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Early retirement for early starters – A well targeted policy for people with high job demand?

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Abstract

Despite growing demographic pressures a reform in Germany has lowered the full retirement age (FRA) for early starters. In this paper we analyze whether the reform was well targeted. Specifically, we study if the early retirement scheme benefits workers with high job strain or if workers with better jobs and the potential to increase employment are the main beneficiaries of this pathway. For the empirical analysis we use high-quality administrative data from the Federal Employment Agency covering the complete employment biographies of individuals and including information about the job strain at the occupational level. We exploit policy variation induced by the pension reform using two complementary identification strategies: Difference in Differences and Regression Discontinuity Design. The reform reduced average labor market exit age by between six to seven months corresponding to three to 3.5 months for each year the FRA was lowered. The effect is stronger for individuals working in demanding occupations and for individuals with higher incomes. Disentangling job strain into physical and psychosocial job strain does not reveal different effects between both measures of job demand.

Keywords: pension reform, job demand, early retirement, differences in differences, regression discontinuity design

JEL: H55, J13, J21, J26

1. Introduction

A key strategy to adapt pay-as-you-go pension systems to demographic aging is to prolong working lives by increasing statutory retirement ages. This policy has the potential to increase contributions and to reduce spending at the same time (Blundell, E. French, and G. Tetlow, 2016). However, a uniform increase in the retirement age for all workers raises concerns about the distributional consequences and fairness. For those in good health and better paid jobs it is easier to adjust to a higher statutory retirement age and to extend the working life while workers in low paid employment and with high job demand have less flexibility to react to these policies. This is one reason why such policies are very unpopular and difficult to implement (Staubli and Zweimüller, 2013).

One way to address the distributional concerns is to develop targeted early retirement programs for people with high job demand or health problems. While many countries have – in addition to disability pensions – rules for specific occupations, there are some countries which favor a more general approach by allowing early starters which a long working history to retire earlier. When Germany decided to raise the retirement age from 65 to 67 beginning from 2012 it also introduced a new early retirement program which allowed retirement at age 65 without deductions for workers with 45 years of contributions. In 2014 the government lowered temporarily the full retirement age, i.e. without actuarial deductions, to 63 and relaxed permanently the eligibility conditions. As documented in Dolls and Krolage (2023) this early retirement scheme induced strong negative employment effects.

In this paper we analyze whether the reform was well targeted. Specifically, we study if the early retirement scheme benefits workers with high job strain or if workers with better jobs and the potential to increase employment are the main beneficiaries of this pathway. For the empirical analysis we use high-quality administrative data from the Federal Employment Agency covering the complete employment biographies of individuals. The data contains occupational information with daily accuracy which are merged to information about the job strain at the occupational level. Based on this data we estimate heterogeneous effects of the early retirement pathway and test if specifically workers with high job strain and difficult working conditions benefited and responded to the reform.

We exploit the policy variation induced by the pension reform using two complementary identification strategies. First, similar to Dolls and Krolage (2023) we use the introduction of the reform in a difference-in-differences (DiD) estimation in which we compare employment behaviour of individuals eligible for the early retirement path, i.e with more than 45 years of employment, with those who just do not qualify for the pathway. Second, in a regression discontinuity design (RDD) we investigate how individuals react to the consecutive increases of the FRA by two month steps for cohorts born after 1952. In the RDD we compare individuals from two adjacent cohorts with - besides the date of birth - very similar characteristics. To test if early the retirement program is well targeted we focus on heterogeneous employment effects by job strain and pre-reform earnings.

We find that the early retirement pathway has a significant impact on the average labor market exit age of treated individuals. Individuals leave the labor market on average six to seven months earlier due to the reform. Estimates based on IAB data are robust to specification changes and

confirm the results of Dolls and Krolage (2023). In addition we show that there is considerable heterogeneity in the treatment effect. The effect is especially pronounced for individuals with higher incomes and individuals in more demanding occupations. The effect on the exit age of the last job subject to social security contributions is slightly smaller with an earlier job exit age between 3.5 and 5 months. In addition, we show that treated cohorts change their retirement behavior in line with the change in the full retirement age. That is, the increase of the FRA successfully shifting retirement ages for the group eligible for this retirement pathway.

We contribute to the rich and growing literature on the evaluation of pension reforms and the heterogeneous response to changes in the statutory retirement age.

Using SHARE Börsch-Supan, Coppola, and Rausch (2015) offer ex-ante descriptive evidence on the likely beneficiaries of the 2014 pension reform. They show that eligible individuals expect above average pension income and are not more likely to be in bad health than those who do not qualify. Using administrative data Dolls and Krolage (2023) empirically evaluate the effects of the 2014 pension reform on average retirement claiming and labor market exit ages. In addition, they use SHARE which is linked to administrative data and descriptively show that there are no significant differences in health status between individuals qualifying for the early retirement pathway and those who do not.

Adjustments to the possible pension claiming ages often affect birth cohorts differently, thereby providing a natural experiment where some cohorts are treated by the new regulation while neighbouring cohorts are not.¹ Central outcomes under investigation are changes to the pension claiming age, employment effects as well as substitution effects into alternative retirement pathways (e.g., Staubli and Zweimüller, 2013; Manoli and Weber, 2016; Engels, Geyer, and Haan, 2017; Rabaté and Rochut, 2020). Increases in the ERA and the NRA² are likely to have different effects. Increases in ERA restrict the choice set of affected individuals and the effect on retirement ages are expected to be larger than for changes of NRA (Staubli and Zweimüller, 2013; Riphahn and Schrader, 2023).³ Ex-post evaluations have provided evidence that in fact individuals do react significantly to shifts of the ERA. In many cases ex-post evaluations of increases of the ERA or NRA have found larger effects than predicted in ex-ante simulations (Cribb, Emmerson, and Gemma Tetlow, 2016; Blundell, E. French, and G. Tetlow, 2016)⁴

¹Note that most studies identify short term effects, as the policy changes are usually not known long in advance giving individuals limited possibilities to adjust their labor market decision early on with the goal of smoothing consumption. Due to potential long term adjustments by individuals such as increased working hours in younger years, long-run responses to the reform are likely to be smaller (Staubli and Zweimüller, 2013).

²We refer to the age at which a person can take-up pension benefits without deductions as normal retirement age (NRA) or full retirement age. Early retirement with deductions is abbreviated as ERA.

³If individuals face no borrowing constraints, shifts in the ERA would theoretically not alter the age of labor market exit as individuals could bridge the time by borrowing (Eric French and Jones, 2012). In most countries it is not possible to borrow against future Social Security wealth but as Eric French (2005) has shown for the US context, borrowing constraints are not binding at relevant ages.

⁴A further relevant dimension is the question of when workers learn about changes to their respective statutory retirement ages. Since increases to the statutory retirement age correspond to reduced lifetime income, employment could be increased long before to smooth consumption over time. In this case retirement would not be postponed as much (Mastrobuoni, 2009). This depends upon individuals to be informed about their respective statutory retirement ages which is often not the case (Cribb, Emmerson, and Gemma Tetlow, 2016; Mastrobuoni, 2009). If individuals learn about their actual statutory retirement ages only shortly before their retirement age, they have little option to smooth consumption and one would expect a more pronounced labor market response directly

Studies show positive effects on the claiming age of an increasing full retirement age by cohorts (e.g., Krueger and Pischke, 1992; Mastrobuoni, 2009; Atalay and Barrett, 2015; Oguzoglu, Polidano, and VU, 2016). Mastrobuoni (2009) finds an increase in the actual retirement age of one month for every increase in the FRA of two months. Manoli and Weber (2016) study the effects of increases in the ERA in the Austrian context with a regression kink design. They find comparable results in magnitude to those of Mastrobuoni (2009) in the US. According to their preferred specification a one year increase in the ERA causes a five month higher job exit age and a six month higher pension claiming age. Seibold (2021) finds a similar effect for a German reform which increased the NRA for women (see also Engels, Geyer, and Haan, 2017; Riphahn and Schrader, 2023). Cribb, Emmerson, and Gemma Tetlow (2016) investigate an increase in ERA for women in the UK. The reform reduced the probability of individuals to be retired by 11.5 percentage points. Geyer and Welteke (2021) investigate a large increase in the ERA for women in Germany. They find a reduction of old-age pension receipt due to this reform by 27.6 percentage points and an increase in female employment by 13.5 percentage points.

We also contribute to studies that analyze heterogeneity by health status in retirement behavior. Health has early on been identified as an important determinant of retirement decisions but it is usually not directly observed in the administrative data used for retirement analyses (Rust and Phelan, 1997; Blau and Gilleskie, 2001). Alternatively, survey data or general occupation level job strain measures are used in the literature. Blekesaune and Solem (2005) combine Norwegian survey data on job strain with census and administrative data and study the relationship between job strain and retirement in an associative non-causal framework. They find a stronger relationship between physical job strain and disability retirement than for physical strain and non-disability early retirement. At the same time psychological job strain is instead found to be highly associated with non-disability early retirement. Bellés-Obrero, Jimenez-Martin, and Ye (2022) investigate the effect of an increase in the ERA on mortality in the Spanish context. They find that a one year increase in the labor market exit age led to a 4.2 percentage points higher hazard of dying for those aged 60 to 69. As a heterogeneity analysis they merge psychosocial occupation level job strain information by Kroll (2011) as is also done in our study. The effect of a delayed labor market exit on mortality is roughly double the size for individuals in psychosocially demanding jobs in comparison to those in with low psychosocial job strain. Another application of the occupation level information on job strain by Kroll (2011) in the retirement context is an analysis of Zwick et al. (2022) who study the effects of a German reform that increased the ERA of women from 60 to 63. The authors find no differences in the increase in employment of treated women exposed to low and high job strain.

The subsequent work is structured as follows: Section 2 provides an overview over the literature on evaluations of pension reforms, section 3 summarises the necessary institutional background of the "retirement with 63 reform" and the German pension system, section 4 introduces the data sources used and presents descriptive statistics of the sample used for analysis, section 5 presents estimates of the effects of the reform on labor market outcomes and section 6 investigates the reactions to the incremental increase in statutory retirement ages. Section 7 concludes.

around statutory retirement ages.

2. Institutional Background

Germany responded to the challenge posed to the pension system by rising life expectancy and falling birth rates through increasing statutory retirement ages and abolishing early retirement options. These adjustments were implemented in eleven separate reform acts between 1989 and 2017 (Hinrichs, 2021). This section will briefly describe the general system of retirement pathways in the German pension system and will provide more in-depth explanation of the 2014 pension reform. For a full description of all changes to the German pension policy since 1980 see Börsch-Supan, Rausch, and Goll (2020).

2.1. Pension benefit formula

Pension benefits depend on the income earned throughout the entire labor market career and can be calculated according to the following formula⁵:

$$PB_{T+s} = \left(\sum_{t=1}^T EP_t \right) \times PT_T \times AF_T \times CPV_{T+s} \quad (1)$$

PB are the individual's monthly pension benefits which depend on the sum of the earning points (EP), the pension type factor (PT), the age factor (AF) and the current pension value (CPV). t are the years of contribution before retirement at T and s are the years after retirement. Earning points are calculated annually by comparing the individual's gross earnings with the average gross earnings of all German employees. Earning the average gross income leads to one pension point with pension points being capped at a year specific contribution limit of around two. Pension benefits are roughly proportional to the averaged individual labor income over the entire working career. The system has some redistributive features which are mainly related to children and informal care (Börsch-Supan, Rausch, and Goll, 2020). The main source of income in old-age is the public pension system, therefore, rendering any change to the pension policy very influential for the individuals retirement decision (Riphahn and Schrader, 2023). Multiplying the sum of pension points with the current pension value yields the theoretical pension benefits which are then adjusted. The first adjustment is made depending on the type of pension being received – it equals one for old age pensions. The second adjustment by the age factor depends on the pension type and leads to a lifelong reduction of pension benefits by 0.3% for each month of retirement before the NRA of the pension type. Individuals who continue working after reaching the statutory retirement age get a bonus of 0.5% per month.

