CESifo Working Papers

10420
2023

May 2023

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Mathias Dolls, Carla Krolage

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
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CESifo *Working Paper No. 10420*

‘Earned, Not Given’? The Effect of Lowering the Full Retirement Age on Retirement Decisions

Abstract

This paper analyzes behavioral responses to a 2014 reform in the German public pension system that lowered the full retirement age (FRA) of individuals with a long contribution history by up to two years and framed the new FRA as reference age for retirement. Using administrative data from public pension insurance accounts, we first document a substantial bunching response at the FRA exceeding the control group’s bunching by 83%. Second, we show in a difference-in-difference setting that a 1.0 year decrease in the FRA leads to a reduction in the average pension claiming age by 0.3-0.4 years. Treated individuals neither have poorer health nor are more likely to be liquidity-constrained than individuals in the control group. Our results suggest that the strong responses to the reform are driven both by the new FRA serving as a reference point and by financial incentives. Estimated fiscal costs of the reform are at the upper end of the range of previous back-of-the-envelope calculations.

JEL-Codes: H550, J140, J180, J260.

Keywords: retirement age, early retirement, pension reform.

Mathias Dolls
ifo Institute – Leibniz Institute for Economic Research at the University of Munich
Munich / Germany
dolls@ifo.de

Carla Krolage
ifo Institute – Leibniz Institute for Economic Research at the University of Munich
Munich / Germany
krolage@ifo.de

May 2023
This is a revised version of ifo Working Paper No. 291, “The Effects of Early Retirement Incentives on Retirement Decisions.” We are grateful to the editor (Andrea Weber), an anonymous referee, as well as Johannes Geyer, Matthias Giesecke, Holger Lüthen, Andreas Peichl, Arthur Seibold, David Sturrock, Han Ye, and conference and seminar participants at Bocconi University (Dondena Workshop on Public Policy), ifo Institute (CESifo Area Conference on Employment and Social Protection), Ruhr-Universität Bochum (RUB), the Institute for Employment Research (IAB), University of Glasgow (IIPF), University of Manchester (EEA) and University of Leipzig (VfS) for helpful comments and suggestions. The results and their interpretation are solely the authors' responsibility. This paper uses confidential data from the German Public Pension Insurance. The data can be obtained by filing a request directly with the Research Data Centre of the German Public Pension Insurance. In addition, the paper uses data from SHARE Waves 5-8. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. Erik Faller provided excellent research assistance.

1 Introduction

Pension systems around the world face aging populations and demographic change, putting increased pressure on fiscal sustainability. Against this background, many countries have conducted pension reforms aimed at prolonging the working lives of the elderly population (OECD 2017). A key goal of reforms raising statutory retirement ages or introducing actuarial deductions for early retirement is to prevent increases in life expectancy from disproportionately extending the period of pension benefit receipt. However, an often-voiced argument against these reforms is that certain groups who have worked hard throughout their working life, i.e., individuals with long contributions histories, should be exempted from increases in the normal retirement age (NRA) or the early retirement age (ERA). Advocates of this idea argue that these groups should be treated differently as they have contributed to society throughout their lives. Along these lines, a major pension reform in Germany in 2014 sharply increased early retirement incentives for individuals with long working lives by reducing the age at which this group can claim their full pension by up to two years. This paper is the first to study the behavioral responses and fiscal effects of this pension reform in Germany.

The reform of the so-called *old-age pension for the especially long-term insured* became effective in July 2014 and allowed individuals with at least 45 years of qualifying pension contributions to retire without deductions as early as age 63. In the years before the reform, this was only possible at age 65, i.e., the reform implied a significant shift in the full retirement age (FRA).[1] The reform not only changed financial incentives for eligible individuals, but the new FRA of 63 was also framed as a reference age for retirement. The Federal Ministry of Labour and Social Affairs launched a large-scale publicity campaign introducing the new scheme as '*retirement at 63*' which is by now the name the old-age pension for the especially long-term insured is commonly known as. This was accompanied by the key campaign message '*earned, not given*', which was displayed on billboards across Germany. In the eight years following the reform, the 'retirement at 63' scheme has been the most important pathway into early retirement in Germany. Overall, it is the second most common pathway towards retirement, with on average 24% of new retirees claiming old-age pensions through this scheme since the 2014 reform, as compared to 34% of retirees retiring at the NRA (Deutsche Rentenversicherung Bund 2022).

[1] For individuals in younger birth cohorts who are eligible to the old-age pension for the especially long-term insured, the FRA increases by 2 months per year until reaching the age of 65 for the 1964 birth cohort.

We assess responses to this reform based on high-quality administrative data on pension claimants from public pension insurance accounts. We first present stylized lifetime budget constraints illustrating how the reform has changed financial incentives to retire early. In our main analysis, we investigate behavioral responses to the reform making use of two empirical strategies. First, we estimate bunching responses among individuals who are eligible for the new 'retirement at 63' scheme (treatment group) and compare bunching in the treatment group to the retirement behavior of ineligible individuals who just miss the required 45 contribution years before reaching the NRA by a short margin (control group). The latter group faces an ERA of 63 at which they can retire early with deductions (before and after the reform). Second, we employ a difference-in-difference approach to estimate the effect of the reform on the retirement entry and the labor market exit age, respectively. Finally, our study provides the first fiscal costs estimates of the reform taking into account actual behavioral responses of eligible individuals.

The paper contributes to the literature on the effects of pension reforms changing statutory retirement ages. A large empirical literature studies reforms which increase the NRA or the FRA (see Mastrobuoni 2009, Behaghel and Blau 2012, Atalay and Barrett 2015, Seibold 2021, among others) or the ERA (see Staubli and Zweimüller 2013, Manoli and Weber 2018, Rabaté and Rochut 2020, Geyer and Welteke 2021, among others). These studies find substantial labor market effects, albeit at varying magnitudes. In addition to increasing employment and an upward shift in pension claiming ages, some of the papers also find evidence for program substitution towards unemployment insurance.

In contrast to these studies, our paper assesses a reform that reduced the FRA for allegedly deserving individuals with long contribution histories. Evidence on such reforms is much more scarce. Previous research suggests that reforms granting the possibility to retire early lead to a reduction in the average retirement entry age (Börsch-Supan and Schnabel 1998; Baker and Benjamin 1999; Vestad 2013). Whether responses to a decrease in the FRA are symmetric to the effects of an increase in the FRA is an empirical question, though, which this paper aims to shed light on. Structural models of labor supply and retirement behavior account for factors such as liquidity constraints and health, which may explain why people claim pension benefits at the earliest possible date despite low or even negative financial incentives to do so (see for example Gustman and Steinmeier 2005, French 2005, and Blundell et al. 2016 for an overview). If liquidity constraints and poor health are more prevalent among treated individuals with long contribution histories, responses to a decrease in the FRA may indeed be stronger than to an increase. We show that this is unlikely to be the case in our setting.

We also contribute to the recent literature on reference dependence and social norms in retirement behavior: Using bunching analysis, Seibold (2021) finds that retirement patterns cannot be explained by financial incentives alone. Rather, framing statutory ages as reference ages results in increased retirement probabilities at these thresholds. Likewise, Behaghel and Blau (2012), Cribb et al. (2016) and Gruber et al. (2022) find behavioral responses to reforms of statutory/early retirement ages that entail no or only limited financial incentives.[2] The reform studied in this paper combines a reduction in the FRA, giving rise to strong financial incentives to retire early, with a framing of the age 63 as new reference age for retirement for the long-term insured.

Our results can be summarized as follows. We find that bunching in the treatment group exceeds the control group's bunching by 83%. Observed elasticities at the FRA range between 0.34–0.85, with larger elasticies found for younger cohorts who had more room to antedate pension claiming and hence to take advantage of the new early retirement pathway. Our elasticities for younger cohorts are larger than the observed elasticities at the FRA reported by Seibold (2021) for older cohorts not affected by the reform studied in this paper, possibly due to the fact that the framing of age 63 as new reference age for retirement for the long-term insured was very salient. They are much larger than estimates based on pure financial incentives (Brown 2013; Manoli and Weber 2016), pointing at the important reference point effect of the FRA. For individuals who only become eligible for the 'retirement at 63' scheme at some point between the FRA and the NRA, we find both a spike at their ERA of 63 and at the contribution threshold, with the spike at 63 becoming relatively larger the more the contribution threshold moves towards the NRA.

In line with the results from the bunching analysis, our difference-in-difference estimates reveal that the treatment group antedates retirement entry by 7-8 months on average. A corresponding effect is found for the labor market exit age. Results do not significantly differ between men and women, but are larger for non-working than for working individuals. Our estimates suggest that a 1.0 year decrease in the FRA reduces the average pension claiming age by 0.3-0.4 years. This effect size is of similar magnitude as estimates in previous studies analyzing the effect of increases in statutory retirement ages framed as reference points (see e.g. Mastrobuoni 2009, Staubli and Zweimüller 2013, Manoli and Weber 2018).

We provide suggestive evidence based on the *Survey of Health, Ageing and Retirement in Europe* matched to administrative records from public pension insurance accounts (SHARE-RV) that individuals eligible for the 'retirement at 63'

[2] Rabaté (2019) shows that labor demand-induced job-exits as a consequence of mandatory retirement can explain part of the bunching at references ages.

scheme neither have poorer health nor are more likely to be liquidity-constrained than individuals in the control group. These findings may rationalize a symmetric response to a decrease as compared to an increase in the FRA, and provide further evidence that the strong responses to the reform are driven by the new FRA serving as a reference point and by financial retirement incentives.

Our estimates of the fiscal impacts of the reform indicate additional pension insurance expenditures of 9 billion euros and aggregate fiscal costs of 18 billion euros between 2014 and 2017. The latter estimate corresponds to 1.8% of overall public pension expenditures over the same period. These estimates are at the upper end of the range of previous back-of-the-envelope projections. They also exceed projected costs assumed in the draft government bill by more than 2 billion euros for the period under consideration. Importantly, with our empirical approach we identify the short run effects of the reform, which is why our fiscal costs calculations should be interpreted with caution. In the longer term, people have more leeway for adjusting their labor market careers in order to become eligible for the 'retirement at 63' scheme. This will likely increase the number of people who can retire early without deductions.

The paper proceeds as follows. Section 2 describes the institutional background of the German pension system and the 2014 early retirement reform. In section 3, we provide information about the data sources used in subsequent analyses, the sample used for estimating behavioral responses, and first descriptive evidence. In sections 4 and 5, we present bunching and difference-in-difference results, respectively. Fiscal costs calculations are reported in section 6. Section 7 concludes.

2 Institutional Background

2.1 The German Public Pension System

Covering almost all private and public sector employees[3], the German statutory pension system provides old-age pensions as well as invalidity and survivors' benefits. Financed as a pay-as-you-go scheme, the calculation of pension benefits is based on a person's contribution history. Entitlements are calculated according to a point system, where the number of pension points is determined by the ratio of individual annual earnings to average earnings across contributors in the same year.[4] The

[3]Civil servants are exempt from the statutory pension system. While self-employed individuals in certain vocations, such as craftspeople, are covered by compulsory insurance, other self-employed individuals have the option to opt into public pension insurance.

[4]Contribution points are accumulated on a monthly basis, hence additional months of qualifying pension contributions always lead to an increase in pension benefits.

system also features certain redistributive properties, such as pension points for child raising.

In light of demographic tensions, the system has seen numerous reforms in recent years. Most of these reforms focused on increasing the Normal Retirement Age (NRA) or restricting pathways for accessing retirement. Most notably, the NRA has been increasing stepwise from 65 to 67. Retiring earlier is possible through several early retirement schemes, but usually requires deductions of 0.3% per month of retiring early.[5] The accessibility of schemes depends on the birth cohort and the insurance record, notably on the number of years of qualifying pension contributions. In addition to periods spent in employment, these also include periods spent raising children, voluntarily contributing or, under certain conditions, receiving unemployment benefits.

Retiring at the Early Retirement Age (ERA) of 63 with deductions is possible for those with at least 35 contribution years. Severely disabled individuals face both a lower NRA and a lower ERA. For those born prior to 1952, two additional pathways were possible. Women with at least 15 contribution years, 10 of which have been spent actively contributing after age 40, may retire as early as age 60, but face deductions for each month of early retirement. Likewise, retiring at an earlier age is also possible after unemployment or partial retirement (*Altersteilzeit*) or in case of disability.

Table 1 shows the respective NRA, ERA and the Full Retirement Ages (FRA) by cohort. The FRA is the age at which individuals with at least 45 contribution years are able to retire without deductions (see Section 2.2 for details on the reform of the *'old-age pension for the especially long-term insured'*). We also list the deductions a person faces when retiring at the ERA of 63. As is shown in the last row of Table 1, deductions for retiring at 63 with less than 45 years of pension contributions are increasing across cohorts. This is due to the rise in the NRA. For a person born in 1950, for example, the difference between the ERA (63) and the NRA (65+4) amounts to 28 months. As pension benefits are reduced by 0.3% for each month of early retirement, deductions at 63 amount to 8.4% for the 1950 cohort. In contrast, a person born in 1954 faces deductions of 9.6% when retiring at the ERA of 63, as for this cohort the difference between the ERA (63) and the NRA (65+8) has grown to 32 months.

[5] There is a reward of 0.5% per month of retiring after the NRA.

