we compare women of at least 51 years of age (born after 1952) with younger women (born until 1952) in 2003 and women of at least 55 years of age (born from 1952 onward) with younger women (born until 1951) in 2008. To make sure that we do not mistakenly interpret differences between the age-groups as cohort effects we perform placebo estimations comparing the same age groups in earlier waves.

We first 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).[8] 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.16 shows the effects using the pooled placebo sample for the full sample, couples, and singles. Panels 2 and 3 of Table A.16 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.17 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

[8] 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.

face some financial penalties in claiming the old-age pension at age 60.[9] This can mean that 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.

Table A.16 and Table A.17 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.

D.2. 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.18 shows that a significant changes in savings rates only for single men in the control waves.[10] 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 cut-off. 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.

E. Additional Details

E.1. 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.

[9] 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.

[10] Table A.19 and Table A.20 show the DD and event study results using households with men born between 1948 and 1955. We also find no significant changes.

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 [11], 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 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, where E is age at exiting employment, is the following:

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

, 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

$$TotalPB = (T - T^R)\frac{AR}{\bar{w}}(T^E - s) = \rho w(T^E - s)(T - T^R)$$

, where ρ is the replacement rate per year of pension contribution on net wage. 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.

E.2. Details on the Discounted Lifetime Income

Here, we explain how we calculate the discounted lifetime income in Figure 1. In Figure1, we show the changes in discounted lifetime income at age 50 for a person plan to exit employment at age E and claim pension at age R. We compare four scenarios with a baseline case when a stylized woman who would retire and claim pension at age 60 (cohorts born before 1952). These four scenarios are she stops working at age 60, 61, 62 and 63 while claim pension at age 63. We make the following assumptions: women earn a constant (after tax) monthly wage w of €2400. They accumulate 0.85 earnings points on average for an additional year of working.

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

Each earnings point is converted to €30 when they retire. At age 60, they have contributed to the pension system for 35 years. Their life expectancy at age 60 is around 25 years. These assumptions are based on statistics from the Federal Statistical Office of Germany. The discount rate r is set to 0.03.

The annual pension benefits for a woman who retire at age E and claim pension at age R is the following:

$$y^{pb}(E,R) = 0.85 * 30 * (35 + (E - 60)) * (1 - AF^R) * 12$$

, where AF^R is the adjustment factor for claiming pension early. While individuals could continue to retire at the ERA, they face an actuarial adjustment in the form of a 0.3% pension reduction per each month they retired in advance of the FRA. In the baseline case, $y^{pb}(60,60) = 0.85 * 30 * (35) * (1 - 0.18) * 12$. In the four scenarios, $y^{pb}(E,63) = 0.85 * 30 * (35 + (E - 60)) * (1 - 0.072) * 12$. The discounted pension wealth at age 50 is:

$$\textit{Basline case, retire and claim at 60: } Y^{pb}(60,60) = \sum_{t=1}^{25} (\frac{y_{pb}}{(1+r)^{60+t-50}}$$

$$\textit{After reform, retire at E and claim at 63: } Y^{pb}(E,63) = \sum_{t=1}^{22} (\frac{y_{pb}}{(1+r)^{63+t-50}}$$

The annual future labor earnings y_{le} is again constant and set to €28800 (€2400 *12). Therefore, the discounted lifetime income at age 50 for individuals retire at age E is

$$Y^{le}(E) = \sum_{t=50}^{59} \frac{28800}{(1+r)^{t-50}} + \sum_{t=60}^{E} \frac{28800}{(1+r)^{t-50}}$$

E.3. Calculating the Impact of Savings and Retirement Timing Responses over the Life-cycle

We calculate the impact of the pension reform on pension income, savings and consumption in retirement and in employment via updated retirement planning. First we calculate agent' pension benefits and the impact of updated retirement planning. The German pension formula is as follows:

$$PB_i = PV * PP_i * DD$$

Pension benefits PB_i depend on the number of pension points PP_i collected during the working life, the year specific pension value PV and deductions of pension benefits DD that depend on the timing of retirement. To keep the model simple we assume a pension value of 30, meaning that every pension point is worth 30€of pension benefits.[12] Deductions are calculated regarding the normal retirement age of 65 for agents in the sample and the cohort and gender-specific expected retirement age as estimated from SHARE data (see Section 3). In line with section E.2 we assume that an average woman has collected 29.75 pension points at age 60. An average year of labor increases her pensions points by 0.85. We use these numbers to calculate the number of pensions points for agents before the reform $(29.75 + (ExpRetage_i^{before} - 60) * 0.85)$ and after the reform $(29.75 + (ExpRetage_i^{after} - 60) * 0.85)$. Data for expected retirement ages $ExpRetage$ are taken from Table 1.

Consumption of agents while not yet retired C_{1i} is defined as

$$C_{1i} = (1 - SR_i) * I_i$$

, where I_i is household income per period and SR_i is the household savings rate. Income per period is average post-transfer income of men and women at age 50, gathered from GSOEP (German Socio-Economic Panel) data.[13] Cumulative savings are gathered over the period from age 50 until retirement CS_{ti} is calculated as follows:

$$CS_{ti} = SR_i * I_i * (ExpRetage_i - 50)$$

Average consumption in retirement C_{2i} is defined as pension benefits plus per-period dissaving DS_{ti}.

$$C_{2i} = PB_i + DS_{ti}$$

, where per-period dissaving is defined as:

$$DS_{ti} = \frac{CS_{ti}}{AGE_{death} - ExpRetage_i}$$

AGE_{death} is the average expected age at death (gender-specific).

Results reported in Section 7 are gathered by calculating consumption in both periods C_{2i} and C_{1i} given the changes in expected retirement ages.

[12] The pension value changes according the current average income of German citizens and is currently at 35.52€in West-Germany. See https://www.deutsche-rentenversicherung.de/SharedDocs/Glossareintraege/DE/A/aktueller_rentenwert.html

[13] For more information on the German Socio Economic Panel, see Goebel et al. (2019).