2.2. Normal and early retirement

The following discussion will focus on retirement options available to individuals used in the analysis who are born between January 1st, 1948 and January 1st, 1955. For these cohorts five old age pension types were available.⁶ The remaining types of old age pension are:

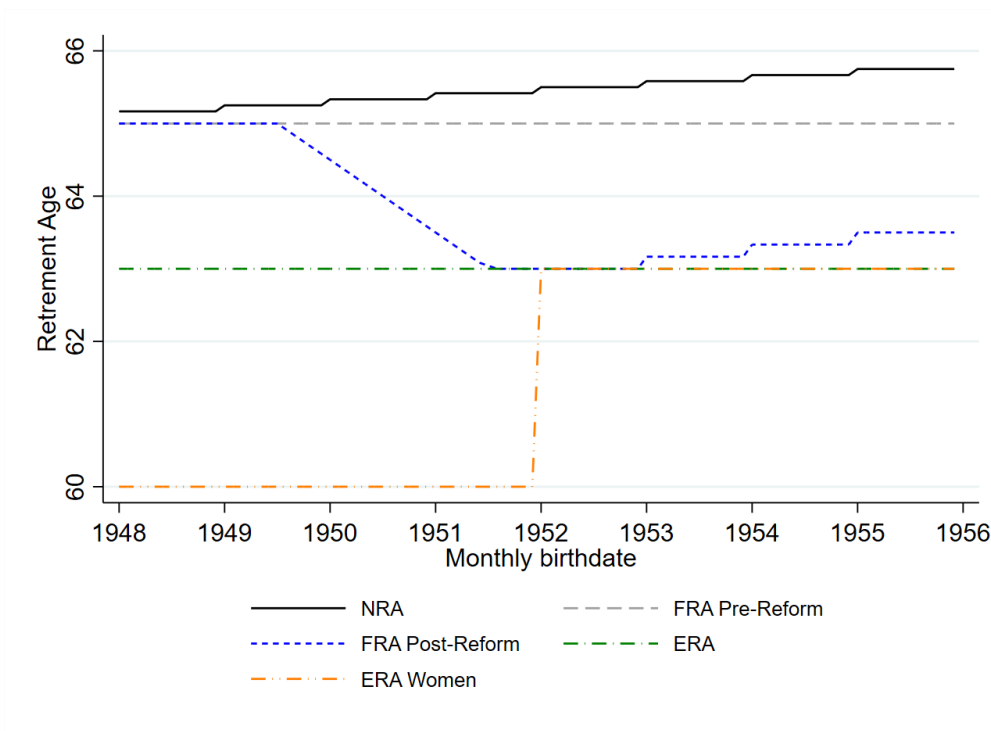
⁵Following the representation in Pfister, Lorenz, and Zwick (2018).

⁶We exclude miners and seamen who have separate pension schemes. Moreover, we exclude people who are likely eligible for disability benefits. In order to draw disability benefits individuals need to pass strict medical tests,

1. the regular old-age pension,
2. the old-age pension for the long-term insured,
3. the old-age pension for women,
4. the old-age pension for the unemployed or those under a progressive retirement plan and
5. the old-age pension for the especially long-term insured ("retirement with 63").

Regular old-age pension: Individuals qualify for the regular old-age pension if they have collected at least five contribution years over their employment biography. Individuals can retire without deductions at the Normal Retirement Age (NRA) which was set at 65 for cohorts until 1946 and has been increasing until eventually reaching 67 for cohort 1964. In figure 1 the solid black line depicts the increasing NRA.⁷

Figure 1: Retirement age thresholds by birth cohorts



Notes: The figure depicts institutional retirement ages for different pension pathways. Beginning from birth cohort 1952 onwards the pension for women was abolished, increasing their pathway specific ERA to the general early retirement age of 63. Deductions apply for retiring before the NRA or FRA and are set at 0.3% per month of earlier retirement in comparison to the NRA.

Source: §§235-237a SGB VI. Own illustration. Eligible individuals can retire at the FRA without deductions.

making this pension type an unlikely substitute for regular old age pensions (Geyer and Welteke, 2021).

⁷From cohorts 1947 until 1958 the increase is set at one month per birth year cohort. From 1959 onwards the increase happens in two month increments per birth year cohort until eventually reaching the NRA of 67 for cohort 1964 onwards.

Old-age pension for the long-term insured: Individuals with at least 35 contribution years face the same NRA as in the regular old-age pension but have the option to retire early at the ERA of 63, see the green dotted line in figure 1. For each month of the retirement age before the NRA individuals face lifelong deductions of 0.3% of their pension benefits (3.6% per year).

Old-age pension for women: Women born before 1952 with 15 years of contributions of which at least 10 had to be accrued after age 40 have a lower ERA of 60 instead of 63 years. For them the NRA is 65 and lifelong deductions of 0.3% per month between the retirement age and the NRA again have to be considered. Maximum deductions at a retirement age of 60 are therefore 18%. This type of pension was abolished starting from birth cohort 1952. The ERA for women is therefore marked by a sharp discontinuity from 60 years for those born before 1952 to 63 for those born after 1952 as depicted by the yellow dotted line in figure 1.

Old-age pension for the unemployed: Individuals born before 1952 had two more options to retire early. If they spend at least 52 weeks in unemployment after reaching 58.5 years of age and had collected 15 contribution years of which at least 8 had to be collected in the last 10 years, they could retire at age 63 with deductions. The same holds for individuals in partial retirement for the 24 months leading up to retirement. These pathways into retirement were closed for cohorts from 1952 onwards.

2.3. Pension for the especially long term insured

Individuals born before 1949 with at least 45 contribution years face a full retirement age (FRA) at which they can retire without deductions of 65 years. Besides the earlier availability the FRA differs from the NRA in that individuals working beyond the FRA only start gathering pension adjustments of 0.5% per month after they have passed their NRA which is the same as in the regular old-age pension. The pension was reformed in 2014, decreasing the FRA to 63 years and is now colloquially known as "retirement with 63". For individuals in the cohorts between July 1949 and June 1951 the reform was introduced after they reached the new FRA of 63 and before they reached the old FRA of 65. These individuals faced an effective FRA between 63 and 65. The FRA is again increasing in steps of two months per yearly birth cohort from cohort 1953 until it will eventually reach a FRA of 65 for birth cohort 1964 (compare the dotted blue line in figure 1). As part of the reform the criteria for contribution years were also broadened. Times of unemployment benefits receipt are now also counted as contribution periods, with the exception of the last two years before the FRA. This increased the number of persons eligible for the "retirement with 63" pension (Börsch-Supan, Coppola, and Rausch, 2015). Since its introduction it has been the second most important pathway into retirement after retirement at the NRA (Deutsche-Rentenversicherung, 2022). The reform was justified on equity grounds. Individuals with long employment careers, especially those performing heavy labor for many years were seen to be less likely to be able to keep working until the increasing NRA and would in consequence draw permanently reduced old-age pensions (Hinrichs, 2021; Deutscher-Bundestag, 2014). Others have framed the reform as backlash against preceding reforms that increased retirement ages (Börsch-Supan, Rausch, and Goll, 2020).

3. Data

The administrative data used in our analysis is the Sample of Integrated Employment Biographies (SIAB) which is provided by the German Federal Employment Agency (FEA).⁸ It consists of a 2% sample of the data on integrated employment biographies by the IAB which covers all individuals in any given period who participated in the labor market through one of the following ways: Individuals had an employment subject to social security contributions, were marginally employed, were receiving services, payments or participating in employment measures by the FEA or were registered at the FEA as searching for a job (Frodermann et al., 2021). Importantly the data therefore does not cover times at which individuals were self-employed, were working as civil servants or were inactive. The SIAB is process-produced data and contains information about employment status, wages as well as the receipt of unemployment benefits from the years 1975 to 2021 with daily accuracy and provides some limited additional individual information such as monthly date of birth, gender, occupation and education. Data on individuals from the former German Democratic Republic (GDR) is available since 1991.

3.1. Sample construction

Our sample is restricted to cohorts born between 1948 and 1955.⁹ Individuals born before or in 1948 were unaffected by the reform as they had already passed the NRA at the time of the reform's implementation in 2014 (Deutscher-Bundestag, 2014). As the data covers employment information until 2021, individuals born after 1955 have not yet completed their entire employment biography at the time of the analysis and are also excluded. Individuals are deleted from the data if death was the reason for the reporting of their last labor market spell. Following Pfister, Lorenz, and Zwick (2018) individuals working as seamen and miners during their employment biography are also deleted since they are covered by special occupational pension insurance.¹⁰ As the SIAB has been collected since 1975 for West-Germany and since 1991 for the former GDR it does not cover the entire life of the individuals under investigation. The number of unobserved years depends on the birth-cohort as depicted in table 1 were it is assumed that individuals start with vocational training or further education with the age of 16.¹¹ Analyzing the effects of the 2014 pension reform requires the identification of contribution years. Given the incomplete span of observation years some assumptions are necessary to determine eligibility for this retirement pathway. Years spent in school or university do not count as contribution years for this retirement pathway, accordingly depending on the educational attainment reported in the SIAB three, six or eight years starting from the age 16 are not counted as contribution years. All remaining unobserved years are assumed to be contribution years. Additionally, the SIAB provides no information on time spend on taking care of children or relatives. Both are activities

⁸Data access was provided via on-site use at the Research Data Centre (FDZ) of the FEA at the Institute of Employment research (IAB) and subsequently via remote data access. For a detailed introduction and description of the SIAB see Frodermann et al. (2021).

⁹To test the parallel trends assumption of the main difference-in-differences identification setting, cohorts 1946 and 1947 are included for the pre-trend analysis in Section 4.1.1.

¹⁰These individuals were identified by occupational codes.

¹¹The German system of vocational training usually means that individuals not pursuing a higher school or university qualification enter the labor market before the age 18 (Wolter and Kerst, 2015).

Table 1: Labor market years observed in the SIAB

Birth cohort	Reform Status	Age from which on data is available	Max. number of unobserved labor years	Labor years observed until 63
1948	Pre-Reform	27 (43)	11 (27)	37 (21)
1949	Transition cohorts	26 (42)	10 (26)	38 (22)
1950	Transition cohorts	25 (41)	9 (25)	39 (23)
1951	Transition cohorts	24 (40)	8 (24)	40 (24)
1952	Cohort fully covered	23 (39)	7 (23)	41 (25)
1953	Cohort fully covered	22 (38)	6 (22)	42 (26)
1954	Cohort fully covered	21 (37)	5 (21)	43 (27)
1955	Cohort fully covered	20 (36)	4 (20)	44 (28)

Notes: Here it is assumed that individuals start with further education or vocational training with the age of 16. The numbers in brackets represent individuals from the former GDR where data is available starting in 1991 as compared to 1975 in West-Germany.

Source: SIAB 7521, own calculations.

that earn contribution years for the "retirement with 63". Individuals from the former GDR are observed after standard childbearing ages. Since unobserved years not spent in higher education are counted as contribution years, time spent on childcare is already included in the calculations of contribution periods for these cases. Following Müller, Filser, and Frodermann (2022) labor market gaps of women that likely arise due to pregnancy are identified by health related reporting in the respective last labor market spell before the gap. The period out of the labor market for these cases is fully counted as contribution period. Note, that the method does not allow a similar identification for fathers. Labor market gaps of fathers due to childbearing are therefore not identifiable and not counted as contribution periods. Individuals who provide informal care for relatives are observed in the data if they receive unemployment benefits or continue working with reduced working hours. In these cases they are credited contribution periods. If they drop out of the labor market completely for a period, it is not possible to identify the reason for the labor market gap and therefore no contribution periods can be accredited for informal caregiving.

Following Brussig (2015), Zwick et al. (2022) and Pfister, Lorenz, and Zwick (2018) who also use the SIAB for retirement analyses, the sample is restricted to individuals who had an employment subject to social security contributions after age 55. Additionally, following Pfister, Lorenz, and Zwick (2018) individuals who are no longer part of the labor market at the age of 59 are deleted as they had no opportunity to retire early through the pension system. Labor market activity in this case encompasses unemployment and marginal employment. This restriction likely drops most individuals eligible for or drawing a disability pension.¹² Individuals eligible for an old-age pension for severely disabled persons cannot be identified in the sample and remain. To test the robustness of these sample restrictions, the main estimation in Section 4.1.2 is replicated with a sample where the aforementioned restrictions were not applied. The estimated treatment

¹²The average age of individuals entering pension due to disability has been between 50 and 54 years in the past decades Deutsche-Rentenversicherung (2022).

effect depicted in Table 15 is slightly smaller than in the main estimation, but otherwise very similar.

Table 2: Sample size after sample restrictions

Group	Number of individuals	Sample share
Panel A: No further restrictions		
Control	15,295	8.24
Treatment	38,668	20.83
Neither	131,637	70.93
Total	185,600	100.00
Panel B: Final sample restrictions		
Control	9,072	10.94
Treatment	35,353	42.65
Neither	38,468	46.41
Total	82,893	100.00

Notes: Panel A excludes individuals where death was the reason for reporting their last labor market spell and those working as seamen or miners. Panel B further excludes individuals who were no longer active in the labor market after age 59 and individuals who did not have employment subject to social security contributions in any year after age 55.

Source: SIAB 7521, own calculations.

Individuals with 45 contribution years at the age of 63 are eligible to retire without deductions and are therefore assigned to the treatment group. Individuals with 42.33 to 44.92 contribution years at age 63 could become eligible for the "retirement with 63" pension somewhere between the age 63 and the normal retirement age and following Dolls and Krolage (2023) are not assigned to either the treatment or the control group. They are, therefore, not used for the analysis in Section 4.1.2. The control group should consist of individuals who marginally do not qualify for the "retirement with 63" pension in terms of contribution years but are otherwise similar to the treatment group. Due to the described imprecise estimation of contribution years from the SIAB and lower sample size, the control group is set more broadly with individuals having 38 to 42 contribution years instead of 40 to 42.33 as in Dolls and Krolage (2023).¹³ The sample size is depicted in table 2 with the share of individuals in the treatment and control group in panel A corresponding closely to the shares of observations in the treatment and control group in Dolls and Krolage (2023).¹⁴

¹³Dolls and Krolage (2023) broaden their range of contribution years to 35 - 42.33 in an additional analysis where they have to deal with lower sample size.