Table 1: Normal, Full and Early Retirement Ages by birth cohort

	1948	1949	1950	1951	1952	1953	1954
NRA	65+2	65+3	65+4	65+5	65+6	65+7	65+8
FRA (pre-reform)[a]	65	65	65	65	65	65	65
FRA (post-reform, born Jan.-June)[a]	65	65	64+1...6[b]	63+1...6[b]	63	63+2	63+4
FRA (post-reform, born July-Dec.)[a]	65	64+7..11[b]	63+7..11[b]	63	63	63+2	63+4
ERA	63	63	63	63	63	63	63
ERA (women)	60	60	60	60	-	-	-
Deductions at ERA[c]	7.2%	∼ 8%	8.4%	8.7%	9.0%	9.3%	9.6%

Notes: This table shows Normal, Full and Early Retirement Ages in years + months for the main pathways towards retirement. For example, '65+2' refers to 65 years and two months. NRAs and FRAs continue to increase up to the 1964 birth cohort. Early retirement schemes other than 'retirement at 63' typically require deductions of 0.3% per month. In addition to the depicted retirement schemes, the 'retirement after unemployment' and 'retirement for severely disabled' schemes allow specific demographic groups to retire early.
a: FRA: age at which individuals with 45 years of pension contributions at their 63rd birthday are able to retire without deductions before and after the reform of the *'old-age pension for the especially long-term insured'* on 1 July 2014.
b: For individuals born between 2 July 1949 and 30 June 1951, the reform of the *'old-age pension for the especially long-term insured'* became effective after reaching the post-reform FRA of 63, but before reaching the pre-reform FRA of 65. They face an effective FRA in between the pre- and post-reform FRA.
c: Deductions when retiring at the ERA of 63 with less than 45 contributory years, not considering foregone benefits due to the shorter contribution period. For birth cohorts 1950 onwards, the reference age for calculating deductions corresponds to the NRA. For birth cohort 1948 (1949), the reference age is 65 (65+1..3, depending on the birth month).

2.2 The 2014 Early Retirement Reform

The reform of the *'old-age pension for the especially long-term insured'* went into effect on 1 July 2014. It effectively reduced the FRA for individuals with at least 45 contribution years by up to two years (see rows 2-4 in Table 1). Before the reform, eligible individuals were able to retire with full pension benefits (i.e., without deductions) at age 65. After the reform, eligible individuals born between 1 July 1951 and 31 December 1952 were able to do so at age 63. For individuals born between 2 July 1949 and 30 June 1951, the reform became effective after reaching the post-reform FRA of 63, but before reaching the pre-reform FRA of 65. This means that depending on their age on 1 July 2014, the reform reduced their effective FRA by 1-23 months as shown in rows 3-4 in Table 1. For the birth cohorts 1953 onwards, the reduction in the FRA is also less than 2 years since the FRA increases stepwise by 2 months per birth cohort until reaching the age of 65 for the 1964 birth cohort. A further element of the reform was a broadening of eligibility criteria by counting additional periods towards the 45 contributory years, in particular periods of unemployment benefit receipt and periods of voluntary contributions to public pension insurance. This has enlarged the pool of potential recipients of the old-age pension for the especially long-term insured.

Since the passing of the reform, the old-age pension for the especially long-term insured is commonly known as the 'retirement at 63' scheme. Through requiring 45 contribution years, it is targeted at skilled workers with stable employment biographies who typically entered the labor market through a vocational training before age 18.[6] The reform was part of a substantial and very salient retirement reform which also increased pensions for mothers of children born before 1992 and increased invalidity benefits. These other aspects of the reform should not confound our analysis. Invalidity benefits are commonly drawn at a much earlier age and with much fewer contributory years than required by the 'retirement at 63' scheme. While invalidity benefits play a very small role in our sample, we nevertheless exclude all individuals drawing invalidity benefits (see section 3.2). Moreover, our analysis accounts for individual characteristics (see section 5). Any potential income effect of mothers' increased pensions should thus not exert a differential effect on control and treatment groups.

The Federal Ministry of Labour and Social Affairs dedicated a publicity campaign to the retirement reform in January 2014[7], claiming that hard-working individuals with long contribution histories close to retirement had benefited less from improvements in working conditions than younger cohorts. The reform was meant to reward these workers for having stabilized public pension insurance through regular contribution payments over the last decades (Deutscher Bundestag 2014a). Figure A.1 in the Appendix shows that the reform was announced as a package that would *'close important fairness gaps and better acknowledge lifetime achievement'.* It also shows the key campaign message (*'earned, not given'*) that was later displayed on billboards throughout Germany. By referring to the reform as 'retirement at 63', the overall reform is framed as a change in the FRA, with the age of 63 being presented as new reference age for retirement. The reform was criticized for breaking the equivalence principle, which states that equal pension contributions within a year should lead to the same pension entitlement.[8] A further criticism was that it would entail adverse distributional effects because it was expected that it would benefit workers with stable employment biographies and above-average pension entitlements (Börsch-Supan et al. 2015).

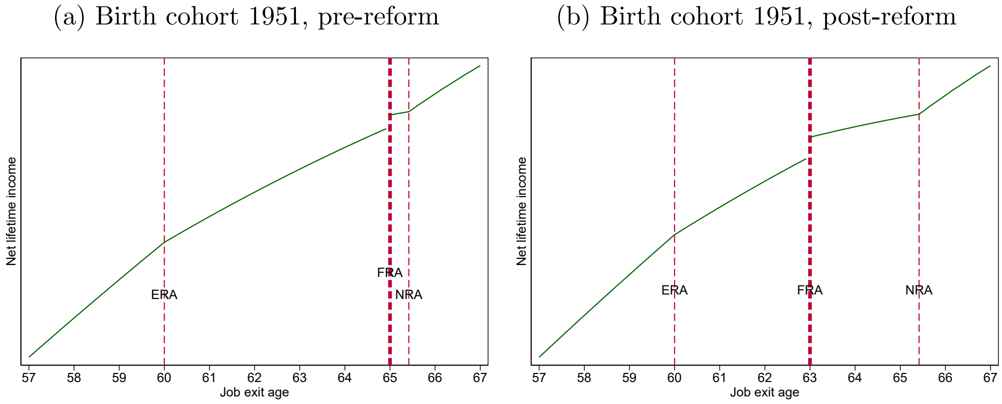
The reduction in the FRA substantially changes financial incentives, which we illustrate by depicting stylized lifetime budget constraints (c.f. Seibold 2021). Figure 1 shows the lifetime budget constraint for a woman with average lifetime

[6]Vocational training is fully credited as a contributory period, both before and after age 18.

[7]The reform was the first large project by the new grand coalition government that had formed at the end of 2013. It was first announced in the coalition agreement in mid-December 2013, a first legislative draft was discussed in January 2014 and the final law was passed in May 2014.

[8]Deductions of 3.6% annually are less than actuarially fair (Börsch-Supan and Wilke 2004). By abolishing these deductions for the long-term insured with 45 contribution years at the new FRA, the reform has led to imbalances between pension contributions and benefits claimed across eligible and non-eligible individuals.

Figure 1: Stylized lifetime budget constraints



Notes: This figure shows a stylized lifetime budget constraint for a women with average earnings born in 1951 before and after the introduction of the 'retirement at 63' scheme. The slope of the budget constraint indicates the gain in net lifetime income through delaying the labor force exit (and, from reaching the ERA onwards, retirement entry) by one month. Kinks and notches are displayed above scale to improve visibility. Figures for men showcase a higher ERA, as men are not eligible for the 'retirement for women' scheme, but are similar otherwise. Special cases for severely disabled individuals are not depicted here, but accounted for in the later analysis.

earnings born in 1951 who has completed 45 contribution years at age 63. Panels (a) and (b) depict her lifetime budget constraints before and after the reform. The first kink is at the ERA of 60 when she can retire early through the 'retirement for women' scheme, but with deductions of 0.3% per month. Before the reform, there is a small notch at her FRA of 65 where she can get her full pension five months before the NRA. This notch arises because she can avoid deductions of 1.8% one month before turning 65 by working one month longer and retiring at the FRA of 65 (income effect). Moreover, the slope of her budget constraint becomes smaller after the FRA (substitution effect). Prior to reaching the FRA, she can increase her lifetime budget through two channels when working longer: (i) through higher labor income and higher associated pension contributions and (ii) through lower deductions on the pension. Upon reaching the FRA, channel (ii) becomes void. The slope of her lifetime budget constraint increases again after reaching the NRA as individuals working longer than the NRA are entitled to retirement benefit supplements of 0.5% per month of working past the NRA. As shown in panel (b), the reform shifts her FRA to 63 and the notch at the FRA becomes larger. This notch arises due to switching retirement entry pathways from early retirement with deductions to the 'retirement at 63' scheme. Now, she can avoid deductions of 9% one month before turning 63 by working one month longer and retiring at the new FRA of 63.

3 Data and Descriptive Evidence

3.1 Public Pension Insurance Accounts

For our main analysis identifying the causal effect of the early retirement reform on retirement choices, we employ high-quality administrative data on pension claimants from public pension insurance accounts (*Versichertenrentenzugang 2008-2020*). The scientific use file contains a 10% random sample of all individuals entering retirement between 2008 and 2020. As the dataset is process-produced, it mainly contains variables needed for calculating pension entitlements. Amongst others, we observe personal characteristics such as gender, marital status, education level and region of residence, as well as variables on the contribution history and retirement. These include the exact retirement entry age in months (i.e., the pension claiming age), the chosen retirement scheme, pension points, i.e., accumulated pension contributions, and pension-relevant periods, which enable us to determine eligibility for the 'retirement at 63' scheme. The dataset provides further details on the three years preceding retirement, such as the respective social insurance status and the annual salary.

We supplement our analysis with results from the *Survey of Health, Ageing and Retirement in Europe (SHARE-RV)*, a cross-national panel survey with a focus on the middle and old-age population. The German survey can be linked to administrative records from public pension insurance accounts (*Versichertenkontenstichprobe*). We exploit this feature and compare self-reported physical health conditions, the presence of liquidity constraints and of private retirement savings between individuals eligible to the 'retirement at 63' scheme and those who fail to become eligible by a short margin.

3.2 Sample for Estimating Behavioral Responses

Our sample includes individuals with a long contribution history of 40 to 47 contribution years at age 63. We focus on birth cohorts 1948-1954 as they are included in the 2008-2020 sample if they retire anytime between age 60 and age 66. This implies we cover retirement entries between the earliest possible ERA under some schemes such as 'retirement for women' and (shortly after) the NRA (see Table 1). We exclude all other birth cohorts as they are only partially covered in the sample.

While all individuals in this group are able to retire with deductions at age 63, their eligibility for early retirement without deductions through the 'retirement at 63' scheme differs. This allows us to identify a treatment (control) group of individuals who are affected (unaffected) by the reform. We exclude individuals with previous retirement spells, and individuals entering invalidity benefits (*Erwerbsminderungsrente*). Individuals who spent part of their career abroad, receive pensions according to the Foreign Pension Law or are subject to transitory regulations are likewise dropped from the sample. The overall sample contains roughly 138,000 individuals. This corresponds to slightly more than a quarter of all retirees in the data as shown in the last row of Table 2.

Table 2 further presents summary statistics for the treatment and control group of our analysis. The treatment group contains individuals who have accumulated 45-47 contribution years at age 63 and hence are immediately eligible for the 're-tirement at 63' scheme once the reform is passed. The control group is composed of ineligible individuals with 40–42.33 contribution years at age 63. These individuals cannot attain 45 contribution years before reaching the NRA. We contrast this with summary statistics for the full sample of individuals retiring through an old-age pension, irrespective of the number of contribution years (third column of Table 2).

Table 2: Public pension data: Summary statistics

	Treatment group	Control group	All retirees
Female	49.9%	56.1%	53.5%
Married	74.7%	71.0%	73.5%
East German	30.7%	24.2%	18.1%
Education			
None	6.8%	7.8%	6.4%
Vocational degree	61.9%	35.0%	38.8%
Advanced occupational degree	4.3%	2.3%	2.7%
University degree	2.2%	17.4%	6.7%
Unknown	24.9%	37.4%	45.5%
Labor market status before turning 63			
Employed	50.5%	35.9%	35.3%
Marginally employed	3.8%	8.6%	7.6%
Voluntarily insured	3.4%	3.0%	2.9%
Short-term unemployed	8.0%	6.2%	5.0%
Receiving other social benefits/credit period	7.7%	18.4%	11.2%
Partial/Employer-sponsored early retirement	23.0%	14.5%	13.3%
None/ unknown	3.7%	13.4%	24.7%
Pension points at 63	43.8	39.2	33.9
Number of observations	98,363	39,353	504,771
Fraction of total sample in respective years	19.5%	7.8%	100%

Notes: This table shows summary statistics for the public pension insurance accounts sample for birth cohorts 1948-1954 in the sample years 2008-2020, based on own calculations. The first two columns indicate summary statistics for individuals in the treatment and control groups used in our estimations. The third column shows summary statistics for all retirees in birth cohorts 1948-1954, regardless of contribution years. All other sample restrictions, such as the exclusion of individuals entering invalidity benefits, apply.

The following differences stand out between groups. First, individuals in the treatment group (and to some extent in the control group) are more frequently East German than the average of retirees, which is partly attributable to the more consistent employment biographies of East German women, resulting in higher contribution years. Second, individuals in the treatment group more frequently have a vocational degree, while individuals in the control group more often graduated university. In order to amass 45 contribution years at age 63, one has to start paying social insurance contributions at age 18, which is the case for individuals with vocational training.[9] In contrast, university graduates tend to start contributing later, which is in line with the higher share of university degrees in the control group. Third, in terms of the labor market status at age 63, workers eligible for the 'retirement at 63' scheme are more frequently employed and more often exit the labor market through partial or employer sponsored early retirement, whereas individuals in the control group more frequently receive social benefits or derive contributory years from credit periods, e.g., through a sickness leave. Finally, individuals in treatment and control group both have an above-average number of pension points accumulated at age 63 due to their long and stable employment biographies.

