Labor market effects of early retirement reforms

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We study causal labor market effects of two early retirement reforms. Reform 1 increased normal retirement age (NRA) stepwise from age 60 to 63. Simultaneously, it became possible to use an early retirement starting age of 60 (the ERA) with benefit discounts. Later, reform 2 increased the age of early retirement (ERA) for retirees on the unemployment pathway stepwise from age 60 to 63. Benefit receipt at the pre-reform ERA thus became impossible. We investigate behavioral responses to the reform based on administrative data and a difference-in-difference-in-differences (DIDID) strategy. We find strong and significant causal effects of both reforms. Individuals postponed retirement and adjusted labor force participation. We observe increased utilization of alternative pathways into retirement. Individuals with low pension wealth and who were affected immediately by the reform responded more strongly.

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1. Introduction

In most industrialized countries, population aging generates a need for policy reforms and adjustments in retirement systems (OECD 2017). Here, evidence based policy decisions need reliable causal analyses. This study exploits two natural experiments with respect to early retirement rules. We offer causal evidence on the behavioral adjustments induced by reforms of the retirement entry age for German men. In particular, we investigate whether and to what extent shifts in retirement entry age affect labor force participation and program substitution.

The retirement entry age is central to the fiscal challenges facing retirement insurances. Under constant budgets, the only alternative policy to postponing retirement entry is to cut retirement benefits, an even less popular measure. However, in most retirement systems, the regulation of retirement entry is complex and allows for different types of adjustments. Besides early and normal retirement ages, there are often regulations for specific groups of the workforce. As an example, the German retirement system uses different "pathways" to retirement for females, those with disabilities or severe handicaps, those with a long work history, and those who are unemployed prior to retirement age. While the differentiation of pathways into retirement is relevant in most retirement systems (OECD 2017), the international literature rarely discusses its relevance and implications.

A large literature investigates the causal effects of retirement reforms. A first group of contributions considers structural parameters and separates wealth and accrual affects (e.g., Hanel 2010, 2012, Brown 2013, Atalay and Barrett 2015). A second group chooses a reduced form approach to determine causal reform effects on individual labor force status. Here, some analyses study reforms of the normal retirement age (NRA) (Mastrobuoni 2009, Hanel and Riphahn 2012, Lalive and Staubli 2015, Oguzoglu et al. 2016) which provides retirement benefits without actuarial deductions. Others consider modifications of the early retirement age (ERA) (Komada et al. 2018, Cribb et al. 2016, Staubli and Zweimüller 2013, Geyer et al. 2018, Manoli and Weber 2016) at which retirement is possible only with reduced benefits.

We contribute to this literature in four important ways: First, we improve on a strategy to identify causal effects first introduced by Krueger and Pischke (1992) (for applications see Mastrobuoni 2009, Hanel and Riphahn 2012, Staubli and Zweimüller 2013). The strategy identifies causal effects of cohort specific reforms by comparing the behavior of treated younger and non-treated older birth cohorts at given ages. The identifying assumption is that absent the reform developments over the lifecycle would have been identical across cohorts. We add a further dimension to this approach by considering behavioral differences for affected (male) and non-affected (female) individuals of the same age and birth cohort where some ages and cohorts are treated and some are not. This accounts for general trends across cohorts as well as for specific trends across cohorts for the male and female subsamples (difference-in-difference-in-differences (DIDID) estimation). Second, we exploit two related reforms of the NRA and ERA. This generates insights beyond those available from using just one reform. Staubli and Zweimüller (2013, p. 20) point out that "a rise in the ERA is likely to be a more effective measure to increase LFP among older workers as opposed to a rise in the NRA." We can test this hypothesis. Third, we take advantage of large samples of potential retirees using administrative data from the German mandatory retirement insurance. The utilization of large and precise data may provide more reliable evidence than prior survey based studies (e.g., Krueger and Pischke 1992, Atalay and Barrett 2015, Giesecke 2018). Finally, we investigate a broad set of labor market responses and study program substitution. Besides causal reform effects on employment and unemployment, we identify the reforms' effects on alternative pathways into retirement. This is important because the reform objective - reducing the fiscal burden of aging populations - cannot be reached if individuals respond by simply shifting to different retirement programs.

In terms of identification, our study is most similar to Atalay and Barrett (2015) who investigate the effects of the 1993 Australian Age Pension reform. That reform progressively increased the ERA for women from 60 to 65 between 1995 and 2014 affecting the birth cohorts

1935 and after. The authors study the effects on labor force participation and program substitution and find strong effects in both dimensions. They identify causal effects using difference-in-differences (DID) estimations on affected birth cohorts for men and women. In addition, they present their DID estimates separately by age following Mastrobuoni (2009), but without specifying a full DIDID model. We similarly identify the causal effect comparing males and females. However, we focus our analysis on treatment effects separately for cohorts before and after the reform in affected and non-affected ages using a full DIDID model. While in the Australian pension program changes in labor force participation do not yield accrual effects on social security wealth and benefit receipt is means tested, we apply similar empirical strategies to a more traditional retirement insurance with accrual and without means tests. Also, we differ from Atalay and Barrett (2015) and Mastrobuoni (2009) by using precise information on benefit receipt from administrative data instead of approximating the timing of retirement based on self-reported non-participation in the labor force. In our setting, a regression discontinuity approach is not convincing because the reforms were introduced stepwise and the reform intensity increased linearly with the value of the running variable. This differs from the setting, e.g, studied by Geyer and Welteke (2019) where a pathway to retirement was abolished abruptly at a given date of birth.

In terms of the institutional setting, our study is most similar to Engels et al. (2017) who investigate a reform of the retirement pathway for females in Germany. That reform consisted of a stepwise increase of the NRA from age 60 to 65 for the birth cohorts of 1940 and after in combination with the contemporaneous introduction of an ERA and related benefit deductions. Engels et al. (2017) study the direct effect of benefit discounts for the affected cohort and consider a broader set of outcomes following Mastrobuoni (2009). They find sizeable employment and retirement responses to the introduction of benefit deductions. In contrast, we focus on reforms of the unemployment pathway to retirement. The first reform increased the NRA stepwise from age 60 to 65 for the birth cohorts of 1937 and after and contemporaneously

introduced an ERA with related benefit deductions. Our second reform increased the ERA stepwise from age 60 to 63 for the birth cohorts 1946 and after. We differ from Engels et al. (2017) by first, considering reform effects on additional outcomes such as disability retirement, second, by focusing on men, third by offering an additional control group in our identification strategy and finally, by comparing the effects of two separate reforms of the unemployment pathway to retirement.

We find that both reforms increase the propensity to stay employed longer, postpone unemployment from before to after age 60, and delay old-age retirement. After reform 1, we observe an increased use of substitute pathways to enter retirement, i.e., disability retirement and retirement of the severely handicapped. Finally, the behavioral adjustments after reform 2 appears to exceed those after reform 1. These results agree well with the prior national and international literature. We also find that individuals who have little time to adjust to the reform and who are caught by surprise delay retirement by more and adjust employment more strongly than those who have time to prepare for regulatory changes. The reform effects vary by pension wealth: in response to financial incentives the poorest prolong employment and postpone retirement by more than those with higher pension wealth.

Our paper is of general interest because it offers and carefully discusses an additional dimension of identification, studies a wide set of alternative pathways to retirement and their interaction, and compares the effects of different reforms, all using reliable administrative data. As recent reforms - certainly in Germany - continue to modify benefit discounts, the issue remains on the political agenda (for an analysis see Dolls and Krolage 2019).

In section 2, we describe the institutional features of the German retirement system in and discuss our hypotheses and the underlying mechanisms of the expected reform effects. Section 3 outlines our data, sample, and variables and provides first descriptive evidence. Additionally, we discuss the empirical method and potential challenges to the identification strategy. Results and robustness tests follow in section 4 and section 5 concludes.

2. Institutional Background and Hypotheses

2.1 Retirement Insurance and Pathways to Retirement

The German retirement insurance (established 1889) operates on a pay as you go basis. It is funded mostly by mandatory contributions of employers and employees. Regulated at the federal level, the mandatory retirement insurance covers more than 80 percent of the population, excluding only civil servants and the self-employed. It offers old-age, disability, and survivor benefits. Given the limited relevance of private pensions in Germany, the mandatory retirement system provides the main source of income for most elderly households (Frommert 2010).[1]

Generally, benefit amounts depend on the number of contribution years and contribution amounts. Benefit eligibility is regulated along pathways to retirement, which differ in requirements and generosity. Examples for such pathways are retirement "due to unemployment", "for women", "after long term employment", or "disability retirement". Pathways can be used either with the normal retirement age (NRA) or the early retirement age (ERA). With the ERA, the minimum retirement entry age and subsequent benefit amounts are lower than with the NRA; specifically, each month of benefit receipt under an ERA prior to the NRA generates a permanent benefit cut by 0.3 percent (i.e., 3.6 percent for each full year).

We exploit two reforms of the entry age for the unemployment pathway into retirement (see **Table 1**, column A). The first reform increased the NRA stepwise from age 60 to 65 starting with individuals born in January 1937 and ending with those born in December 1941. At the same time, an ERA became newly available at age 60 for all cohorts. The second reform increased the ERA stepwise from age 60 to 63 starting with individuals born in January 1946 and ending with those born in December 1948.

[1] For broader discussions of the German retirement system see, e.g., Seibold (2019), BMAS (2010), Geyer et al. (2018), Geyer and Welteke (2019), or Ye (2018).

In addition to a minimum entry age, three requirements characterize the unemployment pathway: the individual must have accumulated at least 15 insurance years (years of contribution or substitution, *Wartezeit*), paid contributions in 8 out of the last 10 years, and be unemployed and have been unemployed for at least 52 weeks after age 58 years and 6 months.[2] The last two requirements were slightly relaxed after 2000.

Both reforms contained regulations to protect the 'legitimate expectation' (*Vertrauensschutz*) of individuals. In particular, individuals born before 1942 with 45 insurance years or more and those born before Feb. 14, 1941 and unemployed on Feb. 14, 1996 were not affected by reform 1. Individuals who were unemployed on Jan. 1, 2004 were not affected by reform 2.[3] We exclude these individuals from the treatment group in our analyses.

Table 1 summarizes pathways into retirement with their NRA and ERA by birth cohort. Column B depicts the retirement pathway for women, which requires that the person accumulated at least 15 insurance years and contributed for at least 10 years after age 40. Here, the reform for birth cohorts 1940 and after is very similar to the first reform of the unemployment pathway. Retirement after long-term employment (column C) is available for individuals with at least 35 insurance years and offers an ERA of 63 and a NRA of 65. Regular old-age retirement (column D) requires only five insurance years for eligibility. It does not offer an ERA and has traditionally been available at age 65. However, the NRA increases for birth cohorts 1949 and after. Column E depicts a pathway that is available only for individuals with severe handicaps, i.e., those who hold a certificate indicating that their physical, mental, or psychological ability is reduced permanently. Claimants must have 35 insurance years.

In addition, the disability pathway (see **Table 1** column F) used to provide retirement benefits independent of age either as retirement in full or in part depending on the degree of

[2] Since 1996, the unemployment pathway is additionally available for individuals who were employed on partial retirement schemes (*Altersteilzeit*) for at least 24 months after age 55. However, the share of individuals entering the unemployment pathway over the partial retirement route has always been small.
[3] The protection for reliance on existing law contained additional detailed regulations (see Steffen 2018).

disability (for detail see Hanel 2012). The disability retirement benefit can be provided until old-age retirement age is reached. Since a 1999 reform, the NRA for disability benefits is 63 for retirement entries that occurred after 31.12.2000. If benefits are taken out earlier than at age 63, the benefit amount is reduced for each month that disability retirement is entered prior to age 63 but not by more than 10.8 percent (equivalent to age 60). The calculation of disability benefits for younger retirees thus assumes that claimants contributed until age 60. Disability benefits are granted without a waiting period. **Table A.1** in the Appendix describes the disability benefit discounts after 2000 which are specific to month of age and retirement entry. In order to account for the change in disability retirement regulations, we consider control variables that reflect the benefit discount connected to a potential disability retirement for a given calendar time and for each specific person.

A final institutional aspect relates to the regulation of retirement insurance contributions paid in East Germany prior to unification. For individuals born prior to 01.01.1937, such contributions were treated like contributions made by immigrating ethnic Germans in their countries of origin (*Fremdrentengesetz*). To avoid effects of regulatory changes around the 01.01.1937 cutoff on our findings, we follow Ye (2018) and perform our main analyses using only individuals who contributed in West Germany. In robustness tests, we include individuals with labor market experience in East Germany.