¹⁴As Section 4.1.2 shows, the results of the main difference-in-difference estimation by Dolls and Krolage (2023) are matched quite closely, providing support for the assumptions made for the identification of contribution years in the SIAB data.

3.2. Job demand measures

A usual feature of administrative labor market data is that comprehensive measures of health outcomes and job burden are lacking. There are efforts to link administrative data-sets such as data by the German pension insurance with survey data such as SHARE or the SOEP which have measures of health conditions. Such linked data-sets have been used to study health outcomes of the "retirement with 63" reform but suffer from low sample size due to the linkage procedure and the small size of most survey studies (Börsch-Supan, Bucher-Koenen, et al., 2022). For example Dolls and Krolage (2023) are only able to obtain health data on 0.42% of their main analysis sample. Additionally, most of the health measures from survey data are reported by the respondents themselves. Blekesaune and Solem (2005) argue that individual level health data is influenced by the preferences for work and leisure of the individual workers and suggest using occupational level health data instead. Occupational-level data on job strains can be a valuable alternative to health outcomes from survey data. Kroll (2011) has developed an occupational-level measure from a large scale representative survey of the German Federal Institute for Vocational Education and Training (BiBB). The resulting index for job demand was validated with further health surveys. Kroll (2011) assigns answers by the BiBB survey to the dimensions physical exposure, exposure through work environment, mental exposure, time exposure and social exposure and calculates an overall index for all occupations on the scale of 1 to 10. This index is available as an overall job exposure index (OJI), a physical index (OPI) as well as a psychosocial index (OSI). The most recent version of the Kroll index was merged to the SIAB data using the 4-digit occupational classifications of the Federal Employment Agency (Härpfer and Neuhauser, 2021; Kroll, 2015). The Kroll index has been used in applications where health outcomes are unobserved, often being applied as an accompanying heterogeneity dimension. In the context of labor market outcomes see for example Giuntella et al. (2019) who use the Kroll index of physical burden in the UK context, Bellés-Obrero, Jimenez-Martin, and Ye (2022) who apply the psychosocial index in the Spanish context or Dengler (2019) who use the overall job exposure index in the German setting.

3.3. Main outcome variable: labor market exit age

In contrast to studies such as Dolls and Krolage (2023) or Engels, Geyer, and Haan (2017) who use data on retirement entries from the public pension insurance, the SIAB does not contain retirement information, but only data on the labor market biography of individuals. The main outcome variable analysed in the following sections is therefore the labor market exit age which can be determined as the age at the end of the last labor market spell in the data. Most individuals do not lower their working hours gradually around retirement ages, instead working hours are marked by a sharp drop to 0 hours worked. Analysing the extensive margin of the labor supply reaction is therefore the preferred choice in pension contexts (Blundell, E. French, and G. Tetlow, 2016). Following Lorenz, Pfister, and Zwick (2018) this labor market exit age is top-coded to a maximum of 66. Table 14 reproduces the main estimation from Section 4.1.2 without any restriction to the maximum exit age with no significant changes to the results. In the main analysis everything is counted as labor market activity with the exception of marginal employment. Marginal employment is possible and popular even after retirement, as the associated income passes the income test of early retirement pensions. The entire analysis is repeated in Appendix

A for a sample where the labor market exit age also includes marginal employment. The main results remain unchanged. See Section 4.1.3 for a discussion.

3.4. Descriptive results

Table 3 depicts descriptive information on key variables similar to those shown by Dolls and Krolage (2023) who study the same retirement reform using data from public pensions accounts. The share of females is much smaller in the treatment group in comparison to the control group. These differences exist in Dolls and Krolage (2023) as well, but are less pronounced. Individuals in the treatment group overwhelmingly obtained a vocational degree as education while a significant share in the control group holds a university degree. The shares in different labor market status before turning 63 are also comparable to Dolls and Krolage (2023) with the absolute majority being in employment and the rest splitting up into marginal employment, partial early retirement and the receipt of social benefits. Individuals in the treatment group have a higher average daily income in the five years before turning 63 than those in the control group.

Figure 2 shows the total number of individuals leaving the labor market in monthly age bins. Three main spikes can be observed, one at the age of 60 where women born before 1952 could retire with deductions, the age 63 which is the early retirement age and full retirement age for those eligible and the age 65.¹⁵ The age at which individuals with 45 years of contributions could retire without deductions was increased in two month steps beginning with the birth cohort of 1953, this explains the smaller successive spikes after age 63 in the figure. Similarly, the normal retirement age is increased by one month for each successive cohort from 65 years and two months for cohort 1948 up to 65 years and nine months for the oldest cohort included in the sample. This explains the large bunching between ages 65 and 66.¹⁶ The large number of labor market exits at 63 underscores the relevance of this age as retirement age. The successive spikes also indicates that the step-wise increase of the FRA was successful in delaying retirement, this is further analysed in Section 4.2. Figure 5 in the Appendix shows the same graph with the last employment subject to social security contributions as measure of labor market activity. It depicts the same general pattern with larger spikes at all round ages.

Table 2 displays additional descriptive information on different measures of job strain. Individuals in the treatment group have a higher average measure of overall job demand (OJI) in the five years before turning 63. This is mainly driven by stronger physical demand (OPI), whereas the psychosocial strain (OSI) is similar in the treatment and control group. A higher share in the treatment group worked in a physical occupation in the last employment subject to social security contributions before their labor market exit. Interestingly the share of individuals for whom any sickness spell could be identified in the data is smaller in the treatment group in comparison to the control group. This is likely driven by the fact that for individuals with longer labor market gaps due to illness the number of contribution years identified in the data is insufficient to assign them to the treatment group.

¹⁵For cohorts born before July 1949 the FRA for individuals with an especially long insurance record was set at 65.

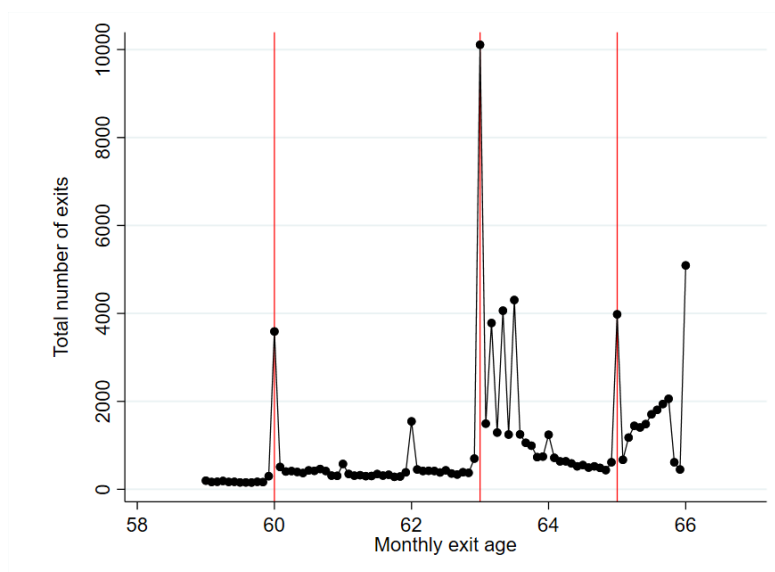
¹⁶Note that the labor market exit age is top-coded to 66 explaining the large mass of labor market exits at this age in the figure.

Table 3: Summary statistics

	Treated	Control	All
Share Female	35.6	54.9	47.3
Share East German	29.9	28.2	21.2
Share Married	80.0	71.7	73.1
Average daily income age 58-63	92.0€	85.5€	81.4€
<i>Education</i>			
None	0.17	0.36	1.04
Vocational Degree	99.57	72.31	88.50
Advanced occupational degree	0.21	4.07	2.16
University degree	0.05	23.25	8.29
<i>Labor market before turning 63</i>			
Non / Unkown	0.64	2.61	2.09
Employed	61.55	59.37	60.89
Marginally Employed	9.14	9.35	9.50
Short-term Unemployed	0.45	0.72	0.77
Receiving social benefits	11.72	15.12	14.51
Partial early retirement	16.51	12.83	12.24
Observations	35,353	9,072	82,893

Notes: This table provides summary statistics of the sample used for analysis. It includes information on the last labor market spell of individuals from birth cohorts 1948 - 1955. With the exception of daily income all results are expressed as shares. See section 3.1 for details on the treatment and control group assignment and the sample restrictions being applied.

Figure 2: Bunching of labor market exit ages



Notes: The labor market exit age is the age at the end of the last labor market activity. Any activity except marginal employment is counted as labor market participation. The sample is restricted to individuals who had an employment subject to social security contributions at age 55 and any labor market participation after age 59. Exit ages are top-coded to age 66.

Table 4: Descriptive statistics of job strain

	Treated	Control	All
Average OJI age 58-63	5.480 (2.787)	5.033 (2.753)	5.557 (2.782)
Average OPI age 58-63	5.428 (2.838)	4.818 (2.737)	5.391 (2.803)
Average OSI age 58-63	5.274 (2.794)	5.280 (2.859)	5.533 (2.853)
Share physical last employment	61.2	46.2	56.5
Share ever sick	10.7	17.2	14.9
<i>Skill requirement in last job before turning 63</i>			
Simple	13.09	16.17	18.16
Professional activity	66.51	49.32	58.51
Complex activity	12.95	13.19	11.40
Highly complex activity	7.45	21.32	11.92
Observations	35353	9072	82893

Notes: This table provides descriptive statistics on different job demand measures. OJI is a general measure of job demand, OPI measures the physical strain in a job and OSI the psycho-social strain. All three measures run from 1 to 10, 10 being the highest job demand. Physical last employment is the share of individuals whose last occupation subject to social security contributions was of a physically demanding nature. The share sick measures the share of individuals with any labor market gaps in the data that can be attributed to illness. The skill requirement refers to the last employment before turning 63. The assignment to the treatment and control group is the same as in section 3.1.

4. Results

4.1. Difference-in-Differences Estimation

The 2014 pension reform differently affects birth cohorts born from July 1949 and onwards. This provides a natural experiment where the neighbouring birth cohorts 1948 and before are not treated but are otherwise similar to those affected by the reform. We will first explain how this setting can be exploited with a difference-in-differences design and what identifying assumptions are needed to determine the causal effect of the reform. Consecutively, the results of the main analysis are discussed followed by additional robustness and heterogeneity analyses.

4.1.1. Identification strategy

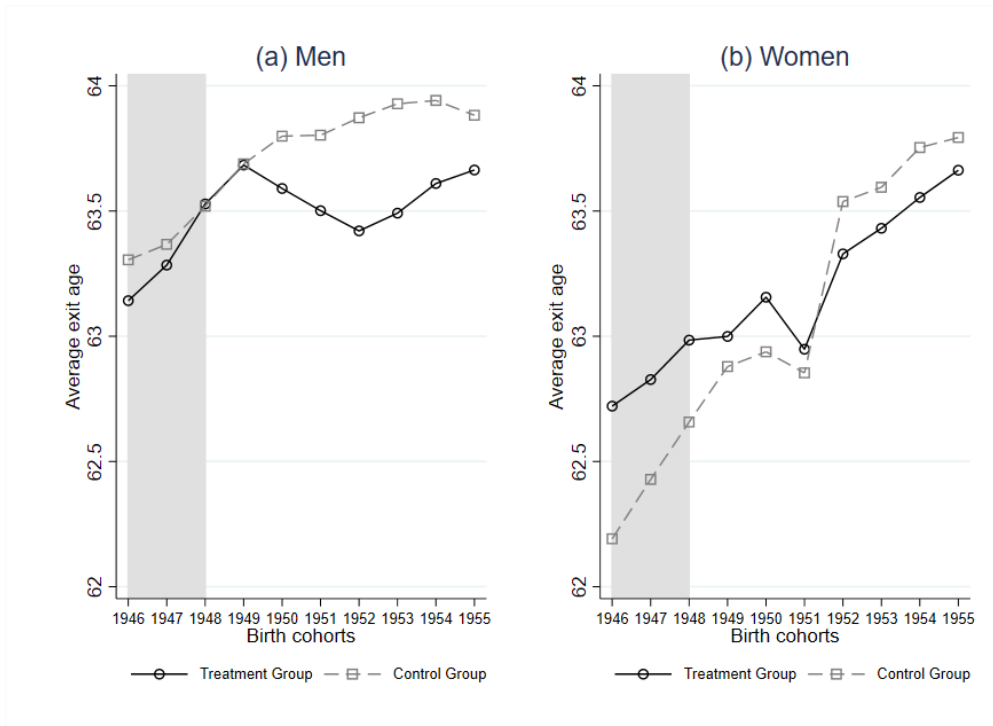
Only individuals with at least 45 contribution years are eligible to retire at the FRA. Individuals from the same cohort that have marginally fewer contribution years have had a similarly strong labor market attachment but do not qualify to retire at the FRA.¹⁷ They therefore act as a suitable control group within the same cohort. Additionally, individuals in the cohort 1948 can be assigned to the control and treatment groups but did not have the option to retire at the FRA. They therefore represent the pre-treatment cohort.