3.3 Descriptive Evidence

Figure 2 presents average retirement entry ages for men and for women in our treatment and control group, respectively. It serves as a first graphical representation of the difference-in-difference analysis presented later in the paper (see section 5). For men, retirement entry ages in the pre-reform cohort 1948 and in the cohort 1949, the oldest cohort partially affected by the reform if an individual was born after July 1, are almost identical. The gap between retirement entry ages of eligible and ineligible men gradually widens for birth cohorts 1950–51 which had more room to antedate retirement entry when the reform went into effect. Birth cohorts 1952 onwards can retire immediately at the new (post-reform) FRA if eligible as they reach the new FRA only after the reform went into effect in July 2014. Men in this group retire on average 6 months earlier than those in the control group.

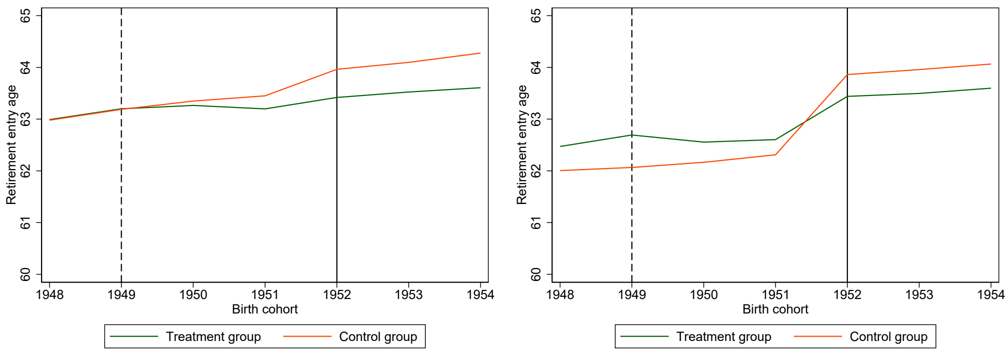
Initial differences for the pre-reform cohort 1948 and subsequent cohorts (1949–51) do arise for women, though: In these birth cohorts, women in the control group retire earlier on average than women in the treatment group. This effect is partly driven by a higher propensity of women in the control group to retire early through the 'retirement for women' scheme.[10] From birth cohorts 1952 onwards, retirement patterns reverse, with higher average retirement entry ages for women in the control group. These younger cohorts are affected both by the 2014 early retirement reform

[9]Note that in Germany, many individuals have started vocational training at age 15, contributing to social insurance at a rather young age.

[10]On average, 40% of women in the treatment group in birth cohorts 1948-1951 retire prior to age 63 through the 'retirement for women' scheme, while 49% of women in the control group do so.

Figure 2: Average retirement entry ages in treatment and control groups

(a) Men (b) Women



Notes: This figure shows average retirement entry ages by birth cohort for men and women in our control and treatment groups. The vertical lines indicate the first cohort that is partially affected by the reform of the 'retirement at 63' scheme (1949) as well as the first cohort for which the reform is fully effective (1952): Individuals born after July 1, 1949, may become eligible to the 'retirement at 63' scheme prior to reaching the NRA, but have reached the post-reform FRA already before the reform went into effect in July 2014. Birth cohort 1952 is the first full cohort reaching the new FRA only after the reform went into effect in July 2014.

and the abolition of the 'retirement for women' scheme. In later estimations, we account for characteristics such as social insurance status prior to the FRA, education or marital status to control for differences across control and treatment groups that may result in different retirement entry patterns, including differing preferences for the 'retirement for women' scheme (see section 5).

Table A.1 in the Appendix shows the fraction of retirees retiring through each retirement scheme in the full sample, regardless of their contribution years (cf. last column in Table 2).[11] Depending on the cohort, 35–40% of all retirees in our sample retire at the NRA. In birth cohorts who were younger than 63 when the reform was passed, 32–34% exit the labor market through the 'retirement at 63' scheme. The remainder retires early through another retirement scheme, in most cases facing deductions.[12] The share choosing to retire through one of the alternative retirement schemes declines across birth cohorts. First, two of these schemes – the 'retirement for women' and the 'retirement after unemployment or partial retirement' – were only accessible to pre-1952 birth cohorts. This leads to an increase in early retirement entries with deductions for birth cohorts 1952 onwards. Second, there is a substitution towards the 'retirement at 63' scheme from other retirement schemes.

[11]These fractions deviate from official statistics (Deutsche Rentenversicherung Bund 2022) which report the number of retirement entries by pathway over time rather than over birth cohort.

[12]The 'retirement for severly disabled' scheme does not entail deductions for individuals born before 1952 who retired at age 63.

In addition, Figure A.2 in the Appendix shows the distribution of retirement entry ages and retirement schemes for all birth cohorts in the full sample. It indicates a shift towards early retirement through the 'retirement at 63' scheme for birth cohort 1951 onwards, with spikes at the respective (cohort-specific) FRAs.

4 Bunching Estimation

4.1 Empirical Strategy

Drawing upon Saez (2010)'s, Chetty et al. (2011)'s and Seibold (2021)'s bunching analysis, we first assess retirement entry at the (post-reform) FRA in response to the reform (cf. Table 1). We quantify the emerging retirement spike at the FRA among eligible and non-eligible individuals by estimating an excess bunching mass above a counterfactual density of retirement entry ages. This is done by fitting a polynomial $h_0(\hat{R})$ to the observed density of retirement entry ages, excluding the FRA $\hat{R}$. We fit a joint counterfactual polynomial to the treatment and control group, later scaled with the fraction of observations in each group.[13] We calculate the excess mass $b = B/h_0(\hat{R})$ at the FRA as the multiple of the bunching mass relative to the counterfactual.

Following Seibold (2021), we then contrast the excess mass with the local percentage change in the implicit net of tax rate $\Delta\tau/(1-\tau)$ for the treatment group. This yields the following observed elasticity of retirement entry with respect to the net-of-tax rate:

$$\hat{\epsilon} = \frac{b/\hat{R}}{\Delta\tau/(1-\tau)} \tag{1}$$

As the FRA of 63 also serves as a common reference age for early retirement, calculating the excess mass at age 63 (or close to age 63) above the counterfactual density yields a combination of two effects: Bunching in response to the financial incentive at the FRA, and bunching at the reference age of 63 regardless of the reform. Similarly, bunching in the control group captures financial incentives at the ERA as well as the reference point character of the ERA. Hence, bunching masses and observed elasticities combine both responses to financial incentives and reference point effects, capturing effects beyond the response to the net-of-tax rate. As a result, observed elasticities, while informative about responses, overestimate

[13]This approach was chosen to ensure comparability between the bunching mass in the treatment and the control group. Comparable results emerge when fitting separate polynomials for each group rather than a joint polynomial for both.

the true, structural elasticities with respect to financial incentives alone. To assess retirement entry behavior following the reform in 2014, our main focus is hence on contrasting the excess mass in treatment and control group.

We estimate bunching for three distinct intervals: (i) $b(FRA\{-1,+1\})$ is estimated at a narrow interval around the FRA, stretching from the FRA - 1 month to the FRA + 1 month, as is typically done in the literature. (ii) Similarly, $b(FRA\{0,+2\})$ includes the period from the FRA to the FRA + 2 months. This specification encompasses the retirement entry ages of those who retire immediately at the FRA to those who retire with a delay of 2 months. As shown in section 4.2, we find some delayed responses, which could be a result of workers submitting the pension application or the notice of termination of the employment contract only upon reaching the FRA.[14]

(iii) The third bunching estimator, $b(63 - 63.5)$, captures a broader interval stretching from age 63 to age 63+6 months. While (i) and (ii) show bunching responses at the FRA, (iii) quantifies overall bunching at early retirement entry ages.[15] If more individuals retire between ages 63 and 63+6 in the treatment group relative to the control group, this indicates that, on average, individuals antedate retirement in response to the reform. This distinction is particularly relevant for birth cohorts 1953 and 1954, where the FRA exceeds the ERA by respectively 2 and 4 months. Bunching at the FRA is thus driven by two underlying effects: Individuals postponing retirement entry from the ERA of 63 to the FRA in response to financial incentives, and individuals choosing to retire earlier.

The bunching analysis focuses on cohorts 1951-1954 who reached age 63 after the reform was passed in July 2014, or shortly before, as is the case for some individuals born in 1951. Older cohorts face an effective FRA above age 63 which makes the comparison of bunching responses with the control group less informative. Section A.2 in the Appendix provides more details on the bunching methodology.

[14]In order to receive a public pension, one needs to submit a pension application. The German public pension insurance recommends to submit the application three months in advance in order to receive the pension at the time of retirement. With regard to the termination of employment contracts, the legal term of notice typically amounts to four weeks to the end of a calendar month. This implies that a notice of termination submitted upon reaching the FRA results in a delay of retirement entry of two months.

[15]Note that as the bunching method is usually used to detect local spikes, observed elasticities which are estimated over this rather wide range of 63 until 63+6 should be interpreted with caution. We hence focus on observed elasticities at the narrow intervals (i) and (ii).

4.2 Bunching Results

Figure 3 displays the results of the bunching analysis. Panels (a), (c), (e) and (g) show the distribution of retirement entry ages in the treatment group, while panels (b), (d), (f) and (h) show the distribution in the control group. There is a substantial excess mass at the FRA (ERA) in the treatment (control) group, indicated by a large spike in the distribution. As shown in Panels (e) and (g) for birth cohorts 1953 and 1954, eligible individuals bunch at the FRA (63+2 and 63+4), while only a very small spike can be observed at the ERA of 63. In contrast, bunching in the control group primarily occurs at age 63. This difference in retirement entry behavior between treatment and control group indicates that many eligible individuals, who would have retired at the ERA of 63 in the absence of the reform, postpone retirement to the FRA in response to financial incentives. At the same time, the treatment group's overall bunching mass $b(63 - 63.5)$ is much larger than the control group's bunching mass. Averaged across the four birth cohorts, bunching of the treatment group in this interval exceeds the control group's bunching by 83%. These results suggest that the reform has triggered strong additional bunching in early retirement. We explore this in more detail in the difference-in-difference analysis in section 5.

Observed elasticities $e(FRA\{-1, +1\})$, which range from 0.34 to 0.85 for birth cohorts 1952–1954, and bunching masses in the treatment group increase strongly across birth cohorts. Note that we do not observe the calendar month of birth in the data, i.e., we cannot distinguish whether individuals born in 1951 turn 63 before or after July 2014. Hence, for birth cohort 1951, we estimate only an upper bound of the net-of-tax rate and a lower bound of the elasticity, based on the assumption that all individuals attain eligibility at age 63. Correspondingly, by far the smallest elasticity is found for birth cohort 1951, which also had more retirement schemes at disposal (cf. section 2.1) and the least time to adapt retirement behavior. Elasticities for birth cohorts 1953–1954 who reached age 63 only after the reform went into effect and who hence had more room to antedate retirement entry are larger than the corresponding elasticity of 0.44 reported by Seibold (2021) for birth cohorts 1933-1944, possibly due to the salience of the 2014 reform.[16]

Especially for birth cohorts 1951–1953, we find a second spike two months after the FRA (ERA). This delayed response may be due to information frictions or inertia: Some individuals only submit their pension application or the notice of

[16]Note that we estimate the elasticity of pension claiming, rather than the elasticity of labor market exit, which Seibold (2021) and most of the literature estimate. As claiming may be more easily adjustable than labor supply, the elasticity of pension claiming might be naturally larger. However, our difference in difference analysis in section 5 reveals very similar effects for labor market exit ages and retirement entry ages (see Table 3), providing evidence for a similar effect size for labor market exit and retirement entry behavior.

termination of their employment contract once they reach the FRA (ERA). Accordingly, observed elasticites ($e(FRA\{0, +2\})$) which account for the second spike two months after the FRA are somewhat larger than elasticites $e(FRA\{-1, +1\})$.

Next, we analyze differences in retirement behavior between men and women and working and non-working individuals, respectively. The group of working individuals includes socially insured employees and the self-employed, non-working individuals are people with any non-working labor market status, such as drawing benefits. We exclude marginally employed individuals as well as individuals in partial or employer-sponsored early retirement as the distinction whether they are working or not is not as clear cut.[17] Figures A.3–A.4 in the Appendix reveal that overall patterns look very similar. Both men and women as well as working and non-working individuals exhibit stronger bunching in the interval $b(63 - 63.5)$ in the treatment group than in the control group. Within the respective treatment groups, we find elasticities for men and women being in the same order of magnitude, while elasticities for non-working individuals are larger than those for working individuals.

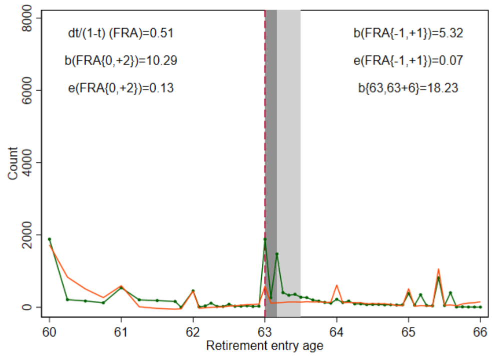
Finally, we investigate bunching at the ERA of 63 and at the effective FRA for individuals who reach eligibility to the 'retirement at 63' scheme only after age 63 due to the contribution threshold. Here, we focus on the birth cohort 1952 for which the ERA equals the FRA, but overall patterns are very similar for the other birth cohorts.[18] We split the sample into individuals who attain 45 contribution years at different ages between age 63+4 and 65+5. For these individuals, we both calculate the bunching mass $b(63, 63 + 2)$, and $b(FRA\{0, +2\})$ at the effective FRA. r depicts the ratio of both bunching masses. Figure A.5 in the Appendix shows that the magnitude of the bunching responses depends on the distance of the effective FRA to the NRA. Two observations are notable. First, we still observe bunching at the effective FRA even if it deviates from the statutory FRA. However, the more the effective FRA moves towards the NRA, the smaller becomes the bunching response. This is in line with expectations as a smaller gap between the effective FRA and the NRA implies smaller financial incentives to retire before the NRA. For those who reach eligibility precisely at age 65, the bunching response again becomes larger, which may be driven by the age of 65 being perceived as a reference age for retirement. Second, many of those who would become eligible past age 63 retire

[17]The reason behind this is as follows: Employees in partial or employer sponsored early retirement can typically choose whether they reduce working hours during their entire partial retirement, or whether they continue working full time initially and then cease working altogether during the second half of their partial retirement. We do not observe whether they are still working or whether they are in the non-working phase of partial retirement. While marginally employed individuals, who earn up to 450 Euros a month, are working some hours a month, they do not fully contribute to social insurance and could technically continue their marginal employment post retirement.