Since anticipation behavior can affect the estimation of treatment effects, it is important to consider reform announcements: in 1989, a reform law was passed (*Rentenreformgesetz* 1992) which stipulated that starting in 2001 retirement entry ages should start to increase towards age 65 beginning with the 1941 birth cohort. However, in 1996 another law was passed (*Gesetz zur Förderung eines gleitenden Übergangs in den Ruhestand*) which brought the starting date of these retirement entry age adjustments forward to 1997 (instead of 2001) thus newly affecting the 1937 (instead of 1941) birth cohort (reform 1). Thus, individuals had little

time for adjustment if they were unemployed at age 59 in 1996 and expected a normal retirement at age 60.

The ERA was available since 1997 for birth cohorts 1941 and after. In 2004 a law was passed (*RV-Nachhaltigkeitsgesetz*) which mandated that the minimum age for early retirement on the unemployment pathway increased starting in 2006 (reform 2). The regulation was "grandfathered in": individuals who were unemployed since January 1, 2004 or in employment relationships that ended before that date were not subject to the reform.

As both reforms were passed into law very briefly before they became effective, we expect to observe short-term behavioral adjustments that are not yet attenuated through anticipatory behavior changes. **Figures 1-4**, as discussed in section 3.1, show no evidence for anticipation behavior. We focus on the behavior of individuals shortly before and after the reform for whom this change in regulation came by surprise.

2.2 Related Institutions: Unemployment as a Bridge to Retirement

As we focus on the pathway to retirement due to unemployment, it is also important to describe unemployment insurance (UI) rules. Historically, two types of unemployment benefits were provided: an insurance based payment (UB I) that covers about 60 percent of last net earnings and a means tested system which provides a more modest minimum income support (UB II). The UB I benefit was provided for a duration of up to 32 months depending on the number of UI contribution years and the age of the unemployed individual (see **Table A.2**). After a reform, benefit availability fell to a maximum duration of 18 months for those falling unemployed after January 31, 2006. The rules became a little more lenient again in 2008 (for an analysis of these reforms see Riphahn and Schrader 2019).

Since the 1990s and prior to our reforms, it was common practice to use the long unemployment benefit durations as a bridge into retirement: retirement benefits after unemployment were available starting at age 60 without benefit deductions. So workers were

laid off starting at the age of 57 and 4 months, received unemployment insurance benefits for up to 32 months, and then entered retirement after unemployment. In our analysis, we follow Engels et al. (2017) and account for changes in the duration of unemployment benefit payout by controlling for the individual and age-specific maximum entitlement length.[4]

2.3 Expected Reform Effects and Hypotheses

The first reform increased the NRA in a stepwise fashion and introduced an ERA. The second reform increased the ERA stepwise and thus disallowed retirement at early ages. Both changes implied negative shocks to individuals' social security wealth: expected benefits declined in magnitude or became entirely unavailable.

Under the first reform, it remained possible to retire starting at age 60. However, the reform introduced a benefit reduction after early retirement which increased over subsequent birth cohorts. To retire at age 60, the 1936 birth cohort did not suffer benefit reductions, birth cohort 1937 had to give up up to 3.6 percent, and by birth cohort 1942 and after individuals lost 18 percent of their benefits.

These adjustments in retirement benefits generated income and substitution effects (for a more formal description, see Hanel and Riphahn 2012). If leisure is a normal good, both effects reduce the demand for leisure, increase attachment to the labor force and incentives to postpone retirement entry. We therefore expect prolonged employment and unemployment. In particular, workers who would have used unemployment as a bridge into retirement before age 60 before the reform may postpone unemployment to later ages, i.e., their unemployment may decline before age 60 and increase afterwards. Also, we expect delayed retirement entry after the first reform.

[4] Individuals on minimum income support (UB II) rather than UB I have to enter old-age retirement as soon as it becomes available.

Alternative pathways of retirement, e.g., for those with severe handicaps, continued to offer retirement entry for the birth cohorts 1937-1939 without benefit deductions at age 60 (see **Table 1**). In addition, disability retirement was available without any age restrictions at full benefits for the 1937 birth cohort. For the birth cohorts 1938 and after, disability retirement at full benefits was available until December 31, 1999. For subsequent entries into disability benefit discounts were introduced if the retiree had not yet reached age 63 (see **Table A.1**). Those who met the health conditions may have used these pathways as substitute exit routes out of the labor force.

The second reform abolished the option to use ERA on the unemployment pathway prior to age 63 in a stepwise fashion for birth cohorts 1946 and after. The effect of increasing early retirement ages has been looked at for other countries before: see, e.g., Cribb et al. (2016) for the UK, Staubli and Zweimüller (2013) for Austria, Vestad (2013) studied the effect of a *reduced* ERA for Norway, and Geyer and Welteke (2019), and Geyer et al. (2018) studied different pathways in Germany. These studies generally find that such a reform (a) affects employment and unemployment, both for those birth cohorts hit by surprise as well as for those who are able to anticipate the change, and (b) incentivizes program substitution for those not willing or unable to prolong their labor market involvement to the increased ERA.

Overall, we expect that after both reforms individuals stay longer in employment (H1). Because unemployment as a bridge to retirement becomes less attractive, after reform 1, unemployment may decline prior to age 60 and increase at age 60 and after. In contrast, after reform 2, unemployment may decrease at age 60 and after because retirement without deductions was available only at age 65 and unemployment would have been shifted to later ages. In summary, we expect that unemployment is postponed to later ages (H2). We expect that both reforms contributed to a delay in the utilization of the unemployment pathway to retirement because it became either more expensive (reform 1) or inaccessible (reform 2) (H3).

Also, we expect that the demand for alternative pathways, such as disability retirement or retirement for the severely handicapped increased (H4).

In addition, it is of interest to compare the effects of both reforms. Staubli and Zweimüller (2013) suggest that responses to changes in the ERA are larger than responses to changes in the NRA: while individuals can ignore changes in the NRA by accepting reduced benefit payments upon retirement postponements of the ERA must be heeded. Here, individuals can only postpone retirement or find alternative pathways. We test the hypothesis that reform 2 caused larger responses than reform 1 (H5).

3. Data and Methods

3.1 Data

We use administrative data offered by the German Pension Insurance. The *Versicherungskontenstichprobe* (VSKT) provides roughly a 1 percent random sample of all persons aged 15-67 and covered by the German Statutory Pension Insurance. The data are available annually since 2002.[5] Each annual wave provides information on individuals' demographic and pension relevant characteristics such as birth year and month, nationality, monthly labor market status, and monthly earning points. For more information about the data, please see Stegmann (2008) and Himmelreicher and Stegmann (2008).

We use different samples for the two reforms. They consist of men and women born between 1935-1939 and 1945-1948. Due to special pension rules, we do not consider civil servants, self-employed, miners, and persons with pension entitlements according to the law on foreign pensions (*Fremdrentengesetz*).[6] We also exclude individuals with missing information

[5] For every wave, the data drop the cohort turning 68 and add the cohort turning 30 in the observation year. The first available VSKT 2002 data observe the cohorts 1935-1972 and the last VSKT 2016 the cohorts 1949-1986. Data for 2003 are not available. The Research Data Centre of the German Pension Insurance (FDZ-RV) created a version of the VSKT data for our project.

[6] We consider persons to be miners if they were insured in the *Knappschaftliche Versicherung* for five years or longer.

on the date of retirement entry.[7] Finally, we exclude persons with contribution periods in East Germany because East Germans in part were subject to special regulations.

We analyze the reform effects by comparing the labor market behavior of treated to that of non-treated individuals. We compare the birth cohorts 1937-39 and 1946-48 who are affected by the reforms to the birth cohorts 1935-36 and 1945 who are not affected. In addition, we take advantage of the fact that females who are eligible for the female retirement pathway also are not affected by the reform of the unemployment pathway. Therefore, they can constitute a control group in our analyses. The female retirement pathway provides a NRA of 60 for birth cohorts potentially affected by our first reform (1935-39) and an ERA of 60 for those birth cohorts potentially affected by our second reform (1945-48). Since the female birth cohorts 1940-44 were affected by the NRA reform for female retirement (see **Table 1**), we do not use them and thus have to limit our post-reform birth cohorts for reform 1 to 1937-39 and our pre-reform birth cohorts for reform 2 to 1945.[8] For reform 1, we therefore consider individuals born 1935-1939 (pre reform: 1935-36, post-reform: 1937-39) and for reform 2 individuals born 1945-1948 (pre-reform 1945, post-reform: 1946-48).

To obtain a homogenous sample, we follow Engels et al. (2017) and consider only those men and women in our sample who fulfill the eligibility criteria for the female retirement pathway when they are at age 55.[9] These eligibility criteria demand a waiting period of 15 years and a compulsory contribution period of at least 10 years after reaching age 40.

For every birth cohort, we use one wave of the VSKT to avoid duplicate observations (see Ye 2018). To handle mortality risks consistently and to avoid cohort-specific biases, for each cohort we use that observation year and data wave in which individuals turn 65. This

[7] These individuals are not yet in retirement when the data is collected. Here, the information on the last months of their employment biography may be misleading because the retirement insurance adjusts their insurance years ex post. For details, please see Stegmann (2016).
[8] We also exclude cohorts 1949 and younger because for them the ERA does not differ by birth month which we use in our identification strategy (see section 3.2).
[9] In our robustness checks, we test whether the estimated effects for this sample differ from those for the full population of treated men.

provides us with biographical information based on which we code monthly observations for each individual.[10] We consider individuals in every age month from 60 years plus 0 months to 62 years plus 11 months. As discussed above some individuals are not affected by the reform because of regulations to protect their 'legitimate expectation' (*Vertrauensschutz*). We code the treatment status of those individuals, about two percent of the sample, as not treated. Overall, we use 11,240 different individuals and 404,640 person-month observations for the first and 8,566 individuals and 308,376 person-month observations for the second reform.

Our dependent variables characterize five different states. For every age month we measure if an observation is in "employment", "unemployment", "old-age retirement", "severely handicapped retirement", and "disability retirement".

Table 2 describes the dependent variables in the analysis samples for the two reforms. In the sample for reform 1, 21 percent are in employment, about 4 percent in unemployment, 57 percent of person-month observations of 60-62 years olds are already in old-age or severely handicapped retirement, and about 13 percent in disability retirement.[11] The reform 2 sample has a higher employment and unemployment rate and a lower rate of retirement.

Figures 1-4 depict cohort- and gender-specific employment and old-age retirement behavior by age for both reforms. In each case, we show one birth cohort not affected by the reform (1936 resp. 1945) and the two most affected birth cohorts (1938-39 resp. 1947-48). **Figure 1** shows employment rates by age for men and women of birth cohorts 1936 and 1938-39. After age 60, employment drops for men and women. However, males stay in employment longer than females and the drop for males clearly differs across birth cohorts. As expected, the male birth cohorts 1938 and 1939 who were affected the most by the reform have higher

[10] Because the waves 2000, 2001, and 2003 are not available, for the birth cohorts 1935-1939, we have to use the VSKT 2002 and 2004.

[11] The remaining observations are either in "other" states, such as training, marginal employment, non-commercial care, or disabled/long-term sick-leave. If observations are in a status that is not relevant for the pension the information on the "Beitragsart (BYAT)" is missing in the data. The definition of the states is based on monthly information on "Beitragsart (BYAT)" in the data.

employment rates than the pre-reform cohort 1936. For the female birth cohorts, we do not observe a change in employment across birth cohorts. The developments for the non-affected birth cohort 1936 are similar for males and females after age 60 supporting our strategy to use females as a control group. **Figure 2** depicts old-age retirement starting at age 60 for the birth cohorts related to reform 1. Generally, males' retirement rates are lower than females'. The male birth cohorts most affected by the reform (i.e., 1938-39) clearly have lower old-age retirement rates than the male pre-reform birth cohort. These patterns are not visible for females and match the expected reform effects. As before, females and males in the birth cohort not affected by the reform (1936) have similar age patterns.

Figure 3 and **Figure 4** present employment and old-age retirement around reform 2, for the birth cohorts 1945, 1947, and 1948. **Figure 3** shows that, for males and females, employment after age 60 is higher for the post- than for the pre-reform cohort. As the change is larger for the male cohorts, this may indicate the expected reform effect. We observe similar age patterns for the male and female pre-reform birth cohort. **Figure 4** shows retirement rates and confirms cohort and gender differences: the retirement of the treated male birth cohorts of 1948 is much below that of the pre-reform 1945 birth cohort. As in **Figure 2**, we observe a similar age pattern for the male and female pre-reform cohort.