Figure 3 depicts the average labor market exit age of the treatment and the control group for each yearly birth cohort. The birth cohorts 1946 and 1947 are also included to provide descriptive evidence that the trends of treatment and control group moved in parallel in the pre-treatment periods. The shaded area of the plot marks cohorts that were not yet covered by the "retirement with 63" reform. The cohorts 1949 to 1951 are transition cohorts for which the FRA was lowered from age 65 by one to up to 24 months depending on their month of birth. The FRA is lowered to 63 years for those born after June 1951. Beginning with the 1953 cohort the FRA increases in steps of two month for each consecutive cohort. The trends in the labor market exit age move in parallel until 1949 where for men the average exit ages start to fall in the treatment group in contrast to the continued increase in the control group. For women, during the treatment periods, the growth in average labor market exit ages in the control group is stronger than in the treatment group; starting from a lower level it eventually overtakes the exit age in the treatment group for the first time in cohort 1952.¹⁸

¹⁷Individuals with 42.33 to 44.92 contribution years at age 63 could become eligible for the "retirement with 63" somewhere between the age 63 and the normal retirement age and are not used in the analysis. See section 3.1 for the assignment into treatment and control.

¹⁸The pronounced jump in average exit ages for women in both the treatment and the control group between cohorts 1951 and 1952 can be explained by the abolishment of the women's pension.

Figure 3: Average labor market exit ages of men and women by treatment group



Notes: The figure plots the average labor market exit age for the birth cohorts 1946 - 1955 separately for men and women in yearly bins. Exit ages are top-coded to age 66. The shaded area marks the pre-reform periods, cohorts 1949 to 1951 are transition cohorts and the FRA is fully lowered to 63 for cohort 1952. Beginning with cohort 1953 the FRA increases in 2 month steps for each consecutive cohort.

The causal effect of the reform can be estimated with the following empirical model which is estimated separately for each birth cohort in comparison to the pre-treatment birth cohort 1948:

$$ExitAge_{iec} = \alpha + \delta Treat_e + \gamma Post_c + \beta_c Post_c \times Treat_e + X'_{ic} \theta + \epsilon_{iec} \quad (2)$$

Where α marks the intercept, i marks the individual, e whether the individual is eligible for the "retirement with 63" pathway and therefore belongs to the treatment or control group and c indicating the birth cohorts. The dependent variable is the labor market exit age as defined in section 3.3 and measured in years. The coefficient of interest is β_c which measures the treatment effect of the "retirement with 63" reform on the individual cohort's average labor market exit ages. $Treat_e$ captures group fixed effects for the treatment and control group while $Post_c$ captures birth cohort fixed effects. In X_{ic} additional covariates are included that account for differences in labor market attachment between the treatment and control group even in the absence of treatment. In the main specification the following variables are included as controls: gender, education, labor market status before turning 63, average daily wage in the five years before labor market exit and a dummy for having grown up in the former GDR.

β_c identifies the causal effect of the reform for each cohort if there are no time-varying differences between the treatment and the control group and trends would therefore have moved in parallel in the absence of the treatment. While this cannot be tested explicitly, differences in trends can be tested for in cohorts that were not yet covered by the reform. Figure 3 highlights that the treatment and control group seem to be following similar trends for the cohorts 1946 to 1948, all of which were not treated by the reform. This is tested for explicitly in a placebo test where cohorts 1947, 1948 and 1949¹⁹ are assigned the treatment status and outcomes are compared with respect to the pre-treatment cohort 1946. Table 11 in the Appendix depicts the results of this placebo test. No significant treatment effect can be found for any of these cohorts at standard significance levels providing support that the parallel trends assumption holds in the identification setting.

4.1.2. Main results

Table 5 provides estimation results of the treatment effect on average labor market exit ages. Panel A depicts a specification without control variables, while the estimation in Panel B controls for gender, education and the labor market status before turning 63. Finally, panel C additionally also controls for the average daily wage in the five years before labor market exit and a dummy for having grown up in the former GDR. Results are largely robust to the inclusion of relevant control variables. The outcome variable is the average labor market exit age in years and the results are depicted for each birth cohort separately. The treatment effect of the main specification in panel C can therefore causally be interpreted as a reduction in the average labor market exit age by between six to seven months for the fully treated cohorts 1952 to 1955. The results are statistically significant at the 1% level. For the transition cohorts 1949 to 1951 the effect is smaller as was to be expected and lies between one to 3.5 months while not always being statistically significant. For later cohorts the effect lies between six to seven months. Putting these results in relation to the decrease of the FRA due to the reform, a one year decrease in the full retirement age therefore leads to an earlier labor market exit age by three to 3.5 months. These results are comparable to Dolls and Krolage, 2023 whose estimates of earlier labor market exit age also fall into the range between three to four months for a one year decrease of the FRA.

To illustrate the development of the treatment effect over time and allow for easier comparison to the robustness analysis in Appendix A the treatment effect is plotted in Figure 4 with associated confidence intervals at the 5% level. The figure clearly shows that the treatment effect grows over time. As highlighted in Figure 1 the FRA remained at 65 for individuals born before July in 1949 and decreased by only one to six months for those born after July in the same year. Similarly, the FRA was lowered to between 64.5 years and 63.583 years for individuals in cohort 1950. It is therefore to be expected that no large treatment effect can be found for these cohorts. Cohort 1951 is the final transition cohort with a FRA between 63 and 63.5 years. Its associated treatment effect lies in between the effect of earlier transition cohorts and cohorts that are fully treated. From cohort 1952 onwards the effect remains mostly stable and is slightly decreasing over the cohorts as the FRA is raised by two months per cohort beginning from 1953 onwards. This decrease is

¹⁹Half of the individuals in cohort 1949 are not treated, while the other half has a maximal reduction in the FRA of 6 months. The cohort is therefore also included in the pre-trend analysis.

Table 5: Difference-in-differences - Main Specification

	1949	1950	1951	1952	1953	1954	1955
Panel A: No control variables							
Treat. Effect	-0.088	-0.196**	-	-	-	-	-
	(0.089)	(0.084)	0.301***	0.593***	0.584***	0.556***	0.474***
Observations	10,173	10,460	10,425	10,863	10,898	10,715	10,927
Panel B: Medium set control variables							
Treat. Effect	-0.089	-0.155*	-	-	-	-	-
	(0.087)	(0.083)	0.298***	0.629***	0.575***	0.610***	0.616***
Observations	10,134	10,424	10,394	10,834	10,872	10,687	10,901
Panel C: Full set of control variables							
Treat. Effect	-0.109	-0.096	-	-	-	-	-
	(0.081)	(0.076)	0.310***	0.577***	0.542***	0.515***	0.511***
Observations	9,911	10,218	10,193	10,649	10,687	10,512	10,728

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the labor market age in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier labor market due to the reform. Control variables in panel B include: gender, education and the labor market status before turning 63. Control variables in panel C additionally include the average daily wage in the five years before labor market and a dummy for having grown up in east Germany. See section 3.1 for sample restrictions. Standard errors are depicted in brackets.

*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$.

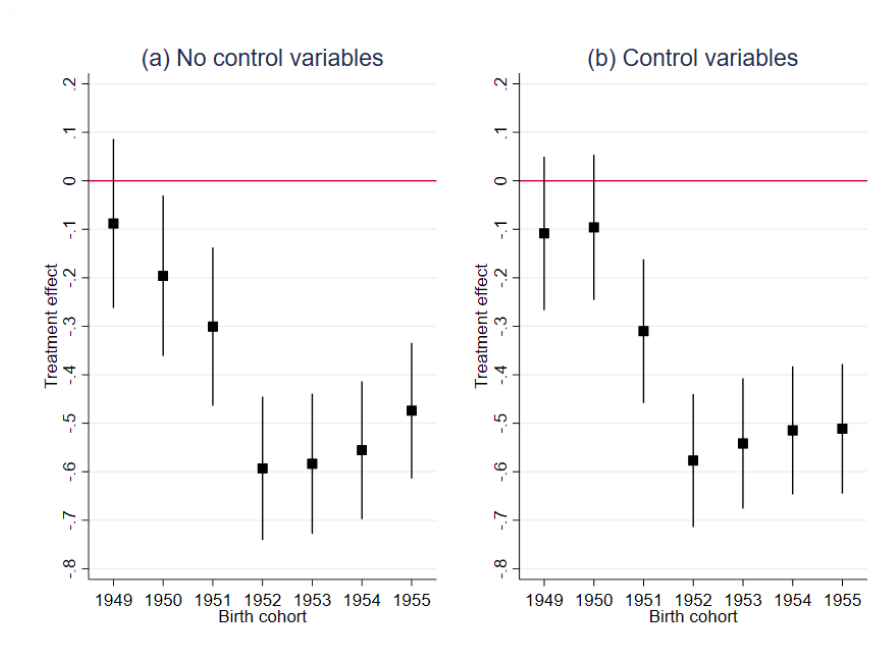
more pronounced when no control variables are included in the estimation.

4.1.3. Robustness

As described in Section 3.1 multiple sample restrictions are applied for the main sample. In the following, the effect of these restrictions on the main results of the difference-in-differences estimation is investigated, providing evidence that the outcomes are not sensitive to the sample restrictions chosen. All associated tables and figures can be found in Appendix A.

To begin, the restriction that marginal employment does not count in the labor market exit age is relaxed. Figure 9 depicts the associated trends of average labor market exit ages for men and women in the treatment and control group. In contrast to figure 4 from the main specification the average labor market exit ages are higher. Nonetheless, the same general trend can be observed over the cohorts. The exit age for women increases more strongly in the control group than in the treatment group and for men the exit ages in the treatment group begin to decrease from 1949 onwards while they continue to increase in the control group. The estimated treatment effects

Figure 4: Treatment effect by birth cohort



Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the labor market exit age in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier labor market exit due to the reform. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before the labor market exit and a dummy for having grown up in east Germany. See Section 3.1 for sample restrictions. Confidence intervals are depicted at the 5% level.

in Table 12 are slightly lower for cohorts 1951 and 1952 but remain largely unchanged for the other cohorts. Figure 10 reveals the same development of the treatment effect over time, being insignificant for the transition cohorts 1949 and 1950, increasing in cohort 1951 and remaining constant for the fully treated cohorts 1952 and onwards.

As discussed in Section 3.1 individuals from the former GDR are observed only from 1991 onwards. Therefore the assumption that unobserved years not spend in education are contribution years weighs more heavily. Figure 11 plots the average labor market exit age as in the main specification but for a sample where all individuals from the former GDR are deleted. No distinguishable difference to the plot in the main sample specification can be determined. Correspondingly, the results from an estimation of the empirical specification in 2 depicted in Table 13 yields very similar results. Due to the reduced sample size point estimates are estimated less precisely.

Following Lorenz, Pfister, and Zwick, 2018 the labor market exit age is top-coded to 66. Relaxing this specification in Table 14 yields the same results as in the main specification with the only difference being that the point estimate for cohort 1949 is now statistically significant at the 10% level.

Finally, as described in Section 3.1 individuals who did not have a labor market spell after age 59 are deleted in the main sample. Additionally, only individuals are included who had an employment subject to social security contributions at any age after 55. Using a sample without these restrictions and re-estimating the main empirical specification yields point estimates that are slightly smaller than in the main specification as depicted in Table 15. Point estimates are reduced by around one month for all cohorts. The general pattern and direction of the treatment effect remains the same however.

4.1.4. Heterogeneity

In this section two forms of heterogeneity analysis are undertaken. In the first part the sample is split by relevant characteristics and the main specification is estimated separately for each sub-sample. In the second part the exit age of the last job subject to social security contributions is used as the outcome variable of interest and the same analysis as in 4.1.2 is undertaken.

4.1.5. Heterogeneity by characteristics

As a first dimension of heterogeneity the gender of individuals is taken into consideration by estimating the main specification separately for men and women. As Table 16 shows no significantly different effects between men and women can be found when controlling for other individual characteristics. This is in line with Dolls and Krolage, 2023 who also find no significantly different effect of the "retirement with 63" reform on men and women.