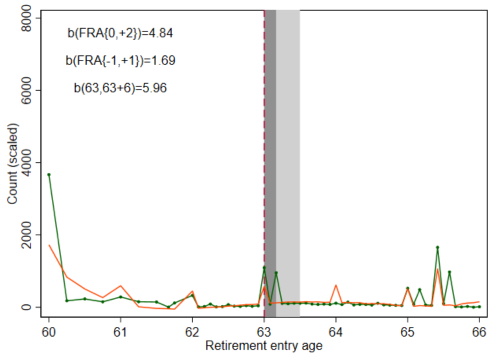
[18]Results are available upon request.

Figure 3: Bunching by birth cohort and eligibility

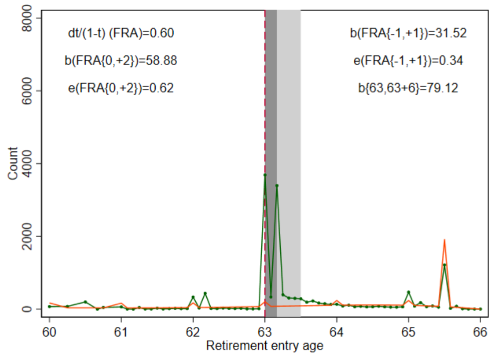
(a) 1951, treatment



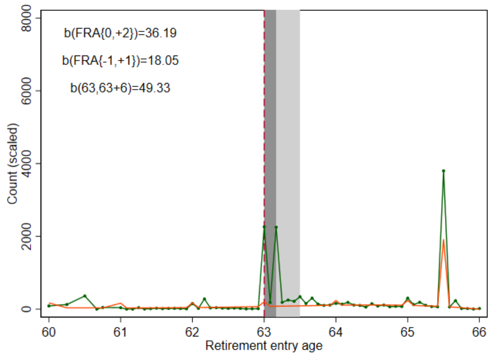
(b) 1951, control



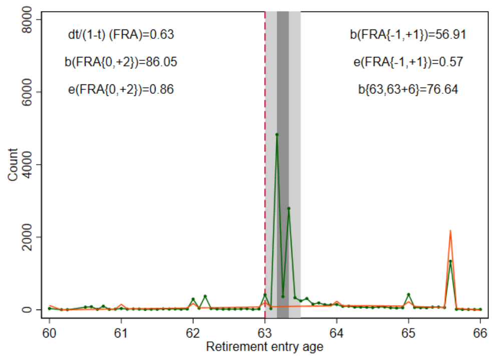
(c) 1952, treatment



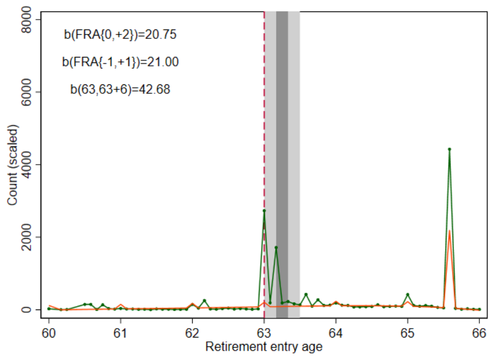
(d) 1952, control



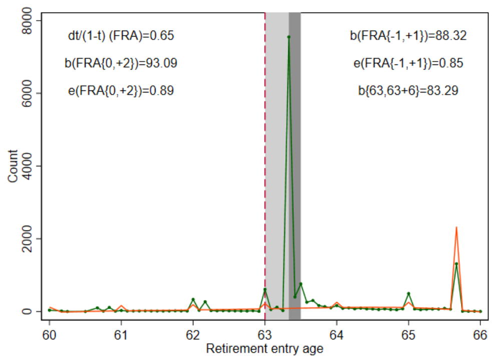
(e) 1953, treatment



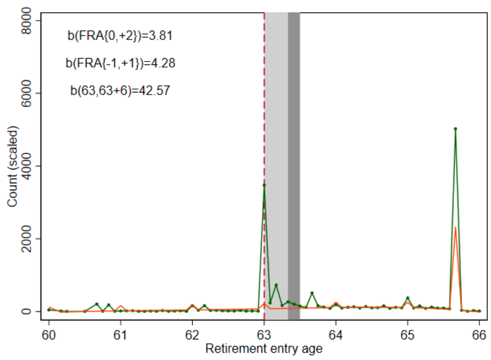
(f) 1953, control



(g) 1954, treatment



(h) 1954, control



Notes: This figure shows bunching at the FRA for individuals in the control and treatment group born in 1951-1954. The dark grey area indicates the bunching interval at the FRA $b(FRA\{0,+2\})$, while the light grey area indicates bunching at the broader bunching interval $b(63-63.5)$.

at the ERA regardless, foregoing the increase in their lifetime budget if they had worked until reaching eligibility. This bunching at the ERA tends to increase with the distance between the ERA and the effective FRA. In other words, workers are more likely to shift retirement entry from the ERA to a higher effective FRA if the gap between the two is small.

5 Differences-in-Differences Estimation

5.1 Empirical Strategy

Subsequently, we employ a difference-in-difference approach to estimate the reform's effect on retirement entry ages, contrasting retirement entry ages of those who are immediately eligible for the 'retirement at 63' scheme when turning 63 with those who do not become eligible before reaching the NRA. As in the bunching analysis, the treatment group is composed of individuals with 45 to 47 contribution years at age 63, whereas individuals in the control group are not able to meet eligibility conditions for the 'retirement at 63' scheme by a short margin (40–42.33 contribution years at age 63).

Formally, we estimate the following equation for each birth cohort between 1949 and 1954, contrasting retirement entry behavior with the pre-reform birth cohort 1948:

$$RetAge_{i,e,b} = \beta_b Post_b * Treat_e + \gamma Post_b + \delta Treat_e + X'_{i,b}\theta + \epsilon_{i,e,b} \qquad (2)$$

Subscript i indicates the respective individual, e whether an individual fulfills eligibility conditions and is part of the treatment or the control group, and b indicates the respective birth cohort. We use the retirement entry age $RetAge_{i,e,b}$ as dependent variable. Our main variable of interest, $Post_b * Treat_e$, is a dummy for eligible individuals in post-reform birth cohorts $b \in \{1949, 1950, 1951, 1952, 1953, 1954\}$, with β_b indicating the effect of the reform on the respective birth cohort's retirement entry age. The specification accounts for treatment and control group fixed effects $Treat_e$, capturing underlying behavioral differences between individuals in treatment and control groups, and birth cohort fixed effects $Post_b$. Specifications also control for individual characteristics $X_{i,b}$, i.e., gender, marital status, education level, a dummy for East Germany and the social insurance status prior to age 63.[19]

[19]This variable differentiates socially insured employees, marginally employed employees, voluntarily insured individuals including self-employed individuals who have opted into public pension insurance, unemployment benefit recipients (*Arbeitslosengeld I*), recipients of other benefits or

To validate this approach, we first analyze pre-trends for cohorts that are not or only partially affected by the reform. As shown above by Figure 2, pre-reform retirement entry ages are virtually identical for men in the treatment and control group. For women, pre-reform retirement entry ages differ in levels, but follow a similar trend. We formally test this by estimating first differences in retirement entry ages. Retirement patterns of men in birth cohorts 1948 (1949) – all (many) of whom attain the NRA prior to the reform – do not differ between treatment and control group (see Panel (a) of Figure A.6 in the Appendix), giving credibility to our estimation strategy.

In contrast, pre-reform retirement entry ages of women are significantly lower in the control group (Panel (b) of Figure A.6). Labor market affinity and selection effects due to gender roles could contribute to the different retirement entry patterns: In the cohorts born in the late 1940s and early 1950s, female labor market participation was much lower than male participation. For this reason, those who accumulated 45 contribution years may constitute a selection of particularly labor market affine women, who on average derive a lower utility from retiring early. In line with this, women in the control group in birth cohorts 1948–1951 were more likely to retire early (before age 63) through the 'retirement for women' scheme (cf. section 3.3).

Therefore, we control for individual characteristics $X_{i,b}$ that may account for differences in labor market affinity across treatment and control groups. We allow for differing coefficients on these control variables across birth cohorts. In order to contrast our findings with the literature, we subsequently set our estimated coefficients in relation to the decrease in the FRA induced by the reform.

5.2 Difference-in-Difference Results

Panel (a) of Figure 4 shows the first difference in retirement entry ages between treatment and control group when accounting for individual characteristics. Both groups' average retirement entry ages are virtually identical for birth cohort 1948, indicating that individual characteristics fully explain the differences in retirement entry patterns in the pre-reform period. As before, we observe a gradually growing effect for birth cohorts that become eligible to the 'retirement at 63' scheme. Panel (b) depicts coefficients in a corresponding difference-in-difference setting, contrasting birth cohorts with the pre-reform birth cohort 1948.

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with credit periods (*Anrechnungszeitversicherte*), e.g. due to sickness, and individuals who are neither working nor otherwise contributing towards pension insurance, such as housewives. If a person retires prior to reaching the age of 63, e.g., through the 'retirement for women' scheme, this variable reflects the last social insurance status prior to retiring.

Table 3 presents the corresponding coefficients for both the specification with and without controls. Responses in post-reform cohorts are slightly lower when controlling for individual characteristics. In our preferred specification with controls, individuals who are eligible for early retirement through the 'retirement at 63' scheme retire on average 7–8 months earlier than those in the control group. The effect slightly increases across birth cohorts from 6.8 months in 1952 to 7.2 months in 1953 and to 8.0 months in 1954. The gap between specifications with and without controls is driven by women's retirement behavior, which may have been altered by the abolition of the 'retirement for women' scheme. As shown by Table A.2 in the Appendix, the reform does not exert a significantly different effect on men and women born in 1952 or later when individual characteristics are controlled for. In contrast, the reduction in retirement entry ages is larger for women than for men when individual characteristics are not accounted for.

We conduct two further robustness checks. First, we estimate a specification without controlling for social insurance status before the age of 63 as this variable may be endogenous to the retirement decision, in particular for younger cohorts who reach the post-reform FRA after the reform went into effect. We find that our results are robust to excluding this control variable as shown in Table A.3 in the Appendix. If anything, not accounting for social insurance status prior to reaching age 63 slightly (but insignificantly) increases the magnitude of effects. Second, we employ labor market exit ages as dependent variable instead of retirement entry ages.[20] Results are very similar, as shown at the bottom of Table 3.

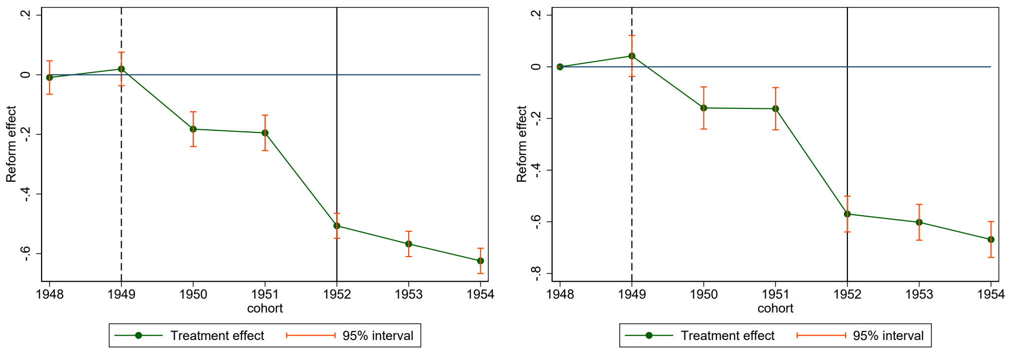
Finally, Table 4 presents additional results for a triple difference specification, interacting effects with labor market status prior to age 63. These results provide evidence on the mechanism. In the interpretation of the results, it should be taken into account that the labor market status of individuals before the age of 63 could be seen as exogenous immediately after the reform, but that younger generations had time to adjust their career paths in response to the reform. Hence, their labor market status prior to the FRA may be endogenous to the choice of the retirement entry age. As in section 4.2, we differentiate working and non-working individuals. Effects are significantly larger for non-working individuals, who antedate their retirement by 6 months more in response to the reform than those who were previously socially insured or self-employed. This is consistent with a larger slope in the lifetime budget constraint of eligible working individuals: They forego additional earnings and pension points, while those who are not working do not.

We subsequently set the estimated coefficients in relation to the decrease of

[20]Labor market exit ages are imputed based on retirement entry ages and social insurance status in the three years leading up to retirement.