In sum, **Figures 1-4** show that affected and non-affected male birth cohorts differ in their employment and retirement behavior in agreement with the expected reform effects and female birth cohorts appear to be suitable controls as they show similar pre-reform patterns.

3.2 Empirical Methods

We aim to identify the causal effect of two reforms on retirement behavior and labor force participation choices of older workers. We exploit the fact that both reforms affected specific birth cohorts at specific ages, e.g., reform 1 modified the NRA of individuals age 60 and born in 1937 or later and reform 2 modified the ERA of individuals age 60 and born in 1946 or later.

Based on the combination of birth cohort and observation period, we can identify the causal reform effect if we assume that the reform is the only determinant of possible behavior changes across birth cohorts at a given age. This identification strategy is widely applied in the literature (e.g., Staubli and Zweimüller 2013, Mastrobuoni 2009, Hanel and Riphahn 2012).

However, we go beyond this standard approach and additionally compare groups of men treated by the reform and women not treated by the reforms, both, at ages when the reform was effective or not, and for birth cohorts which were and were not affected. This DIDID setting is possible because the change in the unemployment pathway to retirement was not accompanied by a similar and simultaneous change in the retirement pathway for females and because the changes were implemented stepwise by birth cohort starting at age 60 for the affected cohorts.

Under the requirements of the female retirement pathway, women continued to be able to retire at an NRA of 60 (reform 1) and similarly to use an ERA of 60 (reform 2) after both reforms (see **Table 1**). Thus, we distinguish men who are affected by the reform (treatment group) from women who are not affected (control group) and compare their behavioral choices at ages which were affected and ages that were not affected by the reforms for birth cohorts affected (post-reform) and not affected (pre-reform) by the reforms. We calculate the reform effect as the difference in changes between men and women across ages and cohorts. This identifies causal effects if the behavioral adjustments across ages and cohorts would have been identical between men and women without the reform or if the behavioral adjustments across cohort groups and gender would have been identical between ages. This is a weaker identifying assumption than that required for a DID model comparing behavior changes across cohorts either between treated men and non-treated women or between affected and non-affected ages: we can allow for changes across birth cohorts and ages that are independent of the reform as long as they occur identically for men and women and for changes in the labor market affecting men and women differently across birth cohorts as long as they are not age-specific.

Let *men* indicate whether the individual is male and belongs to the treatment group (men=1) or female and belongs to the control group (men=0). *C* contains month and year of birth fixed effects and *post* is equal to one for cohorts affected and equal to zero for cohorts not affected by the reform. *A* represents the individuals' monthly age in a given monthly observation a. Some ages are affected by the reform and others are not; *age* (valued 0 or 1) indicates whether an individual is affected by the reform in a given month. X are control variables at the individual level. We use measures of past earnings, health, tenure and insurance group indicators (blue collar, white collar, other) as of age 55, i.e., prior to the observation window. Finally, to account for the unemployment and disability benefit reforms (see section 2.2), we also control for the maximum length of (potential) unemployment benefit receipt and the month- and person-specific potential discount after disability retirement.[12] We use the following linear regression model:

$$Y_{i,a} = \beta_0 + \beta_1 men_i + \beta_2 C_i + \beta_3 A_{ia} + \beta_4 C_i * A_{ia} + \beta_5 men_i * C_i + \beta_6 men_i * A_{ia} + \gamma (men_i * post_i * age_{ia}) + X \theta_1 + \varepsilon_{i,a} . \quad (1)$$

We estimate the parameters β, γ, and θ_1 using least squares regression, where γ represents the causal effect of interest. ε is a random error. We consider variations of this specification where we control for a post reform indicator instead of the detailed date of birth fixed effects, C.

In addition to the interaction model (1), we apply specifications that control for the reform intensity for given individuals. Appendix **Tables A.7** and **A.8** describe the number of months of benefit discounts after reform 1 and the number of months by which an early retirement must be postponed after reform 2. We use these indicators of reform intensity (I) that

[12] For a description and descriptive statistics of all variables, see Appendix **Tables A.3-A.6**. We do not consider indicators for education because these variables mostly hold missing values and the information may not be reliable. Information about marital status is not available.

vary at the cohort, gender, and age level in a separate specification and estimate the following model separately for the two reforms: [13]

$$Y_{i,a} = \beta_0 + \beta_1 men_i + \beta_2 C_i + \beta_3 A_{ia} + \beta_4 C_i * A_{ia} + \beta_5 men_i * C_i + \beta_6 men_i * A_{ia} + \gamma I_{ia} + X \theta_1 + \varepsilon_{i,a} . \quad (2)$$

The estimate for γ yields the causal reform effect. As before, we apply alternative specifications which use a simple post reform indicator (T_i) instead of monthly date of birth fixed effects (C). Generally, γ is identified by the reform which changed the incentives jointly by age and cohort. We report robust standard errors that are clustered at the individual level.

As we are not conditioning our sample on meeting the unemployment pathway criteria, our estimates represent intention to treat effects (ITT) for the population that might be affected by unemployment and that might consider the unemployment pathway. The estimates can be adjusted by the share of individuals in pre-reform cohorts that either used the NRA (prior to reform 1) or the ERA (prior to reform 2).

4. Results and Robustness

4.1 General Comments

In the framework of two reforms which first increased the NRA and introduced an ERA (reform 1) and then increased the ERA (reform 2), we test five hypotheses using two empirical approaches. First, we estimate equation (1) which identifies the causal reform effects (γ) using an interaction of men, post-reform, and affected age indicators. Second, we estimate equation (2) that uses the intensity of the reforms which vary for treated men across the 'birth cohort by age' distribution. We apply both approaches to five dependent variables describing individual labor force status in every age month from age 60 and 0 months to age 62 and 11 months.

[13] The intensity is zero for females, for men of pre-reform cohorts, for men of post-reform cohorts at non-affected ages and for men with protected 'legitimate expectation' (*Vertrauensschutz*) in the respective age month.

4.2 Reform Effects

Table 3 summarizes the estimated causal effects of reform 1 for the five labor force states of those aged 60-62. Columns 1-3 present the estimations based on the interaction model (see equation 1) using different control variables. The direction of the estimated reform effects agree with our expectations: we find increased employment and unemployment, reduced old-age retirement, and increased utilization of the substitute pathways into retirement, i.e. disability and severely handicapped retirement.

The results in columns 4-6 use precise measures of reform 1 intensity (see equation 2) based on individuals' age and birth cohort (see **Table A.7**). The results are mostly statistically significant and confirm the patterns in columns 1-3. More specifically, they suggest that - e.g., with full controls in column 6 - one year of postponed normal retirement increases the propensity to be employed in any given month by 0.8 percentage points (insignificantly) and the propensity to be unemployed by 1.6 percentage points. It reduces the propensity of old-age retirement by 8.6 percentage points and yields large and significant increases in the propensity to use substitute retirement pathways in total by more than 4 percentage points per month.

We apply identical procedures to analyze the effects of reform 2. **Table 4** shows the estimates for individuals aged 60-62 based on the interaction model in columns 1-3. We find large and significant increases in the propensity to be employed and declines in the propensity to be unemployed after the reform. As expected, the propensity for old-age retirement declines. Surprisingly, we do not observe a significant increases in the use of substitute retirement pathways.[14] The estimation results presented in columns 4-6 confirm these findings.

[14] The large negative effect for severely handicapped retirement is surprising. It may be connected to an early reform of that pathway which increased the NRA from 60 to 63. The reform was grandfathered in and protected those who were severely handicapped at a given date in 2000. To the extent that the share of such protected individuals declines over time, we expect a falling entry rate into severely handicapped retirement which temporally coincides with the linear shift in unemployment retirement incentives (see **Tables 1** and **A.8**).

Finally, we compare the effect size of the two reforms. In hypothesis H5, we argued based on Straubli and Zweimüller (2013) that the effect of reform 2 should exceed that of reform 1 because reform 2 is more strict and renders, e.g., an early retirement at age 60 impossible instead of merely expensive. It may be misleading to simply compare coefficient estimates across reforms because the estimates represent intention to treat (ITT) effects. They are estimated for the population of potential retirees that might consider the unemployment pathway. The results do not account for the share of individuals who will actually be unemployed and subject to the reform. Therefore, we adjust the estimated effects based on the observed share (p) of individuals in pre-reform cohorts that either used the NRA (prior to reform 1) or the ERA (prior to reform 2) of the unemployment pathway. Out of all men born in 1935 or 1936 (1945) in our sample, 29.7 (16.3) percent used the unemployment pathway prior to age 63. The ratio of the ITT estimates over p yields estimates of the average treatment effect of the treated (ATT) (see Angrist and Pischke 2009). **Table 5** restates the estimation results of column 6 of **Tables 3** and **4**, i.e., the causal effect of an additional year of benefit discounts on the propensity to be in each state. Columns 3 and 4 of **Table 5** present the ATT after dividing by p. The resulting estimates for employment (both positive) and unemployment (in absolute terms) are substantially larger for reform 2. The negative effects on old-age retirement are larger for reform 1 than for reform 2. Based on the sum of the effects on all three retirement pathways (old-age, severely handicapped, and disability retirement) reform 2 reduced the propensity to be in some type of retirement by more than reform 1. Thus, overall we confirm H5.

4.3 Heterogeneities

To study effect heterogeneities, we focus on differences by birth cohort and pension wealth at age 55. We show subgroup-specific estimation results for the intensity specification (as in columns 6 in **Tables 3** and **4**) in **Tables 6** and **7** for reforms 1 and 2.

First, we distinguish the reform effects by year of birth, and separately estimate our models for the relevant birth cohorts. Given the monthly variation in the reform intensity measure, the causal effect can be identified separately for each subsample. We expect that the reforms caught particularly the oldest cohorts by surprise. Younger cohorts had more time to adjust after the reform laws were passed. The results for reform 1 and 2 in columns 2-4 of **Tables 6** and **7** show that the reform effect was indeed strongest for the oldest, most surprised cohorts. After reform 1, individuals remain in employment longer or substituted into disability retirement and reduced old-age retirement. After reform 2, the main shifts were from unemployment and old-age retirement into employment. For both reforms, the reform effects are smaller for later birth cohorts.

Next, we study the heterogeneity of reform effects along the pension wealth distribution. We consider individuals who by age 55 amassed low, middle, and high numbers of earnings points. Earnings points represent pension wealth and benefit claims against the retirement insurance. They are higher for individuals who contribute for many years and based on high earnings. We consider three wealth tertiles of similar size (separately delineated for the male and female samples) and show the estimates in columns 5-7 of **Tables 6** and **7** for reforms 1 and 2. We expect that individuals with low pension wealth are most susceptible to potential benefit cuts whereas individuals with high expected retirement benefits may more easily forgo a small fraction of benefits when taking advantage of an earlier retirement. The findings mostly agree with our expectations: after both reforms, those with the lowest pension wealth increased employment the most. At the same time, they changed unemployment the least after reform 1 compared to those with higher pension wealth - possibly because of liquidity constraints. Also, they show the largest decline in old-age retirement entry after both reforms and the largest decline in unemployment for reform 2. The heterogeneity in the utilization of retirement substitution pathways does not show clear patterns.

4.4 Robustness tests

We offer several robustness and placebo tests. We (a) consider estimations with overall and group-specific time trend, (b) show results without females as control group, (c) perform placebo estimations, (d) disregard the restriction that treated observations must be eligible for female retirement, (e) consider a sample with contribution periods in East Germany, (f) consider a donut-estimator for reform 2 where sensitive observation months are omitted and (g) consider logit or probit estimators instead of linear regression.

We show the results for the intensity regressions after considering controls for time trends in **Table 8** for both reforms. Columns (1) and (5) repeat the results from column 6 of **Tables 3** and **4**. Columns (2) and (6) of **Table 8** show the results after adding separate linear time trends for males (treatment group) and females (control group). Generally, prior results are robust. In the columns (3) and (7), we control for a linear trend, its interaction with the men indicator, post-reform cohort indicators, the interaction of men and post-reform indicator, and also with the interaction of men, post-reform and affected age indicator. Again, the main results hold up. Therefore, time trends do not drive our main results.