Börsch-Supan, Coppola, and Rausch, 2015 provided descriptive evidence that the likely beneficiaries of the "retirement with 63" reform earned more income throughout their employment biography in comparison to individuals who do not qualify for the pension pathway but also had strong labor market attachment. They note that the reform might lead to a redistribution from the low income group to the high income group since eligible individuals have more pension entitlements and can enjoy them for a longer period due to the reform while others have to continue working and can draw their benefits for a shorter time. As a further dimension of heterogeneity it is investigated whether there are differences in the response to the reform between individuals with different incomes. Two financial effects are likely to drive the decision making of individuals with different incomes. Those with low income might feel the need to continue working despite the option to retire at the FRA as they can earn more pension points, thereby increasing their pension for the remainder of their life. Also, since the pension level is closely tied to the income earned throughout the employment biography, their pension income might be too low to secure their current standard of living (Hinrichs, 2021). From this perspective one would expect a stronger response to the reform by the high income group. At the same time, those with high incomes are more likely to be able to retire at their chosen age, regardless of the reform as they are less liquidity constraint and more able to smooth consumption through their savings (Eric French and Jones, 2012). From this perspective high income individuals should react less strongly to the reform. The SIAB provides a measure of income in the form of the gross daily salary for individuals with employment and of the daily benefit rate for those receiving social security benefits (Frodermann et al., 2021). Using the average daily gross income in the ages 58 up to 63 individuals are assigned into a group that earns below the median income and a group that

earns more than the median daily income.²⁰ Estimates are obtained for each group separately and reported in Table 6.²¹ The treatment effect for individuals with above median incomes is much stronger than the effect on individuals with median or lower incomes. For the fully treated cohorts these differences amount to between two to four months of earlier labor market exit for the higher income group in comparison to the lower income group. These results provide tentative evidence that the likely beneficiaries of the reform are not those targeted by policy makers, but instead individuals with more financial resources. It also suggests that of the two financial effects discussed above, the first seems to dominate. The retirement expectations might still be strongly influenced by changes in the statutory retirement age even for individuals with financial resources sufficient to be able to smooth consumption from their chosen retirement age in the absence of the reform (Coppola and Wilke, 2014).

Table 6: Difference-in-differences - Income dimension

	1949	1950	1951	1952	1953	1954	1955
Panel A: Below or equal Median							
Treat. Effect	-0.060	0.000	-	-	-	-	-
	(0.113)	(0.110)	0.297*** (0.107)	0.603*** (0.099)	0.471*** (0.100)	0.476*** (0.099)	0.447*** (0.099)
Observations	5,416	5,478	5,368	5,502	5,417	5,242	5,270
Panel B: Above Median							
Treat. Effect	-0.191*	-	-	-	-	-	-
	(0.110)	0.355*** (0.102)	0.443*** (0.104)	0.711*** (0.096)	0.780*** (0.093)	0.804*** (0.092)	0.787*** (0.095)
Observations	4,712	4,942	5,020	5,328	5,448	5,438	5,621

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample of individuals with an average daily wage in the ages 58 to 63 below the median and individuals with the median wage or above. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

A central goal of the "retirement with 63" reform was to enable those individuals to retire earlier who were employed in physically demanding occupations and are in danger of not reaching their NRA in employment due to the demanding nature of their job (Hinrichs, 2021). Dolls and Krolage, 2023 investigate the "retirement with 63" reform by building similar treatment and control groups as is done in this work and compare the self reported health status of individuals. They find no significant differences in the share of individuals with long term illness or in the

²⁰Observations are weighted with the respective length of the labor market spell. The median is calculated based on a sample of individuals that belong to either the treatment or the control group. Income of individuals belonging to neither group is not taken into consideration.

²¹These estimates do not change when the income of the last job subject to social security contributions is used instead. Results with this outcome variable are not displayed for the sake of brevity. Income is not used as a control variable in these estimations.

share of individuals with a positive health status. However, their sample of individuals with health information is very small impeding additional analyses with respect to the health dimension. In the SIAB no health information is available. Instead, the Kroll index is merged to the sample via occupational codes as a measure of job strain on the occupational level. See Section 3.2 for a discussion of the index which ranges from 1 to 10 with 10 being the highest job strain. An average is calculated of the general job demand index (OJI) of occupations pursued in the age range 58 up to 63.²² Individuals are assigned into the group of low job demand if their average OJI value is below or equal to five and to the high job demand group if the value is above five. As the purely descriptive Table 17 in the appendix shows, individuals with high job demand retire slightly earlier than those with low job demand in both the treatment and control group.²³ Not only is the difference very small, due to sample selection it is also unclear what kind of differences one would expect from a theoretical perspective. On the one hand individuals with high job strain are likely to be unable to keep on working as long as individuals with low job strain. On the other hand, individuals self select into their employment. Individuals that are still active in a demanding job after age 58 likely differ in many attributes from individuals that are not and could be better equipped to continue working despite the job strain.

Table 7: Difference-in-differences - Dimension of job strain

	1949	1950	1951	1952	1953	1954	1955
Panel A: Low Demand							
Treat. Effect	-0.088	-0.117	-0.236**	-	-	-	-
	(0.107)	(0.103)	(0.103)	0.482***	0.382***	0.383***	0.345***
				(0.094)	(0.091)	(0.091)	(0.094)
Observations	4,844	4,878	4,827	5,095	5,251	5,144	5,289
Panel B: High Demand							
Treat. Effect	-0.067	-0.045	-	-	-	-	-
	(0.124)	(0.116)	0.350***	0.631***	0.681***	0.592***	0.638***
			(0.112)	(0.105)	(0.105)	(0.102)	(0.100)
Observations	4,987	5,265	5,292	5,489	5,352	5,273	5,344

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only individuals with an average general job demand measure in the age 58 to 63 of 5 or lower (low demand) or higher than 5 (high demand). Standard errors are depicted in brackets.

*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

To investigate whether the reform affects both groups differently, the main estimation from 2 is performed for individuals with low or high job demand separately and reported in Table 7. The

²²Observations are weighed with the respective length of the labor market spell. Periods of unemployment or labor market receipt do not have an occupational code and are therefore not included in the calculation.

²³This pattern is less clear for the exit age out of the last employment subject to social security contributions where in the control group individuals with high job demand exit sooner, while in the treatment group they exit slightly later.

treatment effect is consistently higher in the group with high job demand who retire around 3.5 months earlier due to the reform in comparison to individuals in the low job demand group. This difference becomes slightly smaller but remains significant when the group formation instead follows Zwick et al., 2022 who only use job demand from the last employment subject to social security contributions and form the groups more narrowly. Table 18 depicts these results where the low demand group consists of individuals with an OJI of below or equal to 3 and the high demand group of individuals with an OJI of above or equal to 8. Zwick et al., 2022 investigate heterogeneity in the labor market response of women affected by a strong increase in the ERA and do not find differences between individuals in occupations with low and high job strain. The effects identified in Table 7 therefore indicate that the finding in Zwick et al., 2022 cannot be transferred to the present environment.

Job demand can be further disentangled into physical job demand (OPI) and psychosocial job demand (OSI). Comparing the treatment effect between low and high demand groups for these job measures reproduces the fact that individuals in employment with high job demand react stronger to the reform than individuals with low demand. The differences between the groups with low and high job demand are comparable for the physical and psychosocial job strain measure. It therefore does not seem like one of both is especially driving the effects.

An alternative measure of job strain is the share of individuals with long term illness. Individuals with past long term illness might have worse health in general and could react more strongly to the reform as their labor market opportunities are reduced. In process produced data usually only the receipt of sickness benefits is recorded. This benefit type is available for individuals who are sick for longer than six weeks.²⁴ Börsch-Supan, Coppola, and Rausch, 2015 find that individuals eligible for the "retirement with 63" have on average shorter or equal periods of long term illness than individuals who have strong labor market attachment but are not eligible. The SIAB data partially allows for the identification of spells of long term illness. It contains the reason why a labor market spell was reported which is usually due to the mandatory end of the year reporting. In the case of long term illness the reason for reporting the last labor market spell before the gap in the employment biography can be a tag for illness. Out of these labor market gaps due to illness a variable is created that marks individuals for whom no gap due to illness could be identified in the employment biography and individuals for whom at least one gap due to illness was identified. The main specification from 2 is then estimated separately on a sample where no gaps due to illness were identified and on a sample where at least once the individual dropped out of the labor force due to illness.²⁵ Table 19 depicts the resulting estimates which are rather imprecisely estimated for the group with labor market gaps due to illness as the sample size is low. The treatment effect in this group is roughly double the size in comparison to the group where no such gaps could be identified. Due to the selection effect into the eligible group these results only provide tentative evidence that the reform successfully offers individuals with bad health an option to retire earlier. Individuals with more serious health conditions are unlikely to be able to qualify for the "retirement with 63" pension and are likely to exit the labor market through alternative

²⁴Sickness benefits are also recorded when individuals receive inpatient treatment or are absent from work in order to care for a child or relative.

²⁵Note that individuals with longer labor market gaps due to illness are assigned neither to the treatment nor the control group as they lack sufficient contribution years.

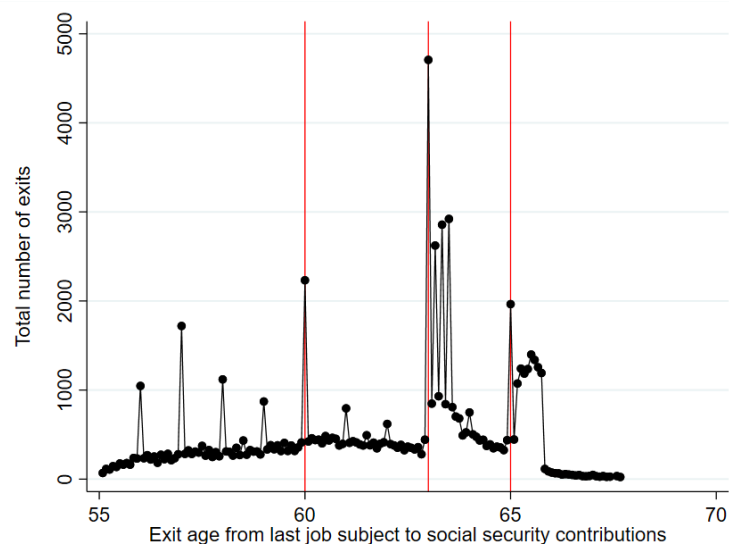
retirement pathways. The effect found in Table 19 is therefore driven by individuals with health conditions that did not obstruct them from their strong labor market participation until age 63.

4.1.6. Alternative outcome variable

For the financial sustainability of the pension system it is not only of interest how much sooner individuals exit the labor market due to the reform but also whether this effect is different for the exit age of the last employment subject to social security contributions as this form of labor market activity constitutes the main source of revenue for the pension system. The main analysis is therefore repeated with the exit age in the last job subject to social security contributions as the outcome variable. For the sake of brevity this will be referred to as job exit age in the following section.

Figure 5 plots the total number of job exits by monthly exit age revealing small spikes at all round ages as well as larger spikes that reproduce the pattern of the main analysis. Large spikes occur at the ages 60, 63 and 65 with staggered smaller spikes after the latter two.

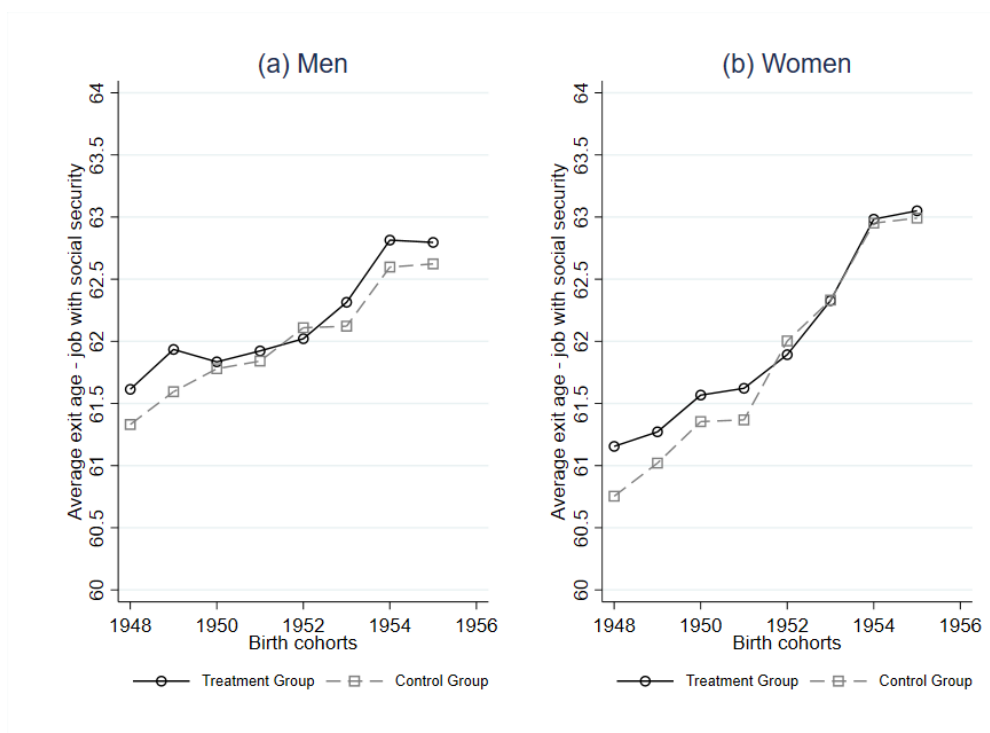
Figure 5: Bunching of exits from jobs subject to social security contributions



Notes: Job exit age is the age measured in months at the end of the last employment subject to social security contributions. As in the main analysis the sample is restricted to individuals who had an employment subject to social security contributions after age 55 and any labor market participation after age 59. Exit ages are top-coded to age 66.