Figure 4: Difference in retirement entry ages, controlling for individual characteristics

(a) First Differences, Pooled Sample (b) Difference-in-Differences, Pooled Sample



Notes: This figure shows first difference and difference-in-difference results for the reform's impact on retirement entry ages by birth cohort. Control variables include gender, marital status, education, a dummy for residence in East Germany and social insurance status prior to reaching age 63. The vertical lines indicate the first cohort that is partially affected by the 'retirement at 63' reform (1949) as well as the first cohort for which the reform is fully effective (1952): Individuals born after July 1, 1949, may become eligible to the 'retirement at 63' scheme prior to reaching the NRA, but have reached the post-reform FRA already before the reform went into effect in July 2014. Birth cohort 1952 is the first full cohort reaching the new FRA only after July 2014.

the FRA.[21] According to our estimates, a 1.0 year decrease in the FRA reduces the average pension claiming age by 0.3-0.4 years. This effect size is in line with previous estimates on the effect of increases in statutory retirement ages framed as reference points (see e.g. Mastrobuoni 2009, Staubli and Zweimüller 2013, Manoli and Weber 2018).

[21]This corresponds to the difference between the pre-reform and the post-reform FRA which becomes smaller for younger cohorts. See Table 1 for details.

Table 3: Difference-in-Difference Estimation

Baseline difference-in-difference: Retirement entry age as dependent variable						
	1949	1950	1951	1952	1953	1954
Post * Treat	0.062	-0.091**	-0.238***	-0.802***	-0.835***	-0.879***
se	(0.044)	(0.045)	(0.045)	(0.038)	(0.037)	(0.037)
N	38681	37692	37810	38127	38564	39257
Difference-in-difference with controls: Retirement entry age as dependent variable						
	1949	1950	1951	1952	1953	1954
Post * Treat	0.042	-0.159***	-0.162***	-0.570***	-0.602***	-0.668***
se	(0.041)	(0.042)	(0.042)	(0.035)	(0.035)	(0.035)
N	38681	37692	37810	38127	38564	39257
Difference-in-difference with controls: Labor market exit age as dependent variable						
	1949	1950	1951	1952	1953	1954
Post * Treat	0.038	-0.162***	-0.145***	-0.546***	-0.556***	-0.626***
se	(0.041)	(0.042)	(0.042)	(0.036)	(0.036)	(0.036)
N	38681	37692	37810	38127	38564	39257

Notes: *** p<0.01, ** p<0.05, * p<0.1. Comparison to retirement behavior in 1948. Treatment group: 45-47 contribution years at age 63. Control group: Individuals with 40-42.33 contribution years at age 63. Heteroskedasticity-robust standard errors. Control variables in specifications 2 and 3 include gender, marital status, education, a dummy for residence in East Germany and social insurance status prior to reaching age 63.

Table 4: Triple Difference Estimation by labor market status before age 63

Triple difference with controls: Retirement entry age as dependent variable						
	1949	1950	1951	1952	1953	1954
Post * Treat	0.061	-0.062	-0.128*	-0.325***	-0.310***	-0.381***
	(0.068)	(0.067)	(0.066)	(0.056)	(0.056)	(0.055)
Post * Treat * non-working	-0.135	-0.136	0.005	-0.535***	-0.543***	-0.453***
	(0.089)	(0.090)	(0.090)	(0.078)	(0.076)	(0.077)
N	25715	25935	26495	27402	28626	29321

Notes: *** p<0.01, ** p<0.05, * p<0.1. Treatment group: 45-47 contribution years at age 63. Control group: Individuals with 40-42.33 contribution years at age 63. Heteroskedasticity-robust standard errors. Control variables include gender, marital status, education, a dummy for residence in East Germany and social insurance status prior to reaching age 63. The sample excludes marginally employed individuals and individuals in partial or employer-sponsored early retirement (Altersteilzeit) as the distinction whether they are working or not is not clear-cut (see section 4.2 for details).

5.3 Factors Possibly Affecting Retirement Behavior

The above symmetric response to a decrease in the FRA suggests that liquidity constraints or poor health do not play an important role in our setting. These two factors have been identified to (partly) account for spikes at early retirement ages even if financial incentives for early retirement are low (Gustman and Steinmeier 2005; French 2005; Blundell et al. 2016), and would favor an asymmetric response.

To shed further light on this issue, we draw on the *Survey of Health, Ageing and Retirement in Europe* (Börsch-Supan et al. 2013; Börsch-Supan 2022). Exploiting that the German version of the panel survey can be linked to administrative records from public pension insurance accounts (SHARE-RV), we compare survey responses of respondents in treatment and control group at age 59–62, i.e., shortly before they reach their FRA or ERA, respectively.[22]

With respect to health, we exploit survey questions on the self-assessed health status and on the presence of a long-term illness. In line with Börsch-Supan et al. (2022), Table A.4 in the Appendix shows that the share of respondents who perceive their health status to be 'excellent', 'very good' or 'good' is somewhat higher in the treatment group compared to the control group (65% vs. 60%). Accordingly, respondents in the control group are slightly more likely to report a long-term illness (59% vs. 58%). However, these differences across groups are not statistically significant.

In order to investigate the presence of liquidity constraints, we rely on survey questions eliciting whether the respondent's household is able to make ends meet and can afford to pay an unexpected expense of 900 EUR without borrowing money.[23] For both questions, we find higher shares in the treatment group (84% vs. 75%, and 83% vs. 77%), with only the former difference being statistically significant.

Finally, we use the SHARE-RV data to shed some light on possible interactions between private retirement savings and early retirement. If more individuals in the control group can rely on private retirement savings, they might be less constrained by deductions on their public pension. Table A.4 indicates that the share of respondents who declare to own a private retirement account is very similar in treatment and control group (32% vs. 33%).

All these results should be interpreted with caution due to the small sample

[22] Note that the definition of treatment and control group slightly differs from the main analysis due to the smaller sample size of the SHARE survey. The treatment group consists of individuals who have accumulated at least 45 contribution years at their FRA, while the control group includes individuals with 35-42.3 contribution years.

[23] 900 EUR roughly represents the at-risk-of-poverty threshold defined as 60 percent of the median equivalised disposable income.

size. Yet, they do suggest that poor health, liquidity constraints or differences in private retirement savings among eligible and non-eligible individuals are not amongst the main drivers of the behavioral responses studied in this paper. Moreover, they rationalize why responses are of a similar magnitude than of studies assessing an increase in the FRA, and provide further evidence that the strong behavioral responses are driven by both financial incentives and the framing of the new FRA of 63 as a reference age for retirement.

6 Fiscal Costs

What are the fiscal costs of a pension reform that lowers the full retirement age for individuals with long contribution histories? This is an important question for policy-makers who want to introduce exemptions for (allegedly) deserving individuals from increasing statutory retirement ages. Fiscal costs will depend on (i) the number of beneficiaries, and (ii) their behavioral response to the reform. Our fiscal costs analysis of the 2014 early retirement reform accounts for both of these factors as explained below. In the longer run, the number of beneficiaries is endogenous as individuals may adjust their labor market careers early on in order to become eligible for the 'retirement at 63' scheme later in life. This latter channel is not reflected in our analysis focusing on the short run effects of the reform.

Previous policy analyses have yielded a wide range of cost estimates (see section A.3 in the Appendix for details). The reason is that these estimates are often based on extreme assumptions concerning counterfactual retirement behavior in the absence of the reform and hence do not account for actual behavioral responses and resulting changes to social insurance contributions and tax revenues. At the lower end, the draft bill of the retirement reform estimated additional public pension insurance expenditures amounting to 0.9 billion euro in 2014, 1.9 billion in 2015, 2.2 billion in 2016 and 2.0 billion in 2017, with costs slightly declining in subsequent years and then again increasing to 3.1 billion in 2030 (Deutscher Bundestag 2014b).

Against this background, we strive to provide a more precise estimate of fiscal costs. We assess costs for fiscal years 2014-2017 as almost all retirees in these years are in birth cohorts 1948-1954, for which we have estimated behavioral responses. Building on our detailed microdata and our difference-in-difference estimation results, we comprehensively account for behavioral responses as well as for foregone deductions, taxes and contributions. Overall, the reform's costs entail changing pension insurance expenditures as well as foregone social insurance contributions and tax revenues. For each of these dimensions, we calculate actual and counterfactual expenditures, which we then aggregate and upweigh to match the total number of those retiring through the retirement scheme, taken from official statistics (Deutsche Rentenversicherung Bund 2022). Section A.3 in the Appendix provides more details on our methodology.

Table 5 presents the ensuing fiscal cost estimates. Total costs reflect overall net fiscal costs, accounting for both changes in tax and social insurance revenues as well as in pension insurance expenditures. While our calculations suggest that aggregate pension insurance expenditures amounted to 9 billion euros between 2014 and 2017, total fiscal costs correspond to 18 billion. The latter correspond to 1.8% of overall public pension expenditures between 2014 and 2017. With estimated pension insurance expenditures of 3.7 billion euros and total fiscal costs of 7.2 billion euros in 2017, our fiscal cost estimates are at the upper end of the range of cost estimates. This is also due to the high number of claimants, which had been underestimated when the reform was announced. Yet, costs per claimant lie between projections that assumed unchanged retirement behavior and projections assuming that all claimants would have retired at the NRA in the absence of the reform.[24]

Table 5: Fiscal costs of the early retirement reform

Costs in billion euros				
	2014	2015	2016	2017
Pension insurance expenditures	0.54	2.35	2.58	3.67
Total costs	1.08	4.62	5.02	7.22

Notes: This table shows annual fiscal cost estimates in current prices (billion euros). Estimations are based on a back-of-the-envelope calculation of fiscal costs, factoring in behavioral adjustments to the reform. Total costs encompass pension insurance expenditures, income taxes and social insurance contributions.

7 Conclusion

This paper assesses the responses to a recent German pension reform that reduced the age at which individuals with long contribution years can retire with full pension benefits by up to two years. The 2014 reform of the 'old-age pension for the especially long-term insured' (commonly known as 'retirement at 63' scheme) was accompanied by a large-scale publicity campaign of the federal government that framed the age of 63 as new reference age for retirement for the long-term insured with at least 45 contribution years. Exploiting high-quality administrative data from the German

[24] In the former scenario, eligible individuals are assumed not to antedate their retirement entry. In the latter scenario, eligible individuals are assumed to retire at the NRA in the counterfactual scenario without the reform, implying a much larger behavioral response as the one estimated in our difference-in-difference analysis.

public pension insurance, we employ bunching methods and estimate the effect of the reform on the pension claiming as well as the labor market exit age using a difference-in-difference approach.

Our results indicate that bunching in the treatment group at the FRA exceeds the control group's bunching at the ERA by 83%, indicating substantial responses. Largest elasticites are found for younger cohorts who had more room to antedate pension claiming. In line with the bunching responses, our difference-in-difference estimates reveal that individuals who are eligible for the 'retirement at 63' scheme retire on average 7–8 months earlier in response to the reform than those in the control group. Scaled by the effective change in the FRA, this effect corresponds to a reduction in the average pension claiming age of 0.3–0.4 years for a 1.0 year decrease in the FRA. Our analysis based on the Survey of Health, Ageing and Retirement in Europe matched to administrative records from public pension insurance accounts (SHARE-RV) suggests that responses are not driven by differences in health conditions, liquidity constraints or private retirement savings between treatment and control group, but rather by the new FRA of 63 serving as a reference point and by financial incentives. Overall, the behavioral responses to a decrease in the FRA found in this paper are of similar magnitude as to increases in statutory retirement ages framed as reference points in the literature, but much larger than estimates based on pure financial incentives.

We subsequently use our difference-in-difference methodology to quantify fiscal costs. With additional pension insurance expenditures of 9 billion euros and aggregate fiscal costs of 18 billion euros between 2014 and 2017, our cost estimates exceed most previous back-of-the-envelope cost estimates, including those presented in the draft government bill. The latter assumed additional pension insurance expenditure of just 7 billion euros over the period 2014–2017, which highlights the policy-relevance of precisely estimated behavioral parameters for ex-ante fiscal cost projections.

Our paper has policy implications for countries that consider similar pension reforms, exempting 'deserving' individuals from general increases in the NRA. Our results reveal that eligible individuals react relatively strongly to a reform that combines a reduction in the FRA with financial incentives to retire early and a framing of the new FRA as reference age for retirement. While we find a larger behavioral response for non-working compared to working individuals, the reform yet has induced skilled workers with stable employment biographies to leave the labor market early. It has thus exacerbated the shortage of skilled labor that is among the main challenges the German labor market faces as a consequence of demographic change. Moreover, the reform has deteriorated the fiscal sustainability of the German public pension system.

These results have to be interpreted in the light of the following limitations. Firstly, as noted before we only identify the short run effects of the reform. Long run effects may be larger than the ones estimated in this paper if younger cohorts adjust their labor maket careers in response to the reform. Secondly, our results apply to labor market affine individuals with long contribution histories. While our sample of individuals with long contribution years covers a substantial share of the retirees in the respective cohorts, individuals with a lower labor market affinity may respond differently to a reform lowering the FRA. Thirdly, we abstract from normative welfare considerations and do not analyze distributional implications.