In columns 4 and 8 of **Table 8**, we show the results after omitting females as a control group. In this setting, the causal effect is identified by comparing men of given ages across birth cohorts, only. For both reforms the findings are generally robust in terms of sign and significance of the estimates. The magnitude of the effects differs depending on whether the female control group is considered (compare columns 1 to 4 and 5 to 8). Using the female control group mostly reduced the effect sizes which may have been overstated otherwise.

In column 9 of **Table 8,** we show placebo results for reform 1, where we consider the birth cohort 1935 as control and cohort 1936 as treated by reform 1. The results confirm that a non-existing reform had no significant effects which supports our setting. We cannot use the placebo test for reform 2 because due to the change in rules for female retirement we use only one pre-reform cohort (see **Table 1**).

In **Table 9,** we show the estimation results for both reforms, first, after adding those male observations who would not have been eligible for female retirement at age 55. Then, we added those observations who had accumulated some employment spells in East Germany. In both cases, our main results hold up to these changes in the sample with only few exceptions.

Our estimations account for an institutional reform of the German unemployment insurance (UI) which shortened the unemployment benefit payout period for unemployment spells starting in February 2006 (see section 2.2). In our specifications for reform 2, we account for the UI reform by controlling for the individual and age-specific maximum entitlement length. However, Riphahn and Schrader (2019) and Dlugosz et al. (2014) show that the reform generated substantial anticipation behavior in terms of earlier unemployment entries. In order to evaluate whether this affects our results, we re-estimated the specification in column 6 of **Table 4** after omitting observations from January-March of 2006. The results in column 7 of **Table 9** show that our findings are robust to this change.

In separate tests, we re-estimated our intensity models to determine whether the marginal effects from logit and probit estimations would agree with our findings (results available upon request). The results hold up to the modified approach.

4.5 Discussion

We tested five hypotheses and evaluated the robustness of the findings. This section discusses the magnitude of the results and offers an alternative perspective on the reform effects.

Based on reform 1, we found that for those aged 60-62 postponing the normal retirement age by one year and introducing a benefit discount of 3.6 percent per year does not significantly affect employment. However, the unemployment incidence in this age range increased by 1.6 percentage points which is a 27 percent effect relative to the pre-reform male sample mean of 5.83 (see last columns of **Table 2**). The old-age retirement rate declined by 8.6 points or 29 percent of the mean and the utilization of health related substitute retirement pathways jointly

increased by 4.4 percentage points or 15.8 percent of the joint mean. While the point estimates are small, the overall effects are substantial and in most cases precisely estimated.

Based on reform 2, we found that for individuals aged 60-62 postponing the early retirement age by one year increases the employment rate by 9.42 percentage points, about 21 percent relative to the pre-reform male sample mean. It reduces the unemployment rate by 5.14 points (48 percent), the old-age retirement rate by 2.5 points (15 percent), and increases the utilization of disability retirement by 0.87 percentage points (9.7 percent).

We can compare these results to those provided by Engels et al. (2017) who investigated the reform of retirement for women in Germany which is very similar to our reform 1. For their sample in the 60-65 age range, they found that a shift in the NRA by one year would increase the employment rate by 3.6 percentage points (3.6 * 1.0), the unemployment rate by 3.24 percentage points (3.6 * 0.9) and the retirement rate would fall by 6.84 percentage points (3.6 * 1.9). While we obtain comparable effects for old-age retirement, their employment and unemployment effects are larger than ours. Unfortunately, the authors do not provide sample statistics. The difference in effect size may be related (i) to the fact that they can observe the two birth cohorts which face an increase in NRA up to age 65 and therefore the highest treatment intensity (up to 60 months of deductions) [15] or (ii) to the fact that a larger population share uses the retirement pathway for women than for the unemployed.

Geyer and Welteke (2019) investigate a reform of the female retirement pathway that is similar to our reform 2: starting with birth cohorts 1952, the early retirement option for women at age 60 was abolished. Instead, women could use early retirement at age 63 via an alternative pathway. Using a regression discontinuity design the authors find that female employment rates increased by 13.5 percentage points or 30 percent due to this reform. As their reform is three times as large as our reform 2 (i.e., we model a loss in ERA of only one year instead of three),

[15] We have to exclude the two comparable birth cohorts 1940 and 1941 most affected by the reform from our analysis (see section 3.1).

our effect of a 9.4 percentage point increase in employment rates is rather large. The reason may be that we investigate a male instead of a female treated sample; Dolls and Krolage (2019) also find larger responses among men than women.[16]

For a different perspective and to investigate effects also for individuals prior retirement age, we additionally consider outcomes that offer summary measures of individuals' labor force states over time. Using precise biographical information, we measure the number of months an individual spent in our five states in three different age ranges (57-59, 60-62, and 57-62) as continuous dependent variables.[17]

We apply a difference-in-differences analysis to this data which holds one cross-sectional observation per person instead of monthly outcomes. Therefore, we cannot control for monthly age fixed effects and have to adjust the definition of some control variables (for details on all variables please see **Tables A.5** and **A.6**).[18]

Table 10 depicts estimates of the reform effect based on the specifications of column 6 in **Tables 3** and **4**. After reform 1, we observe an increase in the number of months spent in employment for all age groups. The unemployment response matches expectations: unemployment declined substantially before age 60 and increased after age 60 with a large negative overall effect. The results show the expected decline in the number of months in old-

[16] The Austrian reform studied by Staubli and Zweimüller (2013) is similar to our reform 2. They find that men and women increased employment by 4.9 and 3.4 percentage points and unemployment by 6.25 and 3.6 percentage points on average for an increase in ERA by one year. The reform studied by Atalay and Barrett (2015) increased the earliest pension age for women in Australia. They find a decline in retirement conditional on eligibility by 19 percentage points for an increase in the NRA by one year. This is somewhat smaller than our ATT estimates in column 3 of **Table 5**.

[17] 57-62 means that we count all months spent from age 57 years, 0 months until age 62, 11 months.

[18] As we use cross-sectional instead of panel observations for each individual, the intensity measure does no longer vary by age. Here, we define intensity as the person's benefit deduction after a retirement at age 60 which generates variation by date of birth. Similarly, controls for institutional reforms of unemployment and disability use age-specific average values and the eligibility for the protection of 'legitimate expectation' (*Vertrauensschutz*) is considered for age 60. The models account only for year of birth fixed effects; we omit month of birth fixed effects and month of age to avoid collinearity (see Mastrobuoni 2009).

age retirement. At the same time, the number of months spent in severely handicapped retirement and disability retirement increased significantly for all age groups. After reform 2, we observe increases in the number of months in employment, declines in unemployment for all age groups, reduced old-age retirement *and* severely handicapped retirement. Among those aged below 60, we find modest but statistically significant substitution into disability retirement. Overall, the patterns are similar to those in response to reform 1.[19]

These results provide a different perspective on the overall reform effects and allow us to sign the reforms' fiscal effects. After reform 1, employment increases and unemployment declines between age 57 and 62. The retirement insurance benefits from a small net decline of about 2 months in the average number of months spent in retirement after the reform (plus 0.6 and 0.4 months in disability and severely handicapped retirement versus minus 3.7 months in old-age retirement). If in response to a change in NRA by one year, individuals on average stay in employment for longer and in unemployment and retirement for a shorter period, the fiscal effect is positive for the social insurances and the taxpayer.

The overall effects of reform 2 are not substantially different. Between age 57 and 62, we observe about 6.6 additional months in employment and 4.7 months less in unemployment due to a shift in the ERA by one year. Overall, individuals spend about 1.5 months less in retirement. So, again the fiscal reform effect is positive. Engels et al. (2017) find larger effects: a shift in the NRA by 5 years generates an overall postponement of retirement by 15 months and a prolongation of employment by the same period (i.e. 3 months per year) with hardly any effects on unemployment. However, as mentioned above, they observe two more birth cohorts who are confronted with larger treatment intensities in ages 60-62 (up to 60 months of

[19] In a robustness test, we account for the fact that individuals born in 1937 and 1938 (1946) had already reached age 59 and 58 in 1996 (2004) when the first (second) reform was passed. While these individual observations are used in the estimations, they were in fact unable to change their behavior at, e.g., age 57 in response to the reform. We re-estimated the models after omitting the birth cohorts above age 57. The results confirm the robustness of our findings (details available upon request).

deductions) and are treated up to age 65. Overall the results of the two studies are comparable and suggest that the reform succeeded in reducing the burden of demographic aging borne by the retirement insurance.

5. Conclusions

This study adds to the literature on the causal effects of shifting retirement entry ages. We exploit two separate reforms of the "unemployment pathway" to old-age retirement in Germany. The pathway offers privileged retirement options for unemployed individuals. The first reform consisted of a stepwise increase of the normal retirement age (NRA) with full benefits, from age 60 to 65 for the birth cohorts of 1937 and after in combination with the contemporaneous introduction of an early retirement age (ERA) with benefit deductions. The second reform increased the ERA stepwise from age 60 to 63 for the birth cohorts 1946 and after. The first reform introduced benefit deductions and made retirement at a given age prior to the NRA more costly. The second reform made early retirement prior to age 63 impossible.

We use administrative data from the mandatory retirement insurance covering a large sample of retirees. We test five hypotheses: we expect that both reforms increase the propensity to stay employed longer (H1), to postpone unemployment from before to after age 60 (H2), to delay retirement (H3), and to use substitute pathways to enter retirement, i.e., disability retirement and retirement of the severely handicapped (H4). Finally, we expect the behavioral adjustments after reform 2 to exceed those after reform 1 (H5).

Our findings confirm hypotheses H1-3 for both reforms. H4 is supported for reform 1, only. Reform 2 appears to have stronger effects than reform 1 thus providing evidence for H5. Overall, both reforms reduced the fiscal burden for the retirement insurance. The findings agree with the prior national and international literature. Heterogeneity tests indicate that individuals who are surprised by the reforms and have little time to adjust delay retirement by more and adjust employment more strongly than those who have time to prepare for regulatory changes.

We find stronger increases in employment and declines in old-age retirement among individuals with the lowest retirement wealth. Our estimates are robust to various tests, changes of the sample and specifications. The results of a placebo test confirm the approach.

Our study stands out in the literature by using rich data, by evaluating two reforms, by looking at a large variety of outcomes, and by applying an identification strategy that compares responses across birth cohorts, age, and for affected and non-affected individuals (difference-in-difference-in-differences estimation). Specifically, we take advantage of the fact that female older workers do not have to rely on the unemployment pathway and therefore are not treated by the reform, and that treatment intensity for men vary by birth and age month.

Our results confirm that the treated men respond to retirement incentives and actively utilize substitute pathways into retirement if those become relatively more attractive. This finding differs from the conclusions of Welteke and Geyer (2019); they find that treated women use 'passive program substitution' in response to reforms and remain in their current labor market status rather than pursuing alternative retirement pathways. Possibly, male and female retirement behaviors follow different patterns as their relative role in spousal retirement choices and timing differ.

Regulatory changes may have potentially unintended distributional effects as those with the lowest retirement wealth change their labor market status much stronger than those who are economically better off. So, while financial incentives appear to be effective in deterring early retirement, the welfare effects of the policy and their heterogeneity across different groups of the population deserve additional attention.