Plotting the average age at the job exit in yearly bins separately for the treatment and the control group by gender reveals a strong upwards trend for both groups and genders. There are initial differences in the levels of the treatment and the control group for older birth cohorts that diminish over time. For women the treatment and control group share the same average job exit age for cohorts 1953 and later, while for men the treatment group maintains a higher average job exit age. In contrast to the main specification of labor market exit ages there is no downwards trend in exit ages of men for the transition cohorts.

Figure 6: Average job exit ages of men and women by treatment group



Notes: The figure plots the average exit age of the last employment subject to social security contributions for the birth cohorts 1948 - 1955 separately for men and women in yearly bins. Exit ages are top-coded to age 66. Cohort 1948 is the pre-reform cohort, cohorts 1949 to 1951 are transition cohorts and the FRA is fully lowered to 63 for cohort 1952. Beginning with cohort 1953 the FRA increases in 2 month steps for each consecutive cohort.

Estimating the empirical difference-in-differences specification from 2 with the exit age of the last job subject to social security contributions as dependent variable but an otherwise unchanged identification setting yields cohort respective estimates reported in table 8. The estimated treatment effect is smaller for all cohorts in comparison to the effect on the labor market exit age. The difference varies by cohort between half a month up to 2.5 months. Notwithstanding, the reform had a significant effect on the average exit age in the last job subject to social security contributions, lowering the exit age by around five months. This has implications for the fiscal costs of the reform which are much higher when individuals are induced to leave employment subject to social security contributions earlier.

In summary, the "retirement with 63" reform had a significant impact on the average labor market exit age of treated cohorts. Individuals leave the labor market on average six to seven months earlier due to the reform. Estimates based on IAB data are robust and confirm the results of Dolls and Krolage (2023). In addition we can show that there is considerable heterogeneity in the treatment effect. The effect is especially pronounced for individuals with higher incomes and individuals in more demanding occupations. The effect on the exit age of the last job subject to social security contributions is slightly smaller with an earlier job exit age between 3.5 and 5

Table 8: Difference-in-differences - Job exit age

	1949	1950	1951	1952	1953	1954	1955
Outcome variable: Exit age at last job subject to social security contributions							
Treat. Effect	0.022	-0.060	-0.167*	-	-	-	-
	(0.102)	(0.097)	(0.097)	0.473***	0.499***	0.303***	0.404***
Observations	9,251	9,522	9,382	9,743	9,667	9,424	9,510

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the exit age from the last employment subject to social security contributions in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier job exit due to the reform. Control variables include gender, education and the labor market status before turning 63, the average daily wage in the five years before job exit and a dummy for having grown up in east Germany. See section 3.1 for sample restrictions. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

months.

4.2. Regression-Discontinuity Analysis

As a central element of the "retirement with 63" reform the FRA was reduced to 63 years for individuals born after June in 1951 or in the year 1952. The entire reform was widely publicised and framed around the age of 63 although only one and a half birth cohorts could actually retire without deductions at this age. For anyone born after 1952 and eligible for this pension pathway, the FRA gradually increases in two month steps until eventually reaching a FRA of 65. At the same time the option to retire at the ERA of 63 with deductions remains. There is a strong financial incentive for eligible individuals to delay pension claiming and to continue participating in the labor market in order to retire slightly later but without deductions at the FRA. For example, for eligible individuals in the birth cohort 1953 lifelong deductions of 9.3% apply when retiring at the early retirement age of 63, while no such deductions have to be borne when retiring two months later at the FRA of 63.17. The larger the difference between the FRA of the "retirement with 63" pathway and the ERA of 63, the longer individuals need to prolong their working careers to be able to retire at the FRA. In parallel the NRA is also increasing for each consecutive birth cohort, but only in one month steps for the cohorts observed in the sample.²⁶ In most pension pathways a lifelong reduction of pension income of 0.3% has to be considered by individuals retiring before the NRA. Each consecutive birth cohort therefore faces higher potential deductions due to the increasing NRA. The increases of the FRA and the NRA were both implemented with the goal of delaying pension claiming ages. As described, the incentive to shift the retirement claiming age is much stronger for the increasing FRA than for the NRA.

Section 4.1 has shown that the lowering of the FRA from 65 to 63 had strongly negative effects on the average labor market exit age as well as on the exit age of the last employment subject to social security contributions. It remains an open question whether the decrease of the generosity of the "retirement with 63" pathway is successful in shifting eligible individuals into later retirement ages. Likewise it is unclear whether individuals react to the increasing NRA. The NRA and the FRA are increased by one or two months respectively at the beginning of each year. This provides small discontinuities between birth cohorts around which individuals likely do not differ in characteristics besides the statutory retirement ages. A regression discontinuity setting allows to exploit this setting in order to estimate the causal effect of the increase in the FRA or the NRA on average labor market exit ages.

The following section will first provide descriptive evidence of the discontinuities and present an empirical framework to estimate the causal effect of the increase in FRA and NRA. Thereafter estimation results are presented and discussed.

4.2.1. Identification strategy

For the analysis the same sample as described in Section 3.1 is used. Individuals are assigned into a group eligible for the "retirement with 63" at the FRA²⁷ and a group of everyone else. For both groups the average labor market exit age is plotted in monthly bins of the birth-year and

²⁶From cohorts 1947 until 1958 the increase is set at one month per birth year cohort. From 1959 onwards the increase happens in two month increments per birth year cohort until eventually reaching the NRA of 67 for cohort 1964 onwards.

²⁷As before these individuals have gathered equal to or more than 45 contribution years.

Figure 7: Average labor market exit age by monthly birth cohorts excluding the eligible group



Notes: Each dot represents the mean labor market exit age per bin of monthly birthdate. The red solid line is the linear trend for each yearly birth cohort, the dotted line marks the normal retirement age. All individuals with less than 42 contribution years are included in the sample which underlies the same sample restrictions as discussed in section 3.1.

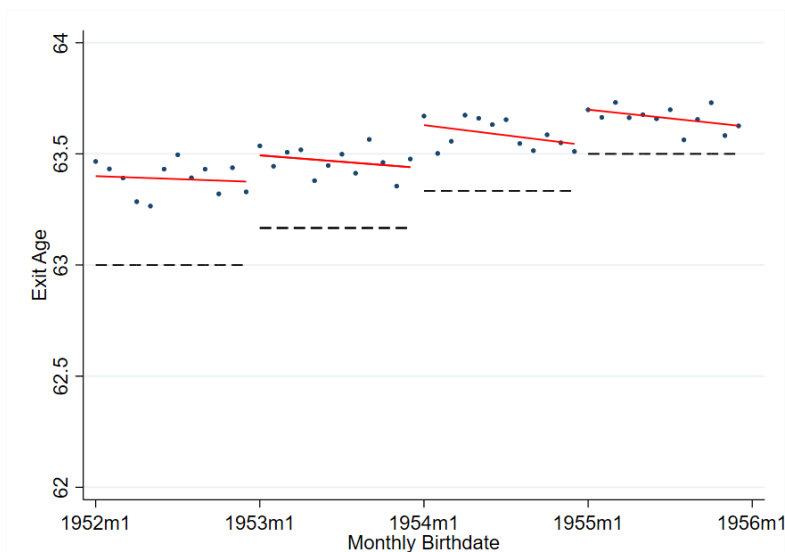
month. For each birth year cohort a linear trend is displayed by fitting a linear regression model to the observations. Additionally the NRA or FRA is displayed as a dotted line in the figures. In the sample of those not eligible for the "retirement with 63" a clear upwards trend in the average retirement ages is visible in Figure 7. The average retirement age increases from slightly above 63 to 64 years. In the same time frame the NRA was increased by 7 months from 65.17 to 65.75 years. The only discontinuity in exit ages clearly visible is around January 1952, which is the time from which on the retirement option of women's pension was abolished. In the sample of individuals eligible for the "retirement with 63" the level of the labor market exit age starts slightly higher around 63.4 years and increases step-wise with each birth cohort as depicted in figure 8. The discontinuities are more pronounced and seem to be of similar magnitude as the increase in the FRA which increases from age 63 to 63.5 years.

To test explicitly whether the increases of average retirement ages between cohorts are due to the changes to the associated statutory retirement ages a regression-discontinuity approach with the following empirical model is pursued:

$$ExitAge_i = \alpha + \beta D_i + \gamma_0 f(z_i - c) + \gamma_1 D_i f(z_i - c) + X_i' \theta + \epsilon_i \quad (3)$$

Where α marks the intercept, $D_i = 1$ if an individual was born on or after the cutoff c which marks the January of the respective birth cohorts from 1949 to 1955, therefore $c \in \{1949, 1950, 1951, 1952, 1953, 1954, 1955\}$ for the NRA analysis and $c \in \{1953, 1954, 1955\}$ for the FRA analysis. Only the two birth year cohorts directly around the cutoff c are used for the estimation of the empirical model in 3. The estimation is therefore repeated for each cutoff.

Figure 8: Average labor market exit age by monthly birth cohorts in the eligible group



Notes: Each dot represents the mean labor market exit age per bin of monthly birthdate. The red solid line is the linear trend for each yearly birth cohort, the dotted line marks the full retirement age. Only individuals with 45 or more contribution years are included in the sample, since only they are eligible to retire at the FRA. The sample underlies the same sample restrictions as discussed in section 3.1.

The yearly month of birth z_i is expressed in difference to the cutoff c . The running variable is included as a linear trend in the form of $f(z_i - c) = z_i - c$ in the baseline specification. To test for potential functional form miss-specification the same model is also estimated with a quadratic trend in table 10. Slopes before and after each cutoff are allowed to differ. X'_{it} controls for gender, having grown up in east Germany and a non-parametric specification of the average daily wage in the age range 58 to 63 by including dummies for belonging to the different wage quartiles. The dependent variable $ExitAge_i$ as defined in section 3.3 is measured in months in this analysis. The treatment effect of interest β therefore directly has a monthly interpretation.

Two assumptions are necessary for this identification approach to hold. Firstly, individuals should not be able to manipulate the treatment assignment rule. In this setting the treatment differs by birth cohorts. Since individuals cannot manipulate their birth year and parents could not have possibly known about the reform at the time of birth this assumption holds. Secondly, neighbouring birth cohorts should not discontinuously vary in characteristics other than the statutory retirement ages. Tables 22 and 23 in the appendix test for discontinuities between neighbouring cohorts by applying the same identification design with different observable characteristics as the dependent variable.²⁸ The discontinuities found in observable characteristics are of low economic magnitude and rarely statistically significant. This assumption therefore also seems to hold. Note that cohorts

²⁸Characteristics include: the share of women, the share having grown up in East-Germany, the average OJI in the age range 58 to 63, the average gross daily income in the age range 58 to 63, the share of individuals with part-time employment.

might still differ in unobservable characteristics as this cannot be tested.

Additionally, there should not be other policy reforms that affect individuals around the respective cohort cutoffs discontinuously. This is violated for the cohort 1951 and 1952 as women in the former cohort had the option to retire via the women's pathway at age 60 and women born from 1952 onwards could only do so at age 63. A large discontinuity in the average labor market exit age is therefore to be expected between these two cohorts.

4.2.2. RDD Results

Table 9 depicts the results with a linear specification of the empirical model in Equation 3 for both the group without individuals eligible for the "retirement with 63" and the group eligible for the "retirement with 63". For both groups the results do not significantly change when control variables are added to the estimation. The outcome is measured in months, i.e. the treatment measures the reform effect in months.

In the group of individuals not eligible for the FRA of the "retirement with 63" pathway there is a significant discontinuity of five months between the cohorts 1951 and 1952 that can be explained by the abolishment of the women's pension pathway. No clear pattern is discernible for the other cohort discontinuities. If individuals were to fully follow the increases in the NRA all coefficients should be equal to one month. While the estimated effect is similar to this magnitude for some of the cohort transitions, the point estimate is very imprecisely estimated and not statistically significant which is unsurprising as the institutional discontinuity between neighbouring cohorts amounts only to 0.3% lifelong deductions.