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A Appendix

A.1 Appendix Figures and Tables

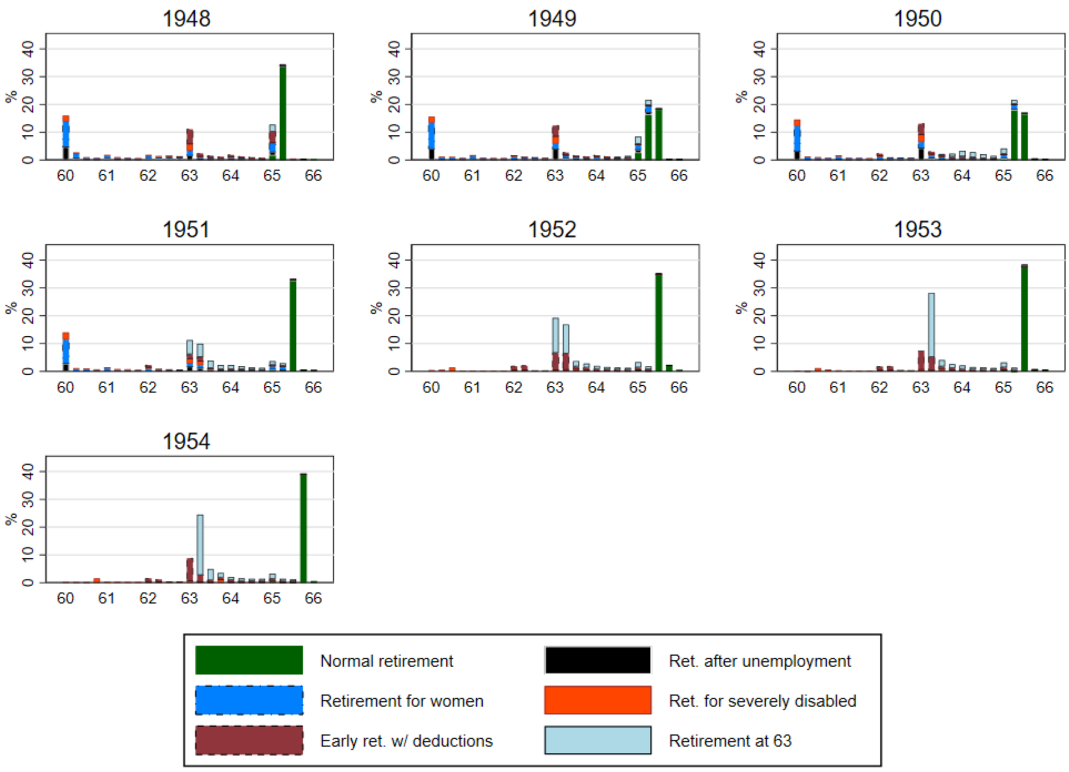
Figure A.1: Framing of the reform

(a) Announcement of the reform

(b) Reform campaign

Notes: The upper photo shows that the reform of the old-age pension for the especially long-term insured was announced as a package that would *'close important fairness gaps and better acknowledge life-time achievement'*. The lower photo shows an excerpt from the launch of the publicity campaign with the former Minister of Labour and Social Affairs, Andrea Nahles. The ad with its key campaign message (*'earned, not given'*) was later displayed on billboards throughout Germany.

Figure A.2: Distribution of retirement entry ages and retirement schemes by birth cohort



Notes: This figure shows the distribution of retirement entry ages and retirement schemes by birth cohort in the overall sample, irrespective of contribution years. All other sample restrictions, such as the exclusion of individuals with previous retirement spells, do apply.

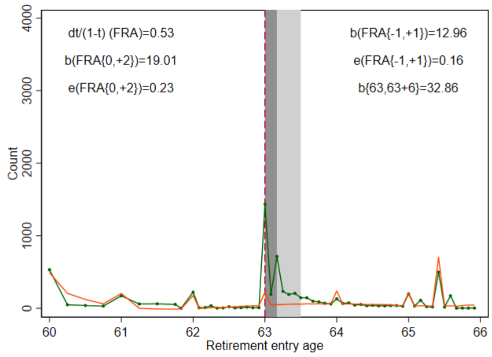
Table A.1: Retirement pathway choices by birth cohort in the full sample

	1948	1949	1950	1951	1952	1953	1954
Normal retirement	36.5%	37.8%	36.0%	34.7%	38.6%	39.5%	40.0%
Early retirement with deductions	14.1%	8.5%	7.8%	5.8%	20.9%	19.9%	18.9%
Retirement at 63[a]	2.60%	5.3%	11.1%	20.4%	32.4%	32.9%	33.8%
Retirement for women	23.7%	24.4%	23.0%	10.6%	0%	0%	0%
Retirement after unemployment	13.1%	13.8%	11.6%	8.4%	0%	0%	0%
Retirement for severely disabled	9.9%	10.1%	10.5%	10.1%	8.1%	7.6%	7.3%

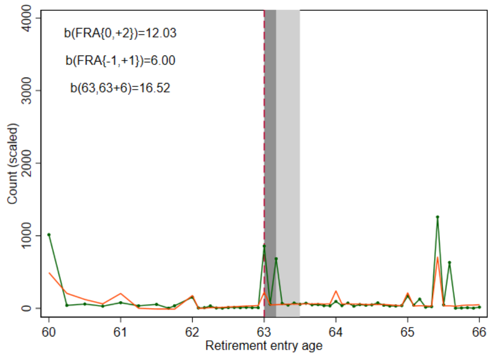
Notes: This table shows retirement choices in the public pension insurance account data. The sample includes retirees regardless of their contributory periods, excluding invalidity benefit recipients. *a*: Prior to July 2014, a precursor of the 'retirement at 63' scheme allowed for early retirement without deductions for individuals with 45 contributory years at age 65.

Figure A.3: Bunching by gender and eligibility

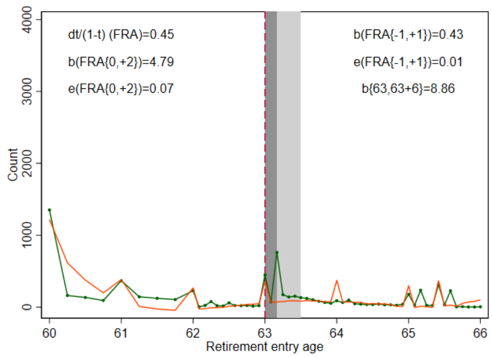
(a) 1951, treatment, men



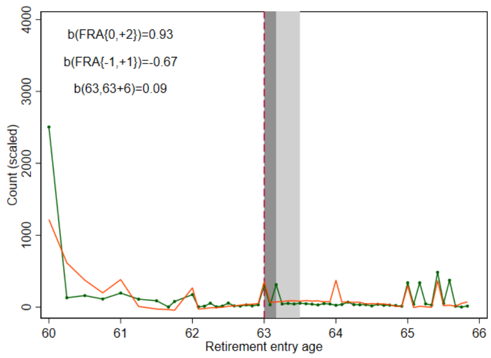
(b) 1951, control, men



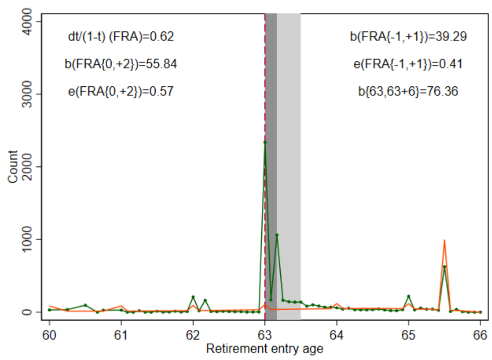
(c) 1951, treatment, women



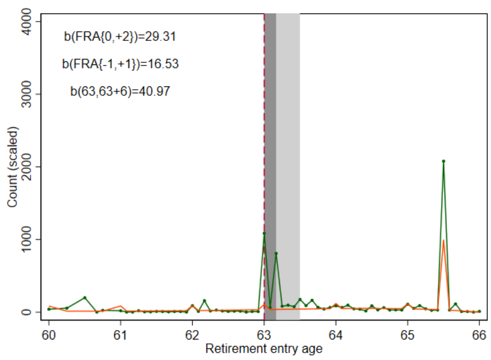
(d) 1951, control, women



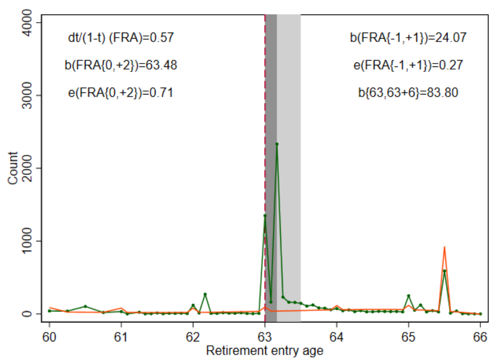
(e) 1952, treatment, men



(f) 1952, control, men



(g) 1952, treatment, women



(h) 1952, control, women

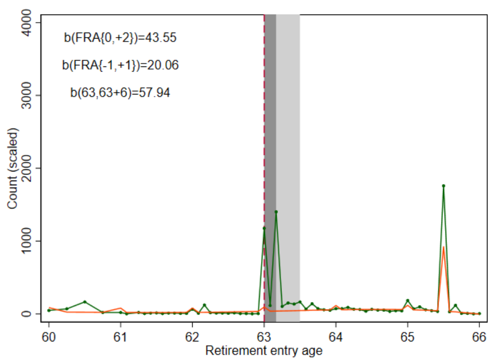
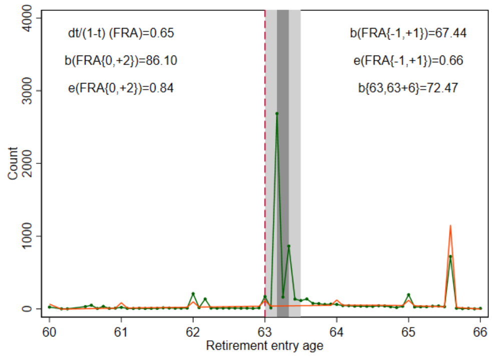


Figure A.3: (Continued) Bunching by gender and eligibility

(i) 1953, treatment, men



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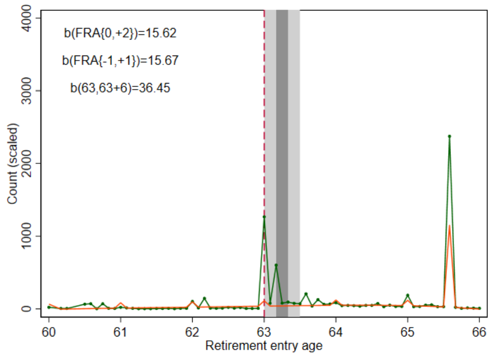
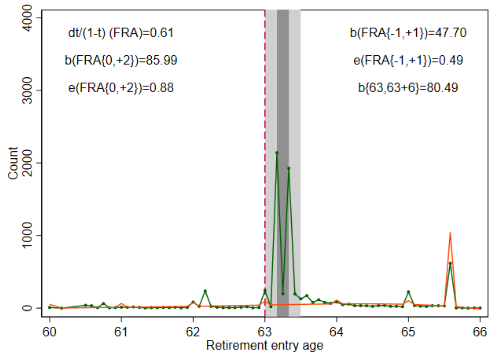
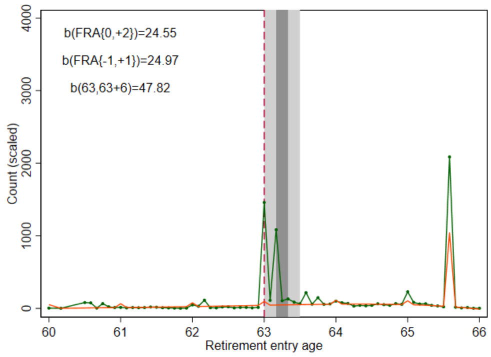


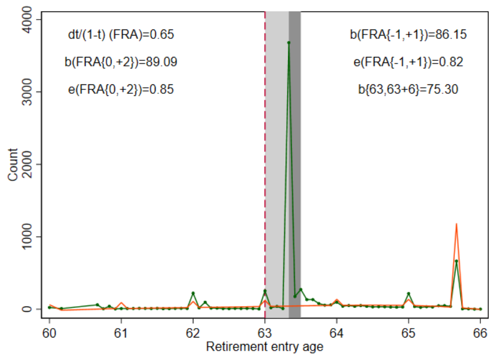
(k) 1953, treatment, women



(l) 1953, control, women



(m) 1954, treatment, men



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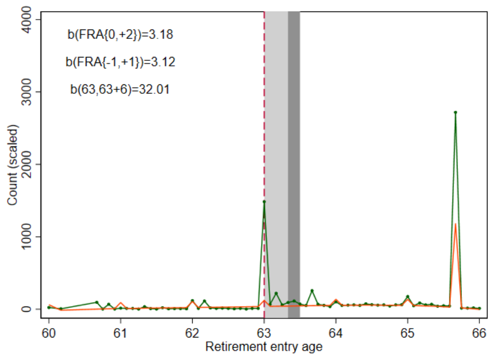
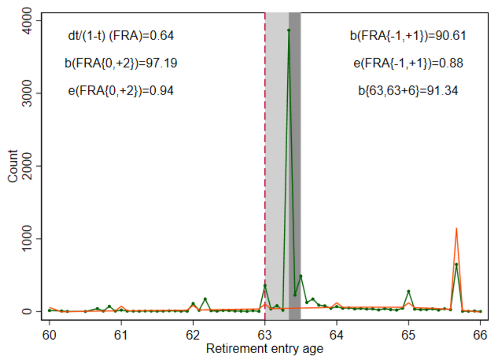


(o) 1954, treatment, women



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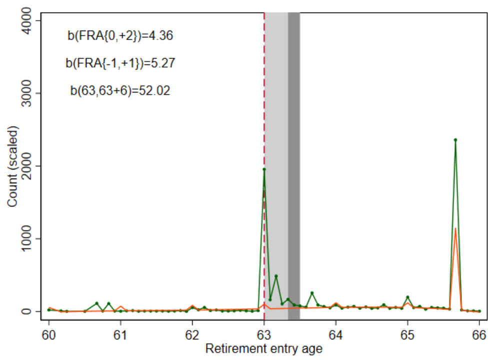
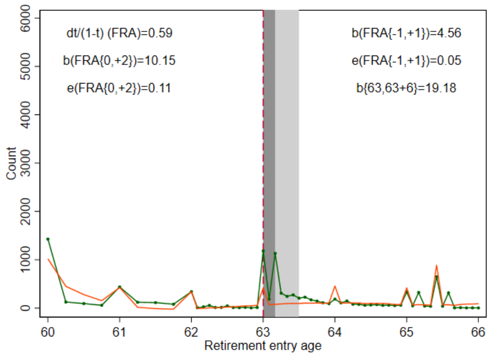
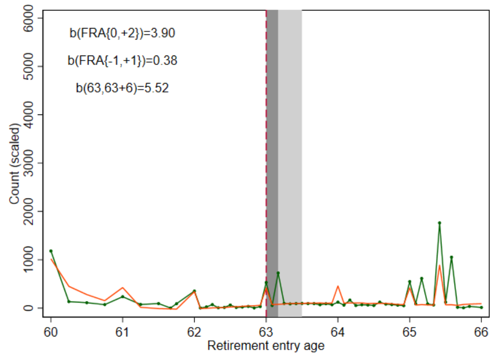


Notes: This figure shows bunching at the FRA for individuals in the control and treatment group by gender. The dark grey area indicates the bunching interval at the FRA $b(FRA\{0,+2\})$, while the light grey area indicates bunching at the broader bunching interval $b(63-63.5)$.