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Table 1 Age at retirement by pathway and birth cohort

Birth	A		B		C		D		E		F	
	Retirement due to unemployment		Retirement for women		Ret. after long term employment		Regular old age retirement		Severely handicapped retirement		Disability retirement	
Cohort	NRA (Yr.)	ERA (Yr.)	NRA (Yr.)	ERA (Yr.)	NRA (Yr.)	ERA (Yr.)	NRA (Yr.)	ERA (Yr.)	NRA (Yr.)	ERA (Yr.)	Full Age (Yr.)	Early Age (Yr.)
1934	60 (1994)	n.a.	60 (1994)	n.a.	63 (1997)	n.a.	65 (1999)	n.a.	60 (1994)	n.a.	^ disability age	n.a.
1935	60 (1995)	n.a.	60 (1995)	n.a.	rising to 64 (1999)	63 (1998)	65 (2000)	n.a.	60 (1995)	n.a.	^ disability age	n.a.
1936	60 (1996)	n.a.	60 (1996)	n.a.	rising to 65 (2001)	63 (1999)	65 (2001)	n.a.	60 (1996)	n.a.	^ disability age	n.a.
1937	rising to 61 (1998)	60 (1997)	60 (1997)	n.a.	65 (2002)	63 (2000)	65 (2002)	n.a.	60 (1997)	n.a.	^ disability age	n.a.
1938	rising to 62 (2000)	60 (1998)	60 (1998)	n.a.	65 (2003)	63 (2001)	65 (2003)	n.a.	60 (1998)	n.a.	63 (2001)	< 63
1939	rising to 63 (2002)	60 (1999)	60 (1999)	n.a.	65 (2004)	63 (2002)	65 (2004)	n.a.	60 (1999)	n.a.	63 (2002)	< 63
1940	rising to 64 (2004)	60 (2000)	rising to 61 (2001)	60 (2000)	65 (2005)	63 (2003)	65 (2005)	n.a.	* rising to 61 (2001)	60 (2000)	63 (2003)	< 63
1941	rising to 65 (2006)	60 (2001)	rising to 62 (2003)	60 (2001)	65 (2006)	63 (2004)	65 (2006)	n.a.	* rising to 62 (2003)	60 (2001)	63 (2004)	< 63
1942	65 (2007)	60 (2002)	rising to 63 (2005)	60 (2002)	65 (2007)	63 (2005)	65 (2007)	n.a.	* rising to 63 (2005)	60 (2002)	63 (2005)	< 63
1943	65 (2008)	60 (2003)	rising to 64 (2007)	60 (2003)	65 (2008)	63 (2006)	65 (2008)	n.a.	* 63 (2006)	60 (2003)	63 (2006)	< 63
1944	65 (2009)	60 (2004)	rising to 65 (2009)	60 (2004)	65 (2009)	63 (2007)	65 (2009)	n.a.	* 63 (2007)	60 (2004)	63 (2007)	< 63
1945	65 (2010)	60 (2005)	65 (2010)	60 (2005)	65 (2010)	63 (2008)	65 (2010)	n.a.	* 63 (2008)	60 (2005)	63 (2008)	< 63
1946	65 (2011)	rising to 61 (2007)	65 (2011)	60 (2006)	65 (2011)	63 (2009)	65 (2011)	n.a.	* 63 (2009)	60 (2006)	63 (2009)	< 63
1947	65 (2012)	rising to 62 (2009)	65 (2012)	60 (2007)	65 (2012)	63 (2010)	rising to 65 1 m.	n.a.	* 63 (2010)	60 (2007)	63 (2010)	< 63
1948	65 (2013)	rising to 63 (2011)	65 (2013)	60 (2008)	65 (2013)	63 (2011)	rising to 65 2 m.	n.a.	* 63 (2011)	60 (2008)	63 (2011)	< 63
1949	65 (2014)	63 (2012)	65 (2014)	60 (2009)	rising to 65 3 m.	63 (2012)	rising to 65 3 m.	n.a.	* 63 (2012)	60 (2009)	rising to 63 7 m.	rising to < 63 7 m.
1950	65 (2015)	63 (2013)	65 (2015)	60 (2010)	rising to 65 4 m.	63 (2013)	rising to 65 4 m.	n.a.	* 63 (2013)	60 (2010)	rising to 63 8 m.	rising to < 63 8 m.
1951	65 (2016)	63 (2014)	65 (2016)	60 (2011)	rising to 65 5 m.	63 (2014)	rising to 65 5 m.	n.a.	63 (2014)	60 (2011)	rising to 63 9 m.	rising to < 63 9 m.
1952	retirement pathway terminated		retirement pathway terminated		rising to 65 6 m.	63 (2015)	rising to 65 6 m.	n.a.	rising to 63 1 m.	rising to 60 1 m.	rising to 63 10 m.	rising to < 63 10 m.
1953					rising to 65 7 m.	63 (2016)	rising to 65 7 m.	n.a.	rising to 63 2 m.	rising to 60 2 m.	rising to 63 11 m.	rising to < 63 11 m.
1954					rising to 65 8 m.	63 (2017)	rising to 65 8 m.	n.a.	rising to 63 3 m.	rising to 60 3 m.	rising to 64 (2008)	rising to < 64 0 m.
1955					rising to 65 9 m.	63 (2018)	rising to 65 9 m.	n.a.	rising to 63 4 m.	rising to 60 4 m.	rising to 64 4 m.	rising to < 64 4 m.
1956					rising to 65 10 m.	63 (2019)	rising to 65 10 m.	n.a.	rising to 63 5 m.	rising to 60 5 m.	rising to 64 6 m.	rising to < 64 6 m.
1957					rising to 65 11 m.	63 (2020)	rising to 65 11 m.	n.a.	rising to 63 6 m.	rising to 60 6 m.	rising to 64 8 m.	rising to < 64 8 m.
1958					rising to 66 (2024)	63 (2021)	rising to 66 (2024)	n.a.	rising to 63 7 m.	rising to 60 7 m.	rising to 64 10 m.	rising to < 64 10 m.
1959					rising to 66 2 m.	63 (2022)	rising to 66 2 m.	n.a.	rising to 63 8 m.	rising to 60 8 m.	rising to 65 (2024)	rising to < 65 0 m.
1960					rising to 66 4 m.	63 (2023)	rising to 66 4 m.	n.a.	rising to 63 9 m.	rising to 60 9 m.	65 (2025)	< 65 0 m.
1961					rising to 66 6 m.	63 (2024)	rising to 66 6 m.	n.a.	rising to 63 10 m.	rising to 60 10 m.	65 (2026)	< 65 0 m.
1962					rising to 66 8 m.	63 (2025)	rising to 66 8 m.	n.a.	rising to 63 11 m.	rising to 60 11 m.	65 (2027)	< 65 0 m.
1963					rising to 66 10 m.	63 (2026)	rising to 66 10 m.	n.a.	rising to 64 (2027)	rising to 61 (2024)	65 (2028)	< 65 0 m.
1964					rising to 67 (2031)	63 (2027)	rising to 67 (2031)	n.a.	rising to 64 1 m.	rising to 61 1 m.	65 (2029)	< 65 0 m.

Notes: * Individuals born before Nov. 17, 1950 and who were severely handicapped on Nov. 16, 2000 can retire at age 60 without deductions.

< 63: Disability retirement prior to age 63 was charged with benefit discounts of up to 10.8 % if retirement entry occurred after 2001.

^ Until 31.12.2000, the "old" disability retirement enabled eligible persons to enter the retirement independently of the age without deductions after the person became disabled.

Source: SGB VI, BMAS (2017), Steffen (2018), and own calculations.

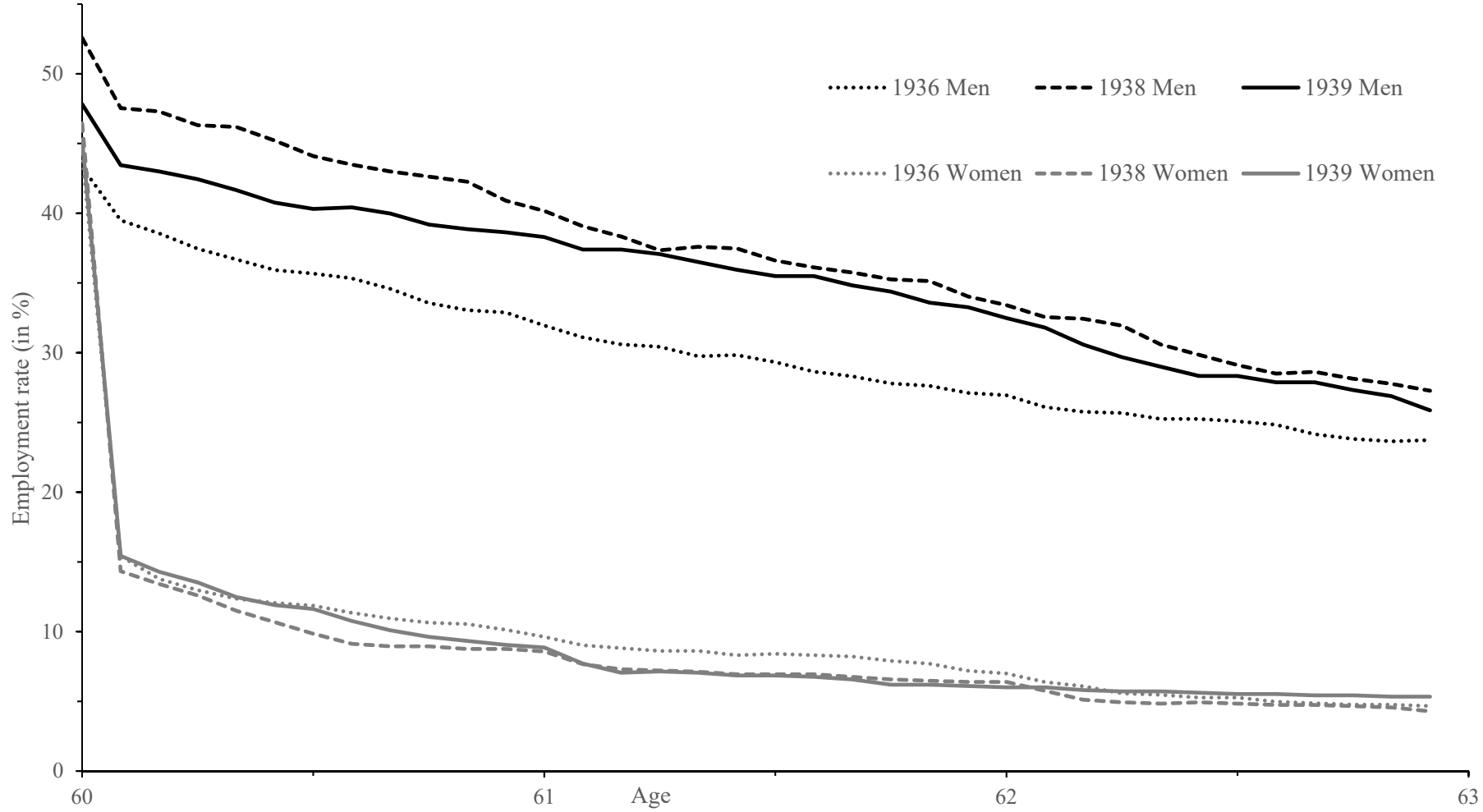
Table 2 Descriptive statistics of dependent variables

	Analysis Sample		Pre-reform means for male sample	
	Reform 1	Reform 2	Reform 1	Reform 2
States				
Employment (0/1)	0.2099	0.3977	0.3064	0.4526
Unemployment (0/1)	0.0414	0.0889	0.0583	0.1070
Old-age retirement (0/1)	0.4899	0.2458	0.2972	0.1628
Severely handicapped retirement (0/1)	0.0818	0.1092	0.1258	0.1259
Disability retirement (0/1)	0.1344	0.0913	0.1528	0.0895
Observations	404,640	308,376	84,636	42,660

Notes: **Tables A.1**-**A.4** describe all variables and provide further descriptive statistics.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