For the group of individuals eligible for the "retirement with 63" pathway the FRA started to be increased in two monthly steps from cohort 1953 onwards. The effect identified by the empirical model 3 is slightly smaller than two months for the first discontinuity between birth cohorts 1952 and 1953 but equal to two months for the later two cohort discontinuities. As described individuals can avoid lifelong deductions between 9.3% to 9.9% by shifting their retirement in compliance with the increase in the FRA. The estimates reveal that the retirement decision of individuals seems to be strongly influenced by the institutional incentive. The increase of the FRA seems to be a successful policy to increase the labor market exit age of individuals eligible for the "retirement with 63" pathway.

To test for functional form misspecification the same estimation is repeated with a quadratic trend in $f(z_i - c)$. The resulting estimates in Table 10 are larger in size. For the group not eligible for the "retirement with 63" pension some coefficients are statistically significant at the 5% level and larger than the associated one month increase in the NRA. For the eligible group the discontinuity remains of a similar magnitude as the increase in the FRA, with the estimates being slightly above two months and statistically significant at the 1% level.

In summary, as part of the "retirement with 63" reform the FRA was first lowered to 63 years and is increasing for birth cohorts 1953 and later. This implies strong financial incentives for eligible individuals from birth cohort 1953 and onwards to shift their retirement from 63 in accordance as they otherwise face lifelong deductions of more than 9% of pension income. Empirical results indicate, that the increase of the FRA is successful in shifting retirement ages for the group eligible for this retirement pathway. Simultaneously the NRA is increasing implying increasing deductions for individuals not retiring at the FRA or the NRA. The associated financial incentive

Table 9: Effects of the increase in FRA or NRA on labor market exit ages by birth cohorts

	1948_49	1949_50	1950_51	1951_52	1952_53	1953_54	1954_55
Panel A: RDD excluding the eligible group - No control variables							
Treat. Effect	0.247 (0.837)	1.133 (1.012)	-0.666 (0.937)	5.131*** (1.489)	0.869* (0.468)	1.226* (0.710)	0.033 (0.440)
Observations	7,732	8,622	9,239	9,670	10,054	10,302	10,621
Panel B: RDD excluding the eligible group - Control variables							
Treat. Effect	0.134 (0.968)	1.017 (1.146)	-0.076 (0.854)	4.929*** (1.107)	0.367 (0.554)	1.419* (0.769)	-0.182 (0.485)
Observations	7,174	8,187	8,815	9,223	9,657	9,942	10,283
Panel B: RDD Eligible group - No control variables							
Treat. Effect	-	-	-	-	1.370** (0.582)	2.070*** (0.744)	2.212*** (0.299)
Observations	-	-	-	-	9,389	9,153	9,194
Panel B: RDD Eligible group - Control variables							
Treat. Effect	-	-	-	-	1.208* (0.617)	2.256*** (0.730)	2.074*** (0.276)
Observations	-	-	-	-	9,369	9,134	9,178

Notes: The outcome variable is the labor market exit age measured in months. Coefficients can therefore directly be interpreted as monthly changes of the average labor market age due to the increase in the NRA or FRA. Estimates are always obtained by comparing neighbouring birth year cohorts. For the group without individuals eligible for the "retirement with 63" pathway the NRA was increase in monthly steps by birth cohorts. For the group eligible to retire at the FRA the FRA was increased in two monthly steps by birth cohort from 1952 onwards. Control variables include gender, a dummy for having grown up in east Germany and a non-parametric specification of the average daily wage in the age range 58 to 63 by including dummies for belonging to the different wage quartiles. Estimates are obtained using a linear specification with a triangular kernel, standard errors are clustered by month of birth. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

to delay pension claiming is much smaller in magnitude than for the increasing FRA. Graphical evidence suggests that individuals do delay retirement, but this phenomenon cannot empirically be attributed to the change in the NRA.

Table 10: Quadratic RDD specification

	1948_49	1949_50	1950_51	1951_52	1952_53	1953_54	1954_55
Panel A: RDD excluding the eligible group - Quadratic polynomial - Control Variables							
Treat. Effect	-0.416 (1.236)	2.830** (1.169)	-1.568 (1.237)	6.701*** (1.223)	1.361** (0.628)	2.431** (1.051)	-0.342 (0.467)
Observations	7,174	8,187	8,815	9,223	9,657	9,942	10,283
Panel B: RDD eligible group - Quadratic Polynomial - Control Variables							
Treat. Effect	-	-	-	-	2.025*** (0.750)	2.375** (1.016)	2.354*** (0.329)
Observations	-	-	-	-	9,369	9,134	9,178

Notes: The outcome variable is the labor market exit age measured in months. Coefficients can therefore directly be interpreted as monthly changes of the average labor market age due to the increase in the NRA or FRA. Estimates are always obtained by comparing neighbouring birth year cohorts. For the control group the NRA was increase in monthly steps by birth cohorts. For the treatment group the FRA was increased in two monthly steps by birth cohort from 1952 onwards. Control variables include gender, a dummy for having grown up in east Germany and a non-parametric specification of the average daily wage in the age range 58 to 63 by including dummies for belonging to the different wage quartiles. Estimates are obtained using a quadratic specification with a triangular kernel, standard errors are clustered by month of birth. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

5. Conclusion

Industrialised countries are facing growing pressure of population ageing. Life expectancy is rising while at the same time birth rates are decreasing. Accordingly the share of individuals in employment relative to those receiving social insurance benefits is falling. This increase of the dependency ratio puts the sustainability of pay-as-you-go pension systems in question. Over the past decades countries have responded to the demographic transition by decreasing the generosity of the retirement system. Statutory retirement ages have been increased, deductions for early retirement have been introduced and the eligibility criteria for pension pathways have become more strict. These reforms were implemented with the stated goal of delaying labor market exits as well as pension claiming. Fundamentally, these reforms constitute a reduction in lifetime income for many individuals, and have been met with fierce opposition and political backlash.

Empirical evaluations of reforms which increased statutory retirement ages have found significant labor market responses. Individuals delay their labor market exit as well as their retirement. Nonetheless, fiscal savings have often been diminished due to substitution effects. Individuals affected by the reforms partially substituted old-age pension claiming with alternative social insurance benefits such as unemployment or disability benefits. This substitution effect is mostly driven by individuals remaining in the labor market state they were in before becoming affected by the reform.

A less common line of reforms are those that increased the generosity of the pension system. This work contributes to a small body of literature that has sought to identify whether responses to increases in the generosity are symmetrical to those of benefit cuts in the system. As a partial backlash to past reforms, Germany has implemented a reform that allows individuals with especially long insurance records to retire earlier without deductions. The full retirement age (FRA) was decreased from 65 to 63 in a pension pathway commonly known as "retirement with 63". The reform was justified on the grounds that eligible individuals had stabilised the pension system by their long contribution history and were especially adversely affected by rising statutory retirement ages due to the demanding nature of their jobs and associated health issues.

This work complements Dolls and Krolage, 2023 in investigating the reform's effects on labor market exits. The main empirical finding of Dolls and Krolage, 2023 could be replicated. The average labor market exit age is reduced by between three to 3.5 months for each year the FRA was lowered. A slightly smaller but also negative and statistically significant effect was found on the average exit age of the last employment subject to social security contributions. The reform therefore reduced the supply of skilled labor and increased the period of pension receipt for a subgroup of the population. The labor market response is especially strong for individuals working in demanding occupations, subject to high job strain. This effect is equally driven by physical as well as psychosocial job strain. Individuals with incomes above the median also react significantly stronger to the reform, likely because they are not dependent on earning additional pension points in order to increase their pension level.

Despite its name the "retirement with 63" reform only allowed individuals born after June in 1951 or in 1952 to retire without deductions at age 63. Individuals born in consecutive years face an increasing full retirement age that rises by two months for each successive birth year cohort until eventually reaching the age of 65. This provides a strong financial incentive for eligible

individuals to delay pension claiming by the difference between their cohort's FRA and the age of 63. Individuals who retire before their FRA have to bear lifelong deductions of more than 9% of their pension income. Exploiting a regression discontinuity approach this work finds that individuals in affected cohorts shift their retirement claiming age together with the increasing FRA due to the reform. Increasing the FRA therefore is an effective means of delaying pension claiming. This is the case despite the salient "retirement with 63" reform that was framed strongly around the age of 63.

Overall, individuals seem to be highly respondent to shifts in the statutory retirement ages. This is the case even for individuals with sufficient liquidity to smooth consumption from their preferred retirement age. Individuals especially vulnerable to increases in the statutory retirement age are those who are unable to continue working due to health issues that often arise out of the demanding nature of their employment. The "retirement with 63" reform was an attempt to partially shield this group of people from rising statutory retirement ages. However, it remains questionable whether the reform was sufficiently targeted. Börsch-Supan, Coppola, and Rausch, 2015 and Dolls and Krolage, 2023 find that eligible individuals do not suffer from worse health and actually have higher incomes than individuals who barely do not qualify. This indicates that the reform was insufficiently targeted at the population most at risk from rising statutory retirement ages. Building on these results and in contrast to findings in Zwick et al., 2022 this work additionally identifies stronger labor market responses by individuals in demanding occupations as well as by individuals with higher incomes.

In conclusion, changes in the statutory retirement ages have significant labor market effects. These effects are symmetric for decreases and increases of the statutory retirement age. A major dimension of retirement decisions is the factor of health. This work has shown that individuals in more demanding occupations react stronger to the possibility to retire at an earlier age. Increasing statutory retirement ages is an effective tool in delaying pension claiming. Major challenges of future adjustments to the pension system is the accurate targeting of groups at risk who cannot prolong their labor market attachment in line with increasing statutory retirement ages.

A. Difference-in-Differences - Robustness

Table 11: Difference-in-differences - Pre-Trend analysis

	1947	1948	1949
Treat. Effect	-0.029 (0.098)	0.032 (0.096)	-0.056 (0.095)
Observations	9,309	9,434	9,595

Notes: Estimates are based on a comparison to the cohort 1946 which is set as the hypothetical pre-reform cohort instead of the actual pre-reform cohort 1948. Half of the individuals in cohort 1949 are not treated, while the other half has a maximal reduction in the FRA of 6 months. It is therefore also included in the pre-trend analysis. No significant differences in the trends between the treatment and control group can be found indicating that the parallel trends assumption holds in the pre-treatment periods. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1.

Table 12: Difference-in-differences - Sample with marginal employment

	1949	1950	1951	1952	1953	1954	1955
Sample including marginally employed							
Treat. Effect	-0.150* (0.087)	-0.043 (0.084)	-0.200** (0.085)	- (0.079)	- (0.077)	- (0.076)	- (0.076)
Observations	9,251	9,522	9,382	9,743	9,667	9,424	9,510

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. Control variables include: gender, education and the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany.

In contrast to the main specification marginal employment is also counted as labor market activity in the construction of the average labor market exit age. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1.

Figure 9: Average labor market exit ages - Including marginal employment



Notes: The figure plots the average labor market exit age for the birth cohorts 1946 - 1955 separately for men and women in yearly bins. Exit ages are top-coded to age 66. Cohort 1948 is the pre-reform cohort. Cohorts 1949 to 1951 are transition cohorts and the FRA is fully lowered to 63 for cohort 1952. Beginning with cohort 1953 the FRA increases in 2 month steps for each consecutive cohort.

In contrast to the main specification marginal employment is also considered as labor market activity.

B. Difference-in-Differences - Heterogeneity

B.1. Regression-Discontinuity Design

B.2. Replication of Results

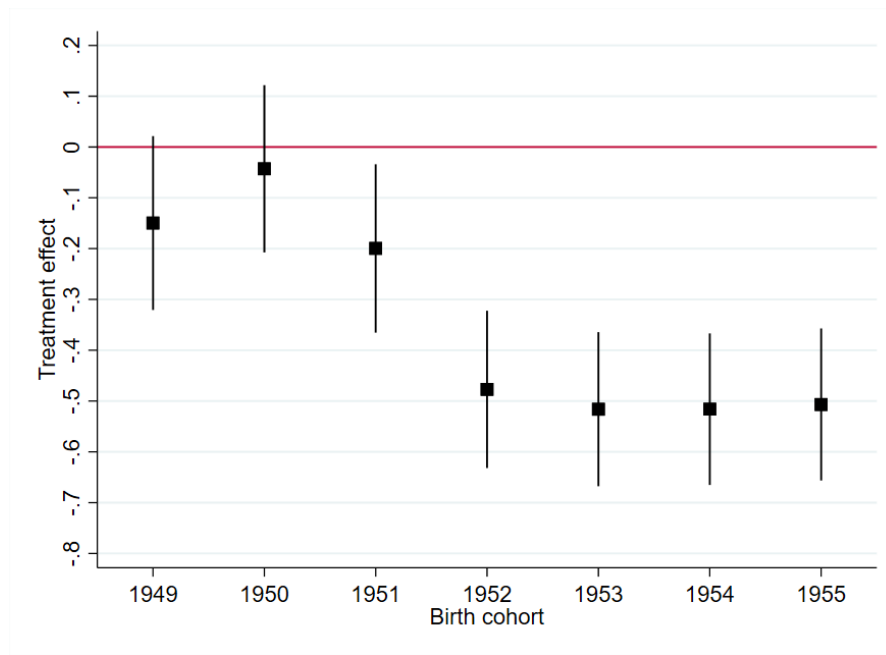
A replication package for all results was submitted to the examination office and supervisors together with this thesis. It is available upon request for anyone via E-Mail.