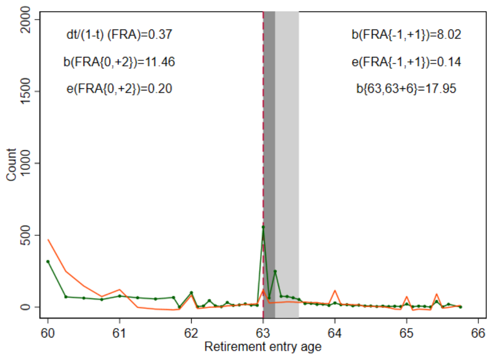
Figure A.4: Bunching by employment status and eligibility



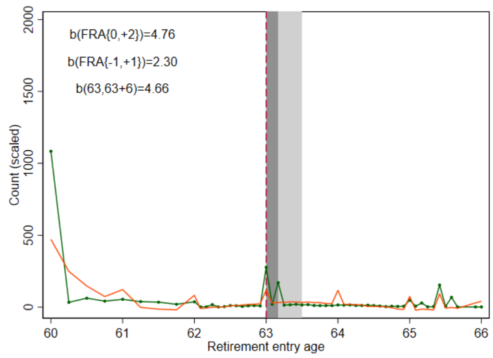
(a) 1951, treatment, working



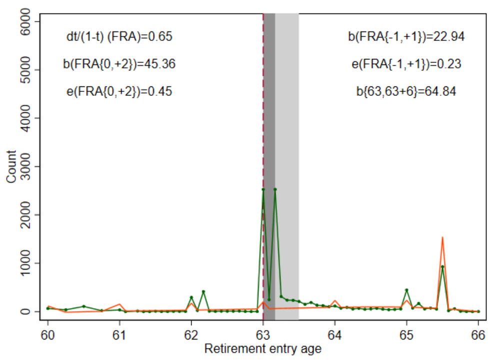
(b) 1951, control, working



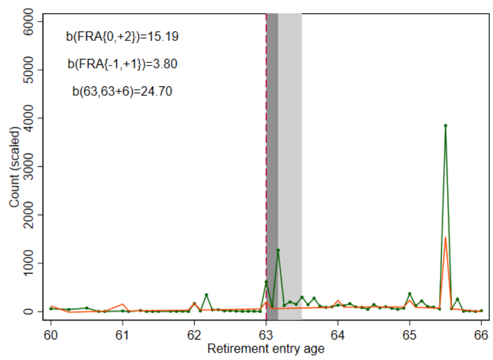
(c) 1951, treatment, non-working



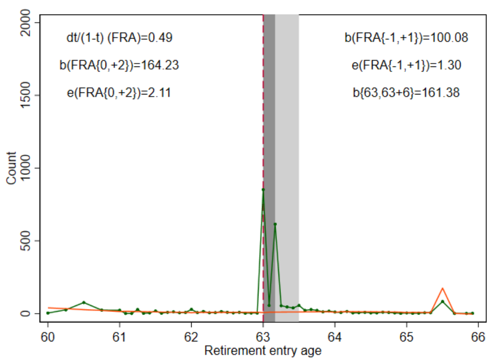
(d) 1951, control, non-working



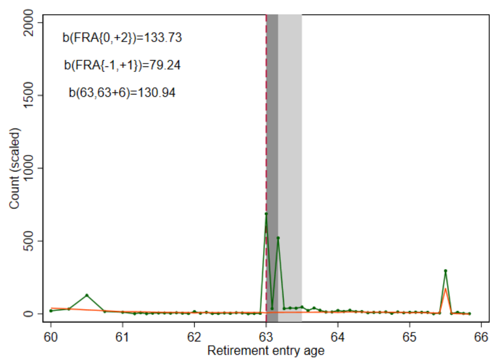
(e) 1952, treatment, working



(f) 1952, control, working



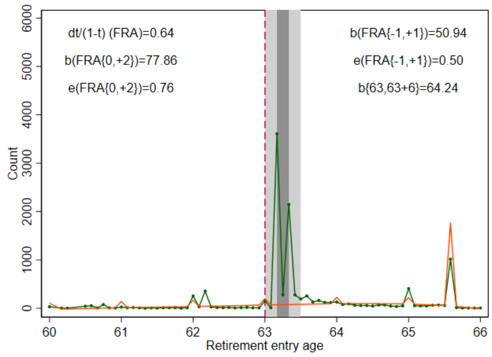
(g) 1952, treatment, non-working



(h) 1952, control, non-working

Figure A.4: (Continued) Bunching by employment status and eligibility

(i) 1953, treatment, working



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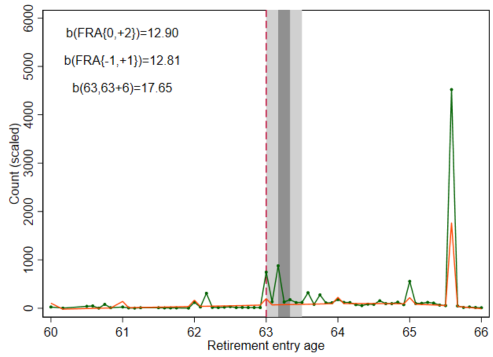
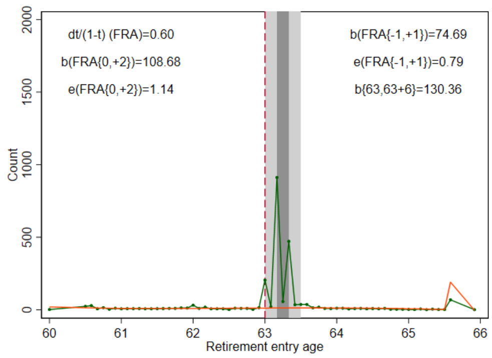
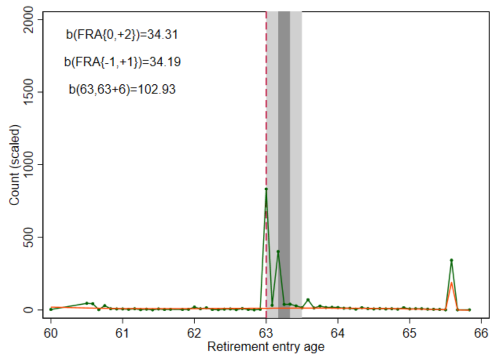


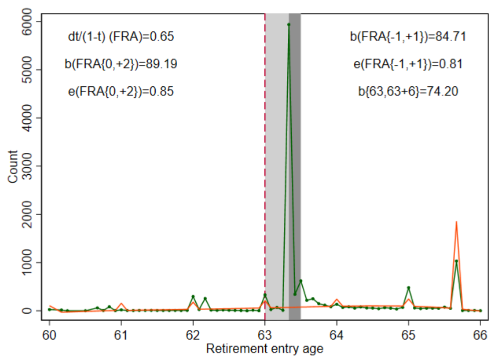
(k) 1953, treatment, non-working



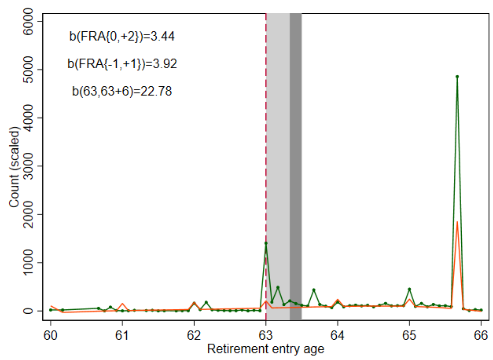
(l) 1953, control, non-working



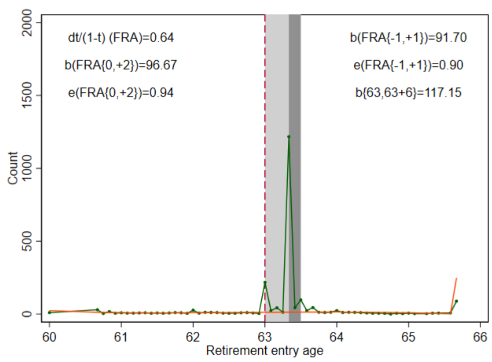
(m) 1954, treatment, working



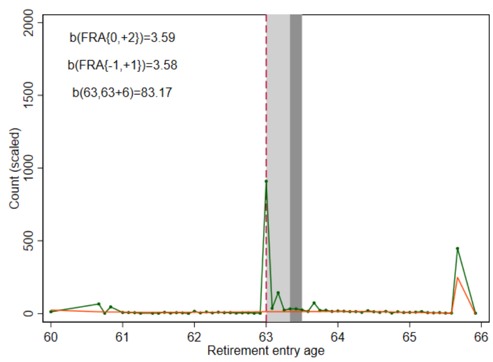
(n) 1954, control, working



(o) 1954, treatment, non-working

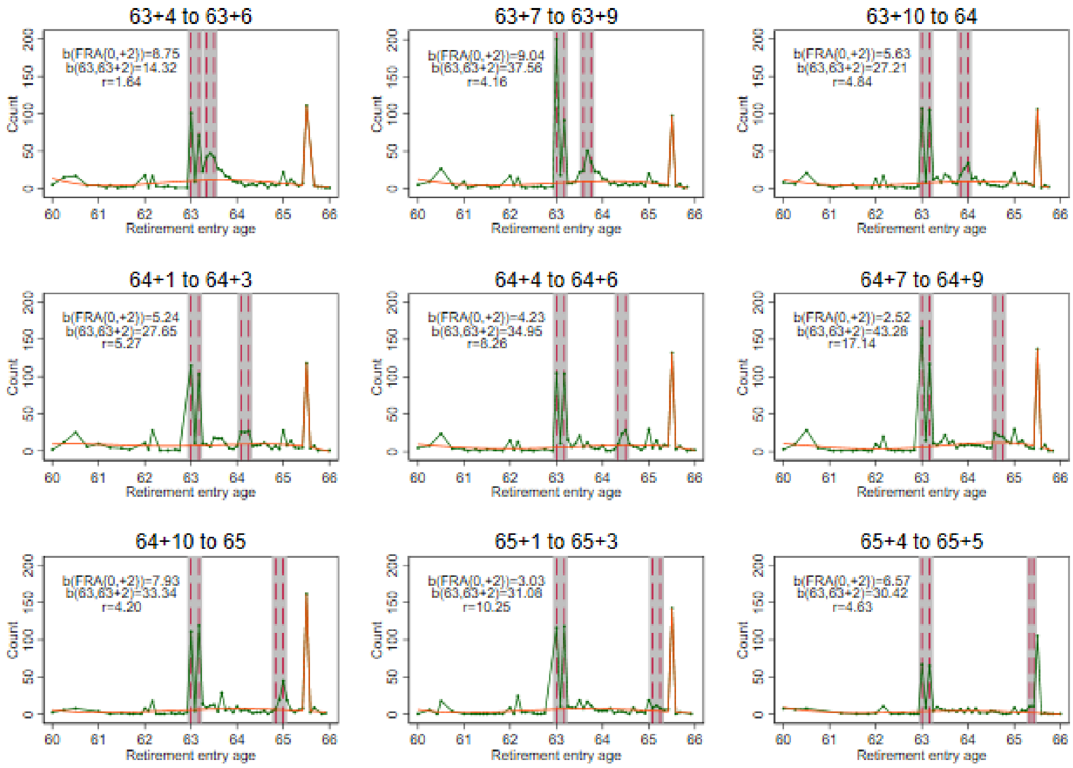


(p) 1954, control, non-working



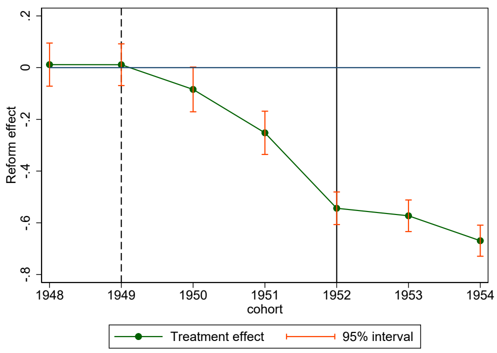
Notes: This figure shows bunching at the FRA for individuals in the control and treatment group by employment status, differentiating between working (socially insured and self-employed) and non-working individuals. The dark grey area indicates the bunching interval at the FRA $b(FRA\{0,+2\})$, while the light grey area indicates bunching at the broader bunching interval $b(63-63.5)$.