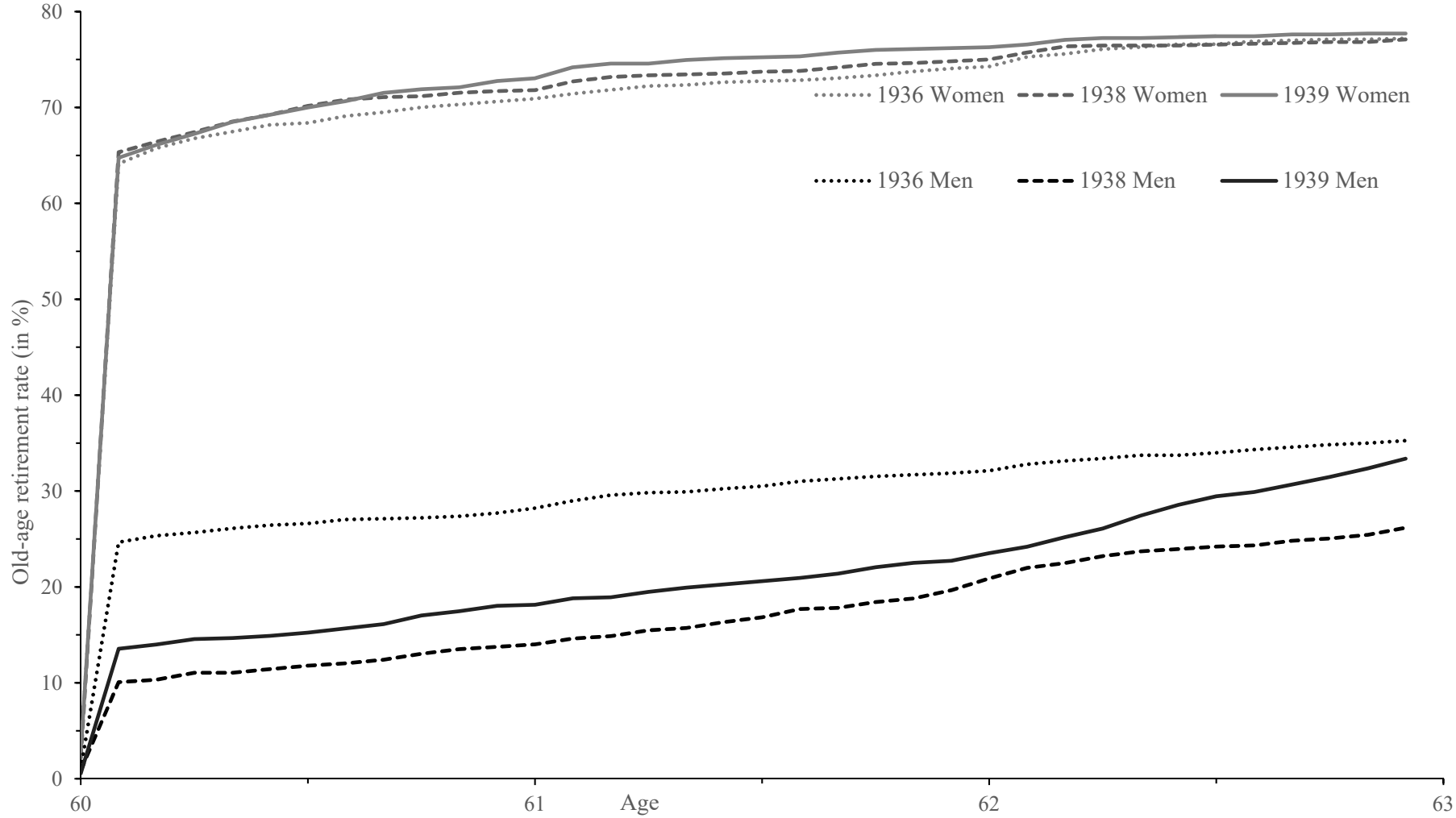
Figure 1 Reform 1 - Employment rate by age, cohort, and gender



Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Note: For the figures, we deleted male, post-reform cohort observations who due to protection of legitimate expectation (measured as of age 60) were not treated by the reforms. We show the gender-specific number of individuals of a given birth cohort and age in employment relative to the number of all individuals in that gender, age, and birth cohort cell in our sample.

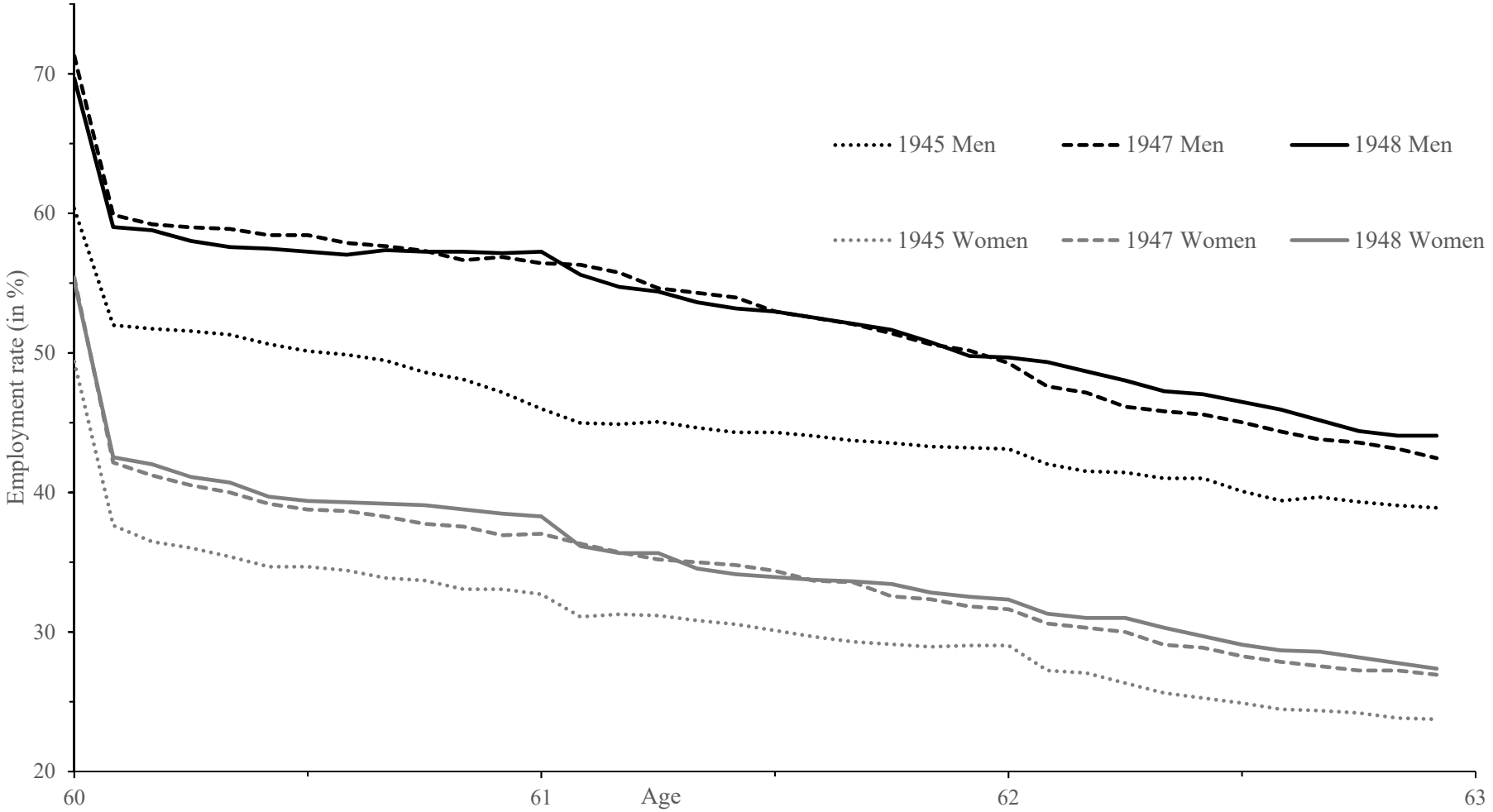
Figure 2 Reform 1 – Old-age retirement rate by age, cohort, and gender



Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Note: For the figures, we deleted male, post-reform cohort observations who due to protection of legitimate expectation (measured as of age 60) were not treated by the reforms.

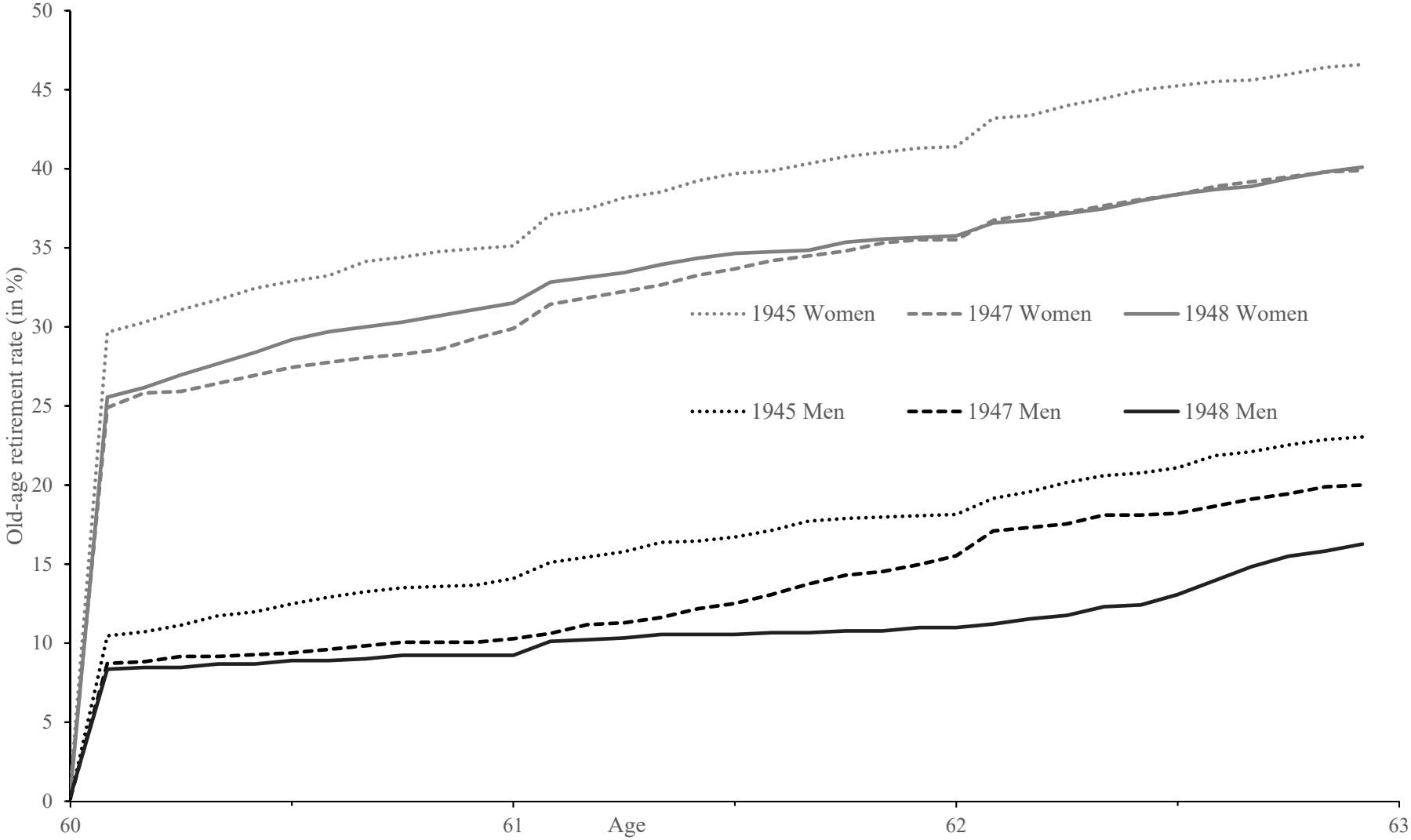
Figure 3 Reform 2 - Employment rate by age, cohort, and gender



Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Note: For the figures, we deleted male, post-reform cohort observations who due to protection of legitimate expectation (measured as of age 60) were not treated by the reforms. We show the gender-specific number of individuals of a given birth cohort and age in employment relative to the number of all individuals in that gender, age, and birth cohort cell in our sample.

Figure 4 Reform 2 – Old-age retirement rate by age, cohort, and gender



Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Note: For the figures, we deleted male, post-reform cohort observations who due to protection of legitimate expectation (measured as of age 60) were not treated by the reforms.