Access to the confidential data source was provided via on-site use at the Research Data Centre (FDZ) of the Federal Employment Agency at the Institute of Employment research (IAB) and subsequently via remote data access. In order to replicate the results of this work a data contract must be signed with the IAB for access to the SIAB version 7521 v1.²⁹ Together with the base version of the data, the following sensitive variables need to be additionally applied for:

- Month of birth (gebmon)

²⁹DOI:10.5164/IAB.SIAB7521.de.en.v1, see Frodermann et al., 2021 for an introduction to the data source.

Figure 10: Treatment effect by birth cohort - Including marginal employment



Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the job exit age in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier job exit due to the reform. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before job exit and a dummy for having grown up in east Germany. Confidence intervals are depicted at the 5% level.

In contrast to the main specification marginal employment is also considered as labor market activity.

- Occupational sub-group (beruf2010_4)

Finally, the occupational level data on job strain by Kroll, 2015³⁰ is available as an open source file. It must be transferred to the FDZ of the IAB via an informal request at iab.fdz@iab.de.

³⁰Job Exposure Matrices (JEM) for ISCO and KldB (Version 2.0), <https://doi.org/10.7802/1102>.

Figure 11: Average labor market exit ages - Excluding the former GDR



Notes: The figure plots the average labor market exit age for the birth cohorts 1946 - 1955 separately for men and women in yearly bins. Exit ages are top-coded to age 66. Cohort 1948 is the pre-reform cohort. Cohorts 1949 to 1951 are transition cohorts and the FRA is fully lowered to 63 for cohort 1952. Beginning with cohort 1953 the FRA increases in 2 month steps for each consecutive cohort.

In contrast to the main specification individuals from the former GDR are delted from the sample.

Table 13: Difference-in-differences - Sample without East-Germany

	1949	1950	1951	1952	1953	1954	1955
No East-Germany							
Treat. Effect	-0.033	-0.052	-0.204**	-	-	-	-
	(0.095)	(0.092)	(0.092)	0.654***	0.620***	0.589***	0.574***
				(0.086)	(0.084)	(0.082)	(0.079)
Observations	7,469	7,549	7,445	7,738	7,689	7,635	7,845

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. Control variables include: gender, education and the labor market status before turning 63, average daily wage in the five years before labor market.

In contrast to the main specification individuals having grown up in the former GDR are deleted from the entire sample. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1.

Table 14: Difference-in-differences - No top-coding of labor market exit age

	1949	1950	1951	1952	1953	1954	1955
Treat. Effect	-0.183*	-0.150	-	-	-	-	-
	(0.105)	(0.098)	0.329***	0.599***	0.545***	0.510***	0.494***
Observations	9,911	10,218	10,193	10,649	10,687	10,512	10,728

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the labor market age in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier labor market due to the reform. Control variables include: gender, education and the labor market status before turning 63, the average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Standard errors are depicted in brackets.

The labor market age was not top-coded to a maximum of 66 in contrast to the main specification in 4.1.2.

*** p < 0.01, ** p < 0.05 * p < 0.1.

Table 15: Difference-in-differences - Alternative sample restriction

	1949	1950	1951	1952	1953	1954	1955
Treat. Effect	-0.110	0.017	-	-	-	-	-
	(0.082)	(0.078)	0.235***	0.479***	0.491***	0.476***	0.413***
Observations	10,824	11,112	11,013	11,402	11,400	11,536	11,235

Notes: Estimates are based on a comparison to the pre-reform cohort 1948. Treated individuals have 45 or more contributions years, members of the control group between 42 and 38. The outcome variable is the labor market age in years, multiplying the treatment effect estimate with 12 therefore yields the number of months of earlier labor market due to the reform. Control variables include: gender, education and the labor market status before turning 63, the average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Standard errors are depicted in brackets.

In contrast to the main specification individuals with no job subject to social security contributions after age 55 and those no longer in the labor market after age 59 are included in this sample. See section 3.1 for a discussion of sample restrictions.

*** p < 0.01, ** p < 0.05 * p < 0.1.

Table 16: Difference-in-differences - Gender dimension

	1949	1950	1951	1952	1953	1954	1955
Panel A: Men							
Treat. Effect	-0.107	-0.141	-	-	-	-	-
	(0.109)	(0.101)	0.434***	0.499***	0.513***	0.410***	0.318***
Observations	6,305	6,576	6,527	6,641	6,478	6,414	6,556
Panel B: Women							
Treat. Effect	-0.163	-0.024	-0.178	-	-	-	-
	(0.128)	(0.125)	(0.121)	0.467***	0.426***	0.452***	0.448***
Observations	3,606	3,642	3,666	4,008	4,209	4,098	4,172

Notes: Same treatment and control group assignment as before. Control variables include education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only males and from a sample containing only females. Standard errors are depicted in brackets.

*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

Table 17: Labor market exit age by job demand

	Low demand	High demand
Control group labor market age	63.69	63.33
	(1.772)	(1.959)
Treatment group labor market age	63.51	63.42
	(1.383)	(1.370)
Control group - exit age job subject to social security contributions	62.10	61.78
	(3.122)	(3.094)
Treatment group - exit age job subject to social security contributions	61.97	62.27
	(2.823)	(2.481)
Observations	22,296	22,129

Notes: Low demand corresponds to all individuals with an average general job demand index of below and including 5 in the age range 58 - 63. High demand to all those with an average general job demand of above 5. 10 marks the highest job demand, while 1 is the lowest.

Table 18: Difference-in-differences - Job demand as in Zwick et al., 2022

	1949	1950	1951	1952	1953	1954	1955
Panel A: Low Demand							
Treat. Effect	-0.191	-0.110	-0.280**	-	-	-	-
	(0.131)	(0.125)	(0.125)	0.528***	0.411***	0.443***	0.395***
Observations	3,404	3,452	3,420	3,572	3,585	3,516	3,605
Panel B: High Demand							
Treat. Effect	0.046	-0.136	-0.164	-	-	-	-
	(0.159)	(0.152)	(0.144)	0.689***	0.667***	0.547***	0.509***
Observations	2,848	2,941	2,986	3,175	3,186	3,148	3,126

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only individuals with a general job demand measure in the last job subject to social security contributions of three or less (low demand) or eight and higher (high demand). Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

Table 19: Difference-in-differences - Illness dimension

	1949	1950	1951	1952	1953	1954	1955
Panel A: Never Sick							
Treat. Effect	-0.096	-0.025	-	-	-	-	-
	(0.086)	(0.081)	0.246***	0.552***	0.513***	0.465***	0.478***
Observations	8,793	9,058	9,060	9,464	9,523	9,348	9,515
Panel B: Sick							
Treat. Effect	-0.263	-	-	-	-	-	-
	(0.236)	0.602***	0.763***	0.856***	0.816***	0.873***	0.754***
Observations	1,118	1,160	1,133	1,185	1,164	1,164	1,213

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only individuals with no gaps in their labor market spells due to illness and from individuals from whom at least one gap due to illness could be identified. Note that short term illness is not captured by these measures. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

Table 20: Difference-in-differences - Physical job strain

	1949	1950	1951	1952	1953	1954	1955
Panel A: Low Demand							
Treat. Effect	-0.139	-0.163	-	-	-	-	-
	(0.110)	(0.104)	0.298*** (0.105)	0.536*** (0.095)	0.452*** (0.092)	0.436*** (0.091)	0.396*** (0.094)
Observations	4,638	4,728	4,693	4,950	5,127	5,057	5,175
Panel B: High Demand							
Treat. Effect	0.004	0.026	-	-	-	-	-
	(0.120)	(0.114)	0.287*** (0.110)	0.577*** (0.104)	0.599*** (0.103)	0.544*** (0.102)	0.609*** (0.100)
Observations	5,214	5,436	5,448	5,655	5,503	5,388	5,488

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only individuals with a physical job demand measure in the age range of 58 to 63 of five or less (low demand) or more than 5 (high demand). Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

Table 21: Difference-in-differences - Psychosocial job strain

	1949	1950	1951	1952	1953	1954	1955
Panel A: Low Demand							
Treat. Effect	-0.049	-0.096	-	-	-	-	-
	(0.108)	(0.104)	0.279*** (0.104)	0.502*** (0.095)	0.433*** (0.092)	0.504*** (0.092)	0.447*** (0.093)
Observations	5,375	5,387	5,352	5,610	5,772	5,645	5,828
Panel B: High Demand							
Treat. Effect	-0.155	-0.104	-	-	-	-	-
	(0.121)	(0.113)	0.349*** (0.111)	0.664*** (0.104)	0.645*** (0.103)	0.510*** (0.101)	0.550*** (0.101)
Observations	4,456	4,756	4,767	4,974	4,833	4,772	4,805

Notes: Same treatment and control group assignment as before. Control variables include gender, education, the labor market status before turning 63, average daily wage in the five years before labor market and a dummy for having grown up in east Germany. Estimations are obtained separately from a sample containing only individuals with a psycho-social job demand measure in the age range of 58 to 63 of five or less (low demand) or more than 5 (high demand). Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

Table 22: Discontinuities in observables - Sample excluding eligible individuals

	1948_49	1949_50	1950_51	1951_52	1952_53	1953_54	1954_55
Women	0.020 (0.023)	-0.035* (0.019)	-0.012 (0.025)	0.021 (0.022)	0.001 (0.013)	0.006 (0.013)	-0.016 (0.019)
Observations	7,732	8,622	9,239	9,670	10,054	10,302	10,621
East Ger.	- 0.032*** (0.012)	0.011 (0.012)	0.013 (0.012)	0.000 (0.007)	0.004 (0.011)	0.001 (0.009)	0.007 (0.010)
Observations	7,732	8,622	9,239	9,670	10,054	10,302	10,621
OJI age 58 - 63	0.089 (0.093)	0.061 (0.082)	0.225*** (0.076)	-0.006 (0.080)	0.039 (0.055)	0.053 (0.138)	0.056 (0.126)
Observations	7,584	8,477	9,083	9,522	9,931	10,161	10,460
Avg. daily income 58 - 63	-0.199 (1.128)	-0.985 (2.030)	-2.860 (2.927)	-1.771 (2.905)	2.672** (1.159)	-2.560* (1.492)	0.328 (1.733)
Observations	7,174	8,187	8,815	9,223	9,657	9,942	10,283
Share part time	-0.001 (0.032)	0.002 (0.023)	-0.002 (0.026)	0.011 (0.023)	- 0.054*** (0.015)	-0.023 (0.025)	-0.004 (0.022)
Observations	6,682	7,824	8,574	8,976	9,431	9,743	10,068

Notes: The outcome variable is the labor market exit age measured in months. Coefficients can therefore directly be interpreted as monthly changes of the average labor market age due to the increase in the NRA. Estimates are always obtained by comparing neighbouring birth year cohorts. The NRA was increase by one month for each consecutive birth cohort. The sample only includes individuals with less than 42 contribution years. Estimates are obtained using a linear specification with a triangular kernel, standard errors are clustered by month of birth. Standard errors are depicted in brackets.

*** p < 0.01, ** p < 0.05 * p < 0.1

Table 23: Discontinuities in observables - Sample including eligible individuals

	1948_49	1949_50	1950_51	1951_52	1952_53	1953_54	1954_55
Women	-	-	-	-	0.003	-	0.008
						0.043***	
					(0.009)	(0.016)	(0.008)
Observations	-	-	-	-	9,389	9,153	9,194
East Ger.	-	-	-	-	-0.004	-0.022	0.038**
					(0.024)	(0.014)	(0.019)
Observations	-	-	-	-	9,389	9,153	9,194
OJI age 58 - 63	-	-	-	-	-0.152	0.116	-
					(0.134)	(0.094)	0.132***
					9,344	9,095	9,121
Observations	-	-	-	-	9,344	9,095	9,121
tentgelt 5ymean	-	-	-	-	2.333***	0.134	0.476
					(0.869)	(1.402)	(1.168)
Observations	-	-	-	-	9,369	9,134	9,178
Part time share	-	-	-	-	-0.015	-0.009	-0.016*
					(0.012)	(0.013)	(0.010)
Observations	-	-	-	-	9,317	9,091	9,143

Notes: The outcome variable is the labor market exit age measured in months. Coefficients can therefore directly be interpreted as monthly changes of the average labor market age due to the increase in the FRA. Estimates are always obtained by comparing neighbouring birth year cohorts. The FRA was increase by two months for each consecutive birth cohort. The sample only includes individuals with more than 45 contribution years. Estimates are obtained using a linear specification with a triangular kernel, standard errors are clustered by month of birth. Standard errors are depicted in brackets.

Notes: *** p < 0.01, ** p < 0.05 * p < 0.1

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