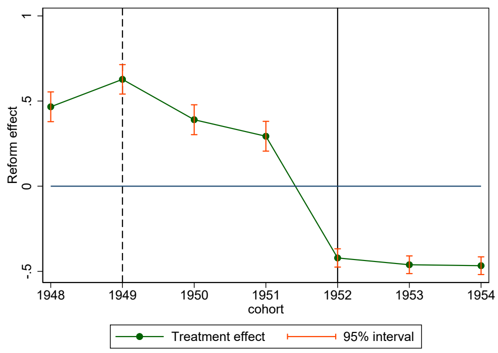
Figure A.5: Bunching by distance between effective FRA and NRA for birth cohort 1952



Notes: This figure shows bunching responses for birth cohort 1952 attaining eligibility, i.e., 45 contribution years, at different ages. The contribution threshold, i.e., the interval in which eligibility is attained, is depicted above each panel. The first grey area indicates the bunching interval at the ERA $b(63, 63+2)$. The second grey area indicates the bunching interval at the effective FRA $b(FRA\{0, +2\})$. r depicts the ratio of both bunching masses.

Figure A.6: Difference in retirement entry ages by gender





Notes: This figure shows first differences for the reform's impact on retirement entry ages by birth cohort and gender. The vertical lines indicate the first cohort that is partially affected by the 'retirement at 63' reform (1949) as well as the first cohort for which the reform is fully effective (1952): Individuals born after July 1, 1949, may become eligible for the 'retirement at 63' scheme prior to reaching the NRA, but have reached the post-reform FRA already before the reform went into effect in July 2014. Birth cohort 1952 is the first full cohort reaching the new FRA only after July 2014.

Table A.2: Triple Difference Estimation by Gender

Triple differences: Retirement entry age as dependent variable, estimates by gender						
	1949	1950	1951	1952	1953	1954
Post * Treat	-0.000	-0.096	-0.264***	-0.555***	-0.585***	-0.681***
se	(0.059)	(0.061)	(0.060)	(0.053)	(0.053)	(0.052)
Post * Treat * Woman	0.161*	0.020	0.091	-0.332***	-0.343***	-0.252***
se	(0.086)	(0.088)	(0.087)	(0.075)	(0.074)	(0.074)
N	38681	37692	37810	38127	38564	39257
Triple differences with controls: Retirement entry age as dependent variable, estimates by gender						
	1949	1950	1951	1952	1953	1954
Post * Treat	-0.101*	-0.208***	-0.288***	-0.537***	-0.570***	-0.695***
se	(0.052)	(0.053)	(0.054)	(0.048)	(0.048)	(0.048)
Post * Treat * Woman	0.271***	0.090	0.223***	-0.061	-0.062	0.042
se	(0.074)	(0.075)	(0.076)	(0.065)	(0.064)	(0.064)
N	38681	37692	37810	38127	38564	39257

Notes: *** p<0.01, ** p<0.05, * p<0.1. Treatment group: 45-47 contribution years at age 63. Control group: Individuals with 40-42.33 contribution years at age 63. Heteroskedasticity-robust standard errors. Control variables in specification 2 include gender, marital status, education, a dummy for residence in East Germany and social insurance status prior to reaching age 63.

Table A.3: Difference-in-Difference Estimation: Robustness Check

Difference-in-difference with controls, but without controlling for social insurance status: Retirement entry age as dependent variable						
	1949	1950	1951	1952	1953	1954
Post * Treat	0.074*	-0.089**	-0.071	-0.659***	-0.678***	-0.752***
se	(0.043)	(0.045)	(0.045)	(0.038)	(0.038)	(0.038)
N	38681	37692	37810	38127	38564	39257

Notes: *** p<0.01, ** p<0.05, * p<0.1. Comparison to retirement behavior in 1948. Treatment group: 45-47 contribution years at age 63. Control group: Individuals with 40-42.33 contribution years at age 63. Heteroskedasticity-robust standard errors. Control variables include gender, marital status, education and a dummy for residence in East Germany.

Table A.4: Health Status, Liquidity Constraints and Private Retirement Savings

Share of respondents	Treatment group Mean/SE	Control group Mean/SE	Diff./SE	N
With self-assessed health status being				
excellent/very good/good	0.646	0.596	0.05	578
	(0.026)	(0.031)	(0.041)	
With long-term illness	0.577	0.588	-0.011	578
	(0.027)	(0.032)	(0.042)	
Being able...				
...to make ends meet	0.841	0.75	0.091**	402
	(0.024)	(0.033)	(0.04)	
...to pay an unexpected expense				
of 900 EUR without borrowing any money	0.827	0.768	0.059	364
	(0.027)	(0.033)	(0.042)	
With private retirement savings	0.32	0.331	-0.011	375
	(0.033)	(0.036)	(0.049)	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Own calculations based on SHARE waves 5-8. Fieldwork times: Wave 5: 2013, Wave 6: 2015, Wave 7: 2017, Wave 8: 2019/2020. Treatment group: Individuals eligible for the 'retirement at 63' scheme when reaching the FRA. Control group: Individuals with 35-42.3 contribution years at the FRA. Survey responses at age 59–62. If respondents participated in several waves when aged 59–62, the interview closest to age 62 is taken. Survey respondents have consented to the linkage of their survey responses with administrative records of the German pension insurance.

A.2 Bunching Methodology

Lifetime budget constraints. Following Seibold (2021), we estimate the observed elasticity of retirement entry of eligible individuals with respect to the implicit net of tax rate. To calculate the local change in the implicit net of tax rate, we first simulate lifetime budget constraints prior to and upon attaining the respective birth cohort's FRA (see Figure 1 for a stylized depiction of the lifetime budget constraints). These lifetime budget constraints draw on lifetime wage earnings as well as pension benefits under the respective pathways, using a discount factor of 3%. As we only directly observe earnings in the last three years before retirement entry, we approximate gross wage earnings based on pension contribution histories. We then apply an income tax simulator to calculate net wage earnings. Pension benefits are estimated based on earned pension points, accounting for pension adjustments by retirement entry pathway and birth cohort. Our calculations use average remaining life expectancies by gender and birth cohort from mortality tables by the German Statistical Office. If employees are eligible for more than one pathway (e.g., the retirement for women and the long-term insured pathway), we choose the most advantageous pathway yielding the highest lifetime income.

Bunching estimation. Based on Chetty et al. (2011) and Seibold (2021), we estimate an excess bunching mass above a counterfactual density of retirement entry ages. This is done by fitting a joint counterfactual polynomial to the treatment and control group's observed density of retirement entry ages, excluding the FRA. We approximate notches as kinks for marginal bunchers, using the Kleven and Waseem (2013) approximation to convert the change in the average tax rate at the notch into a change in the marginal tax rate.

As described in section 4, we estimate bunching for three distinct intervals, $b(FRA\{-1,+1\})$, $b(FRA\{0,+2\})$, and $b(FRA\{63,63.5\})$, to assess both bunching responses at the FRA and overall bunching at early retirement entry ages. Our estimation allows for round age effects by including a dummy for full year ages, as well as a dummy for the NRA. In our main specifications, we choose an order of 7 for the polynomial and include 36 monthly bins on both sides of the ERA for estimating the polynomial. As shown in Table A.5 below, results are generally robust to changes in the order of the polynomial and in the number of bins for birth cohorts. Bunching masses increase for birth cohort 1951 when a smaller number of bins is used. These differences are mainly attributable to whether individuals who retire at age 60 through the 'early retirement for women' scheme are included when estimating the counterfactual distribution.

Table A.5: Bunching Estimation: Robustness Check

		Excess Mass									
	Bins:	24					36				
	Polynomial:	5	6	7	8	9	5	6	7	8	9
Year of Birth	Intervall										
1951	[-1, +1]	11.74	11.78	11.81	10.86	10.35	12.02	5.64	5.32	5.80	5.60
	[0, +2]	20.98	21.05	21.10	19.51	18.72	20.90	10.78	10.29	11.08	10.78
	[63, 63.5]	38.83	38.96	39.16	34.62	32.70	58.78	20.21	18.23	20.31	19.63
1952	[-1, +1]	25.88	25.85	25.84	25.99	26.45	29.52	31.79	31.52	31.12	31.31
	[0, +2]	49.60	49.54	49.51	49.80	50.62	55.31	59.37	58.88	58.16	58.48
	[63, 63.5]	71.67	71.53	71.46	72.07	73.62	73.19	79.66	79.12	77.90	78.40
1953	[-1, +1]	52.95	52.88	52.86	49.79	49.95	51.73	57.53	56.91	55.70	55.73
	[0, +2]	81.01	80.88	80.87	76.70	76.90	78.51	86.72	86.05	84.08	84.16
	[63, 63.5]	69.61	69.51	69.51	66.75	66.83	71.02	77.36	76.64	75.33	75.39
1954	[-1, +1]	82.92	82.83	82.78	80.19	78.77	76.91	87.34	88.32	85.96	85.60
	[0, +2]	89.00	88.88	88.85	86.42	84.86	82.07	92.87	93.09	91.01	90.81
	[63, 63.5]	74.98	74.92	74.88	73.05	71.90	74.11	83.17	83.29	81.61	81.42

		Elasticity									
	Bins:	24					36				
	Polynomial:	5	6	7	8	9	5	6	7	8	9
Year of Birth	Intervall										
1951	[-1, +1]	0.15	0.15	0.15	0.14	0.13	0.15	0.07	0.07	0.07	0.07
	[0, +2]	0.26	0.26	0.26	0.24	0.23	0.26	0.13	0.13	0.14	0.13
1952	[-1, +1]	0.28	0.28	0.27	0.28	0.28	0.31	0.34	0.34	0.33	0.33
	[0, +2]	0.52	0.52	0.52	0.53	0.54	0.58	0.63	0.62	0.61	0.62
1953	[-1, +1]	0.53	0.53	0.53	0.50	0.50	0.52	0.58	0.57	0.56	0.56
	[0, +2]	0.81	0.81	0.81	0.76	0.77	0.78	0.86	0.86	0.84	0.84
1954	[-1, +1]	0.80	0.80	0.80	0.77	0.76	0.74	0.84	0.85	0.83	0.83
	[0, +2]	0.85	0.85	0.85	0.83	0.82	0.79	0.89	0.89	0.87	0.87

Notes: The table shows the sensitivity of the excess mass and the elasticity with respect to the order of the polynomial and the number of bins used in the bunching estimation for the treatment group. The main specification is based on an order of 7 for the polynomial and includes 36 monthly bins on both sides of the ERA.

A.3 Fiscal Costs

Previous policy analyses have quantified the fiscal costs of the reform without fully accounting for behavioral responses. Pimpertz (2017) provides a lower bound estimate of the reform's cost, assuming unchanged retirement behavior and focusing on foregone deductions only. This approach yields cost estimates ranging between 0.14 billion in 2014 and 1.2 billion euros in 2017. Using a simulation model, Werding (2014) projects somewhat higher costs of 0.5 billion euros in 2014, rising to 2.6 billion in 2015 and 3.2 billion in 2016 and 2017. Schnabel (2015) estimates that annual fiscal costs might rise to 6 billion euros if 125,000 individuals retire via the scheme per year – a figure which the number of actual claimants exceeds by far.[25] At the upper end, monthly costs of 1.3 to 2 billion euros were frequently circulated

[25] In 2016, 225,290 individuals retired via the 'retirement at 63' scheme (Deutsche Rentenversicherung Bund 2022).

in the media – an overestimation based on the total sum of pension benefits paid under the 'retirement at 63' scheme, neglecting that many of those claiming early retirement benefits would have otherwise retired early through another scheme.

Against this background of widely varying cost estimates, we strive to provide a more precise estimate of fiscal costs, accounting for behavioral responses. We build on our difference-in-difference results to model behavioral responses to the reform for each birth cohort. For this endeavor, we derive the share of eligible individuals who antedate their retirement in response to the reform, calculating fiscal costs both for those who adjust their behavior and those who primarily derive windfall gains. Our calculations encompass the following aspects:

Pension insurance expenditures. The retirement reform affects pension insurance expenditures along three dimensions: deductions, retirement timing, and accumulated pension points. Costs are assigned to the relevant fiscal years, assuming that retirement under the reform takes place in the middle of the year. While retirement benefits under the reform are provided in the data, we impute counterfactual retirement benefits, accounting for changes in retirement timing, the ensuing change in pension points, as well as deductions. The abolition of deductions under the 'retirement at 63' scheme increases benefits of those retiring early, even absent any behavioral response to the reform. In addition, retiring earlier due to the reform results in less accumulated pension points, which reduces retirement benefits. At the same time, claiming benefits early raises fiscal costs in the years of early retirement.

Income tax revenues. The timing of entering retirement and the amount of benefits affect taxable income and thus have direct implications for tax revenues. Furthermore, retirement benefits are only partially taxable. We calculate average tax rates on wages and retirement benefits by applying the German individual income tax schedule to taxable income. We then apply these tax rates to the income changes following the reform. Individuals postponing their retirement in the counterfactual scenario are assumed to extend their pre-retirement employment status at their previous wage. As we lack comprehensive data on partners' taxable income as well as on other income sources such as investment or rental income, we are only able to provide a rough approximation of the reform's impact on tax revenues.

Social insurance contributions. Entering retirement likewise affects social insurance contributions. While employed individuals' employer and employee social insurance contributions amount to about 40%[26], contributions of retirees only correspond to roughly 18% of their retirement benefits. Considering that the public pension insurance covers about half of retirees' health insurance fees, retirees are

[26]Contribution rates slightly differ across years, between those with and those without children, for those with lower wages and by health insurance provider.

only liable to contribute the remainder of about 10.5%. These low rates are due to retirees not contributing to public pension and unemployment insurance. At the same time, if retirement benefits increase due to foregone deductions, a small percentage of these costs is compensated by rising health insurance and nursing care insurance contributions. As before, calculations assess actual and counterfactual social insurance contributions in each year.

Our total fiscal cost measure comprises all three aspects, reflecting net costs by accounting for both changes in revenues and expenditures. Our calculations do not account for second round effects, such as possible increases in social insurance contribution rates or lower future pension adjustments to compensate for foregone revenues following the reform.