Table 3 Reform 1 - Treatment and treatment intensity effects for the labor force states

	Men x post-reform x age			Intensity		
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
States						
Employment	0.0232**	0.0130	0.0140	0.0158*	0.0073	0.0076
	(0.0115)	(0.0118)	(0.0113)	(0.0084)	(0.0086)	(0.0082)
Unemployment	0.0300***	0.0270***	0.0259***	0.0183***	0.0163***	0.0158***
	(0.0061)	(0.0060)	(0.0059)	(0.0045)	(0.0044)	(0.0044)
Old-age retirement	-0.1680***	-0.1754***	-0.1577***	-0.0888***	-0.0967***	-0.0863***
	(0.0113)	(0.0117)	(0.0119)	(0.0074)	(0.0082)	(0.0083)
Severely handicapped retirement	0.0283***	0.0238***	0.0230***	0.0164***	0.0112*	0.0106*
	(0.0088)	(0.0086)	(0.0084)	(0.0060)	(0.0059)	(0.0058)
Disability retirement	0.0594***	0.0775***	0.0647***	0.0238***	0.0412***	0.0341***
	(0.0083)	(0.0079)	(0.0077)	(0.0060)	(0.0055)	(0.0054)
Controls:						
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Post-reform effects	Yes	No	No	Yes	No	No
Birth fixed effects	No	Yes	Yes	No	Yes	Yes
Individual characteristics	No	No	Yes	No	No	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	404,640	404,640	404,640	404,640	404,640	404,640

Notes: Table shows OLS estimates of the coefficient on "men x post-reform x age" in columns (1)-(3) and on intensity in column (4)-(6). In addition to reported controls, all specifications include an indicator for men. Columns (1) and (4) use interactions of "post-reform" and age fixed effects; and interaction effects of age and birth fixed effects in columns (2)-(3) and (5)-(6). All controls are described in **Table A.3**. Individual level-clustered standard errors (SE) in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 4 Reform 2 - Treatment and treatment intensity effects for the labor force states

	Men x post-reform x age			Intensity		
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
States						
Employment	0.1322***	0.1817***	0.1594***	0.0904***	0.1092***	0.0942***
	(0.0120)	(0.0102)	(0.0099)	(0.0083)	(0.0081)	(0.0077)
Unemployment	-0.0517***	-0.0740***	-0.0620***	-0.0258***	-0.0597***	-0.0514***
	(0.0075)	(0.0091)	(0.0084)	(0.0046)	(0.0067)	(0.0063)
Old-age retirement	-0.0640***	-0.0592***	-0.0758***	-0.0489***	-0.0140**	-0.0250***
	(0.0091)	(0.0097)	(0.0096)	(0.0056)	(0.0070)	(0.0069)
Severely handicapped retirement	-0.0151*	-0.0305***	-0.0229***	-0.0187***	-0.0192***	-0.0145***
	(0.0082)	(0.0080)	(0.0077)	(0.0050)	(0.0058)	(0.0056)
Disability retirement	-0.0069	-0.0092	0.0081	-0.0008	-0.0027	0.0087*
	(0.0071)	(0.0068)	(0.0067)	(0.0049)	(0.0053)	(0.0052)
Controls:						
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Post-reform effects	Yes	No	No	Yes	No	No
Birth fixed effects	No	Yes	Yes	No	Yes	Yes
Individual characteristics	No	No	Yes	No	No	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	308,376	308,376	308,376	308,376	308,376	308,376

Notes: Table shows OLS estimates of the coefficient on “men x post-reform x age” in columns (1)-(3) and on intensity in columns (4)-(6). In addition to reported controls, all specifications include an indicator for men. Columns (1) and (4) use interactions of "post-reform" and age fixed effects; and interaction effects of age and birth fixed effects in columns (2)-(3) and (5)-(6). All controls are described in **Table A.4**. Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 5 Reform 1 and 2 - Comparison of the treatment intensity effects

	Intensity			
	ITT		ATT (=ITT/p)	
	Reform 1	Reform 2	Reform 1	Reform 2
Dependent variable	(1)	(2)	(3)	(4)
States				
Employment	0.0076	0.0942	0.0256	0.5779
Unemployment	0.0158	-0.0514	0.0532	-0.3153
Old-age retirement	-0.0863	-0.0250	-0.2906	-0.1534
Severely handicapped retirement	0.0106	-0.0145	0.0357	-0.0890
Disability retirement	0.0341	0.0087	0.1148	0.0534
Controls:				
Age fixed effects	Yes	Yes	Yes	Yes
Birth fixed effects	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes
Institutions	Yes	Yes	Yes	Yes
Observations	404,640	308,376	404,640	308,376

Notes: Table shows OLS estimates of the coefficient on intensity. In addition to reported controls, all specifications include an indicator for men. All controls for Reform 1 are described in **Table A.3** and for Reform 2 in **Table A.4**. In column (3) we use p = 0.297 as the share of all men born in 1935 or 1936 in our sample who used the unemployment pathway prior to age 63; in column (4) we use p = 0.163 as the share of all men born in 1945 who used the unemployment pathway prior to age 63.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 6 Reform 1 - Heterogeneity of treatment intensity effects

	Intensity						
	Basic estimation	Cohort 1937	Cohort 1938	Cohort 1939	Low pension wealth at 55	Middle pension wealth at 55	High pension wealth at 55
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
States							
Employment	0.0076	0.2172***	0.0426***	-0.0080	0.0697***	-0.0367***	-0.0017
	(0.0082)	(0.0396)	(0.0146)	(0.0097)	(0.0130)	(0.0139)	(0.0149)
Unemployment	0.0158***	-0.0505**	0.0090	0.0197***	0.0073	0.0164**	0.0222***
	(0.0044)	(0.0244)	(0.0087)	(0.0052)	(0.0084)	(0.0070)	(0.0075)
Old-age retirement	-0.0863***	-0.3461***	-0.1328***	-0.0653***	-0.1324***	-0.0611***	-0.0687***
	(0.0083)	(0.0316)	(0.0140)	(0.0098)	(0.0145)	(0.0145)	(0.0138)
Severely handicapped retirement	0.0106*	0.0263	-0.0001	0.0140**	-0.0036	0.0241**	0.0068
	(0.0058)	(0.0200)	(0.0098)	(0.0069)	(0.0095)	(0.0111)	(0.0088)
Disability retirement	0.0341***	0.1541***	0.0658***	0.0230***	0.0323***	0.0436***	0.0274***
	(0.0054)	(0.0274)	(0.0103)	(0.0062)	(0.0116)	(0.0093)	(0.0070)
Controls:							
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	404,640	237,096	239,292	239,148	133,560	133,560	137,520

Notes: Table shows OLS estimates of the coefficient on intensity. In addition to reported controls, all specifications include an indicator for men and interaction effects of age and birth fixed effects. All controls are described in **Table A.3**. Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations**.**

Table 7 Reform 2 - Heterogeneity of treatment intensity effects

	Intensity						
	Basic estimation	Cohort 1946	Cohort 1947	Cohort 1948	Low pension wealth at 55	Middle pension wealth at 55	High pension wealth at 55
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
States							
Employment	0.0942***	0.2382***	0.1341***	0.0770***	0.1647***	0.0837***	0.0786***
	(0.0077)	(0.0337)	(0.0146)	(0.0095)	(0.0216)	(0.0124)	(0.0125)
Unemployment	-0.0514***	-0.0896***	-0.0620***	-0.0462***	-0.1500***	-0.0447***	-0.0138**
	(0.0063)	(0.0268)	(0.0120)	(0.0078)	(0.0216)	(0.0098)	(0.0065)
Old-age retirement	-0.0145***	-0.0584***	-0.0159	-0.0123*	-0.0227	-0.0117	-0.0173**
	(0.0056)	(0.0216)	(0.0098)	(0.0071)	(0.0149)	(0.0092)	(0.0088)
Severely handicapped retirement	-0.0250***	-0.0886***	-0.0576***	-0.0144*	-0.0170	-0.0289***	-0.0332***
	(0.0069)	(0.0259)	(0.0130)	(0.0086)	(0.0110)	(0.0105)	(0.0127)
Disability retirement	0.0087*	-0.0021	0.0122	0.0082	0.0357*	0.0078	0.0056
	(0.0052)	(0.0183)	(0.0080)	(0.0067)	(0.0191)	(0.0092)	(0.0048)
Controls:							
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	308,376	161,352	156,348	156,348	101,772	101,808	104,796

Notes: Table shows OLS estimates of the coefficient on intensity. In addition to reported controls, all specifications include an indicator for men and interaction effects of age and birth fixed effects. All controls are described in **Table A.4**. Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 8 Reforms 1 and 2 - Treatment intensity effects with controls for time trends, when omitting females as control group, and placebo estimation

	Intensity								Men x post-reform x age
	Reform 1				Reform 2				Reform 1
	Basic estimation	Time trend men	Time trend men & post & age	No female control group	Basic estimation	Time trend men	Time trend men & post & age	No female control group	Reform 1936*
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
States									
Employment	0.0076	0.0019	0.0004	-0.0031	0.0942***	0.1074***	0.1210***	0.1226***	0.0110
	(0.0082)	(0.0088)	(0.0096)	(0.0092)	(0.0077)	(0.0091)	(0.0102)	(0.0089)	(0.0204)
Unemployment	0.0158***	0.0136***	0.0129***	0.0141***	-0.0514***	-0.0564***	-0.0615***	-0.0647***	0.0068
	(0.0044)	(0.0046)	(0.0048)	(0.0048)	(0.0063)	(0.0071)	(0.0081)	(0.0075)	(0.0065)
Old-age retirement	-0.0863***	-0.0890***	-0.0941***	-0.0955***	-0.0250***	-0.0393***	-0.0459***	-0.0403***	-0.0114
	(0.0083)	(0.0088)	(0.0097)	(0.0092)	(0.0069)	(0.0082)	(0.0093)	(0.0080)	(0.0254)
Severely handicapped retirement	0.0106*	0.0163***	0.0176***	0.0204***	-0.0145***	-0.0112*	-0.0132*	-0.0123*	-0.0089
	(0.0058)	(0.0062)	(0.0068)	(0.0065)	(0.0056)	(0.0066)	(0.0075)	(0.0069)	(0.0147)
Disability retirement	0.0341***	0.0374***	0.0392***	0.0436***	0.0087*	0.0110*	0.0129*	0.0077	-0.0236
	(0.0054)	(0.0058)	(0.0064)	(0.0058)	(0.0052)	(0.0062)	(0.0071)	(0.0061)	(0.0200)
Controls:									
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trend	No	Yes	Yes	No	No	Yes	Yes	No	No
Time trend x men	No	Yes	Yes	No	No	Yes	Yes	No	No
Time trend x post-reform	No	No	Yes	No	No	No	Yes	No	No
Time trend x men x post-reform	No	No	Yes	No	No	No	Yes	No	No
Time trend x men x post-reform x age	No	No	Yes	No	No	No	Yes	No	No
Observations	404,640	404,640	404,640	218,412	308,376	308,376	308,376	159,840	155,448

Notes: Table shows OLS estimates of the coefficient on intensity in columns (1)-(8) and on "men x post-reform x age" in column (9). In addition to reported controls, all specifications except for columns (4) and (8) include an indicator for men and all control for interaction effects of age and birth fixed effects. For a description of all controls for reforms 1 and 2 see **Table A.3** and **A.4**, respectively. Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

* Placebo reform is set so that cohort 1935 is pre-reform and cohort 1936 is only post-reform cohort.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 9 Reforms 1 and 2 - Treatment intensity effects when adding non-eligible men and those with East German spells and when omitting observations in January-March 2006

	Intensity						
	Reform 1			Reform 2			
	Basic estimation	With non-eligible men	With times in East Germany	Basic estimation	With non-eligible men	With times in East Germany	Without Jan-Mar 2006
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
States							
Employment	0.0076	-0.0048	0.0111	0.0942***	0.0625***	0.0946***	0.0947***
	(0.0082)	(0.0060)	(0.0068)	(0.0077)	(0.0058)	(0.0067)	(0.0078)
Unemployment	0.0158***	-0.0034	0.0154***	-0.0514***	-0.0575***	-0.0619***	-0.0511***
	(0.0044)	(0.0034)	(0.0039)	(0.0063)	(0.0054)	(0.0058)	(0.0063)
Old-age retirement	-0.0863***	-0.0626***	-0.0928***	-0.0250***	-0.0081	-0.0227***	-0.0256***
	(0.0083)	(0.0063)	(0.0071)	(0.0069)	(0.0052)	(0.0062)	(0.0070)
Severely handicapped retirement	0.0106*	0.0088**	0.0098**	-0.0145***	-0.0051	-0.0138***	-0.0143**
	(0.0058)	(0.0042)	(0.0046)	(0.0056)	(0.0041)	(0.0051)	(0.0056)
Disability retirement	0.0341***	0.0371***	0.0383***	0.0087*	0.0140***	0.0118***	0.0088*
	(0.0054)	(0.0041)	(0.0046)	(0.0052)	(0.0040)	(0.0045)	(0.0052)
Controls:							
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	404,640	533,952	606,096	308,376	401,616	413,172	300,581

Notes: Table shows OLS estimates of the coefficient on intensity. In addition to reported controls, all specifications include an indicator for men and interaction effects of age and birth fixed effects. Specification in column (2) and (5) include an indicator for eligibility and specification in column (3) and (6) an indicator for times in East Germany. For a description of all controls for reforms 1 and 2 see **Table A.3** and **A.4**, respectively. Columns (2) and (5) add observations on men to the baseline sample who failed to meet the eligibility requirements of female retirement. Columns (3) and (6) add those observations on men and women to the baseline sample who at some point earned pension points in East Germany. Column (7) omits observations from January 1 to March 31 2006 for reform 2.
Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table 10 Reform 1 and 2 - Treatment intensity effects for the number of months in labor force status

	Intensity	
	Reform 1	Reform 2
Dependent variable	(1)	(2)
Months in employment		
Age 57-59	2.7454***	3.8989***
	(0.2374)	(0.2878)
Age 60-62	1.6730***	2.5683***
	(0.2189)	(0.2885)
Age 57-62	4.4215***	6.6862***
	(0.4127)	(0.5256)
Months in unemployment		
Age 57-59	-4.2429***	-3.9444***
	(0.2099)	(0.2444)
Age 60-62	0.4058***	-0.8941***
	(0.0926)	(0.1707)
Age 57-62	-3.8466***	-4.8133***
	(0.2284)	(0.3405)
Months in old-age retirement		
Age 60-62	-3.7387***	-1.4853***
	(0.2331)	(0.2535)
Months in severely handicapped retirement		
Age 60-62	0.4376***	-0.4729**
	(0.1685)	(0.2058)
Months in disability retirement		
Age 57-59	0.9290***	0.4262***
	(0.1402)	(0.1192)
Age 60-62	0.6740***	0.0793
	(0.1708)	(0.1851)
Age 57-62	1.6032***	0.4656*
	(0.2841)	(0.2642)
Controls:		
Birth fixed effects	Yes	Yes
Individual characteristics	Yes	Yes
Institutions	Yes	Yes
Observations	11,240	8,566

Notes: Table shows OLS estimates of the coefficient on intensity in column (1) and (2). In addition to reported controls, all specifications include an indicator for men. All controls are described in **Tables A.7** and **A.8**. Individual level-clustered SE in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Appendix Tables

Table A.1 Benefit reductions after disability retirement by month of age and retirement

	Deductions for a retirement entry with age…																							
Birth cohort	61+0	61+1	61+2	61+3	61+4	61+5	61+6	61+7	61+8	61+9	61+10	61+11	62+0	62+1	62+2	62+3	62+4	62+5	62+6	62+7	62+8	62+9	62+10	62+11
1.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
2.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.3
3.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.6	0.3
4.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.9	0.6	0.3
5.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1.2	0.9	0.6	0.3
6.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1.5	1.2	0.9	0.6	0.3
7.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1.8	1.5	1.2	0.9	0.6	0.3
8.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2.1	1.8	1.5	1.2	0.9	0.6	0.3
9.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
10.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
11.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	0	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
12.1938	0	0	0	0	0	0	0	0	0	0	0	0	0	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
1.1939	0	0	0	0	0	0	0	0	0	0	0	0	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
2.1939	0	0	0	0	0	0	0	0	0	0	0	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
3.1939	0	0	0	0	0	0	0	0	0	0	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
4.1939	0	0	0	0	0	0	0	0	0	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
5.1939	0	0	0	0	0	0	0	0	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
6.1939	0	0	0	0	0	0	0	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
7.1939	0	0	0	0	0	0	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
8.1939	0	0	0	0	0	5.7	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
9.1939	0	0	0	0	6	5.7	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
10.1939	0	0	0	6.3	6	5.7	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
11.1939	0	0	6.6	6.3	6	5.7	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3
12.1939	0	6.9	6.6	6.3	6	5.7	5.4	5.1	4.8	4.5	4.2	3.9	3.6	3.3	3	2.7	2.4	2.1	1.8	1.5	1.2	0.9	0.6	0.3

Source: Steffen (2018), own calculations.

Table A.2 Duration of unemployment benefit receipt (in months) by age

Age	July 1987-Jan.2006	Feb. 2006-Dec. 2007	since Jan. 2008
< 45	12	12	12
45-46	18	12	12
47-49	22	12	12
50-51	22	12	15
52-54	26	12	15
55-56	26	18	18
57	32	18	18
> 57	32	18	24

Notes: Column 1 describes the age cutoffs. The cut in benefit durations as of February 2006 affected those unemployed since February 1, 2006. The prolongation of unemployment benefit durations as of January 2008 affected those entering unemployment on or after January 1, 2008 and aged 50 or 58 at that time, or those still receiving unemployment benefits from a prior entry to unemployment on January 1, 2008 and aged at least 50 or 58 at that time.

Source: Riphahn and Schrader (2019).

Table A.3 Reform 1 – List of variables

Variable	Description
Dependent variables	
Employment (0/1)	Employed with age 60-62 (monthly status)
Unemployment (0/1)	Unemployed with age 60-62 (monthly status)
Old-age retirement (0/1)	Retired (old-age without severely handicapped) with age 60-62 (monthly status)
Severely handicapped retirement (0/1)	Retired (severely handicapped) with age 60-62 (monthly status)
Disability retirement (0/1)	Retired (disability) with age 60-62 (monthly status)
Independent variables	
Reform indicators	
Men (0/1)	Men (ref. = women)
Age fixed effects	
Age1-Age36 (0/1)	Monthly age from 60 years+0 months to 62 years and 11 months (ref. = 60 + 0)
Men x Age60 - Men x Age62 (0/1)	Men times age from 60 years to 62 years (ref. = women of same age)
Post-reform effects	
Post-reform (0/1)	Individuals born in 1937-39 (ref. = 1935-36)
Men x post-reform (0/1)	Men times individuals born in 1937-39 (ref. = women born in 1935-39, and men born in 1935-36)
Post-reform x Age60 - Post-reform x Age62 (0/1)	Individuals born in 1937-39 times age from 60 years to 62 years (ref. = individuals born in 1935-36 of same age)
Birth fixed effects	
Birth1-Birth60 (0/1)	Month and year of birth from January 1935 to December 1939 (ref. = January 1935)
Men x Birth1935 -Men x Birth1939 (0/1)	Men times year of birth from 1935 to 1939 (ref. = men born in 1935)
Birth1935 x Age60 - Birth1939 x Age62 (0/1)	Year of birth from 1935 to 1939 times age from 60 years to 62 years (ref. = 1935 times age 60, 61 or 62)
Treatment effect indicators	
Men x post-reform x age (0/1)	Men times individuals born in 1937-39 times individuals in affected age (ref. = women born in 1935-39, men born in 1935-36, and men born in 1937-39 in non-affected age (**Table A.9**) or with protected 'legitimate expectations')
Intensity	Increase of NRA/deductions for early retirement for men born in 1937-39 measured in years (monthly status, ref. = women born in 1935-39, men born in 1935-36, and men born in 1937-39 with zero deductions (**Table A.9**) or protected 'legitimate expectations')
Individual characteristics	
Past earnings	Earning points (pension calculation) until age 55
Months poor health	Months in disability/sickness until age 55
Months tenure	Months of last employment until age 55
Blue collar, White collar, and other (0/1)	Insurance group at age 55 (ref. = White collar)
Institutions	
Deductions disability retirement	(Potential) deductions of the pension benefit for disability retirement in percent (monthly status)
Time trend	
Time trend	Linear time trend for month of observation from January 1995 to November 2002
Time trend x men	Linear time trend times men
Time trend x post-reform	Linear time trend times individuals born in 1937-39
Time trend x men x post-reform	Linear time trend times men times individuals born in 1937-39
Time trend x men x post-reform x age	Linear time trend times men times individuals born in 1937-39 times individuals in affected age

Table A.4 Reform 2 – List of variables

Variable	Description
Dependent variables	
Employment (0/1)	Employed with age 60-62 (monthly status)
Unemployment (0/1)	Unemployed with age 60-62 (monthly status)
Old-age retirement (0/1)	Retired (old-age without severely handicapped) with age 60-62 (monthly status)
Severely handicapped retirement (0/1)	Retired (severely handicapped) with age 60-62 (monthly status)
Disability retirement (0/1)	Retired (disability) with age 60-62 (monthly status)
Independent variables	
Reform indicators	
Men (0/1)	Men (ref. = women)
Age fixed effects	
Age1-Age36 (0/1)	Monthly age from 60 years+0 months to 62 years and 11 months (ref. = 60 + 0)
Men x Age60 - Men x Age62 (0/1)	Men times age from 60 years to 62 years (ref. = women of same age)
Post-reform effects	
Post-reform (0/1)	Individuals born in 1946-48 (ref. = 1945)
Men x post-reform (0/1)	Men times individuals born in 1946-48 (ref. = women born in 1946-48, and men born in 1945)
Post-reform x Age60 - Post-reform x Age62 (0/1)	Individuals born in 1946-48 times age from 60 years to 62 years (ref. = individuals born in 1945 of same age)
Birth fixed effects	
Birth1-Birth48 (0/1)	Month and year of birth from January 1945 to December 1948 (ref. = January 1945)
Men x Birth1945 -Men x Birth1948 (0/1)	Men times year of birth from 1945 to 1948 (ref. = men born in 1945)
Birth1945 x Age60 - Birth1948 x Age62 (0/1)	Year of birth from 1945 to 1948 times age from 60 years to 62 years (ref. = 1945 times age 60/61/62)
Treatment effect indicators	
Men x post-reform x age (0/1)	Men times individuals born in 1946-48 times individuals in affected age (ref. = women born in 1945-48, men born in 1945, and men born in 1946-48 in non-affected age (**Table A.10**) or with protected 'legitimate expectations')
Intensity	Increase of ERA for men born in 1946-48 measured in years (monthly status, ref. = women born in 1945-48, men born in 1945, and men born in 1946-48 with zero increase (**Table A.10**) or protected 'legitimate expectations')
Individual characteristics	
Past earnings	Earning points (pension calculation) until age 55
Months poor health	Months in disability/sickness until age 55
Months tenure	Months of last employment until age 55
Blue collar, White collar, and other (0/1)	Insurance group at age 55 (ref. = White collar)
Institutions	
UB months	Maximum length of (potential) unemployment benefit (UB) receipt (monthly)
Time trend	
Time trend	Linear time trend for month of observation from January 2005 to November 2011
Time trend x men	Linear time trend times men
Time trend x post-reform	Linear time trend times individuals born in 1946-48
Time trend x men x post-reform	Linear time trend times men times individuals born in 1946-48
Time trend x men x post-reform x age	Linear time trend times men times individuals born in 1946-48 times individuals in affected age

Table A.5 Reform 1 – Descriptive statistics of variables as described in **Table A.3**

	Mean	SD	Min	Max	N
Dependent variables					
Employment (0/1)	0.2099	0.4072	0	1	404,640
Unemployment (0/1)	0.0414	0.1993	0	1	404,640
Old-age retirement (0/1)	0.4899	0.4999	0	1	404,640
Severely handicapped retirement (0/1)	0.0818	0.2741	0	1	404,640
Disability retirement (0/1)	0.1344	0.3410	0	1	404,640
Independent variables					
Reform indicators					
Men (0/1)	0.5398	0.4984	0	1	404,640
Age fixed effects					
Age1-Age36 (discrete)	61.4583	0.8657	60	62.9167	404,640
Men x Age60 - Men x Age62 (discrete)	32.9259	30.4093	0	62	404,640
Post-reform effects					
Post-reform (0/1)	0.6158	0.4864	0	1	404,640
Men x post-reform (0/1)	0.3306	0.4704	0	1	404,640
Post-reform x Age60 - Post-reform x Age62 (0/1)	37.5660	29.6772	0	62	404,640
Birth fixed effects					
Birth1-Birth60 (discrete)	30.9578	17.3235	1	60	404,640
Men x Birth1935 -Men x Birth1939 (discrete)	1,045.5552	965.4549	0	1939	404,640
Birth1935 x Age60 - Birth1939 x Age62 (discrete)	118,160	1,583.9335	116,100	120,218	404,640
Treatment effect indicators					
Men x post-reform x age (0/1)	0.1045	0.3059	0	1	404,640
Intensity	0.1145	0.4089	0	3	404,640
Individual characteristics					
Past earnings	32.7003	12.4666	3.6847	63.5557	404,640
Months poor health	4.5265	7.7551	0	83	404,640
Months tenure at age 55	197.6244	144.3428	1	495	404,640
Insurance group (categorial)	1.4617	0.5451	0	2	404,640
Institutions					
Deductions disability retirement	0.3296	1.0373	0.0000	6.9000	404,640
Time trend					
Time trend	48.4578	20.1995	1	95	404,640
Time trend x men	26.1470	28.3638	0	95	404,640
Time trend x post-reform	36.9736	31.3990	0	95	404,640
Time trend x men x post-reform	19.8930	29.5409	0	95	404,640
Time trend x men x post-reform x age	10.3204	23.4979	0	95	404,640

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table A.6 Reform 2 – Descriptive statistics of variables as described in **Table A.4**

	Mean	SD	Min	Max	N
Dependent variables					
Employment (0/1)	0.3977	0.4894	0	1	308,376
Unemployment (0/1)	0.0889	0.2846	0	1	308,376
Old-age retirement (0/1)	0.2458	0.4306	0	1	308,376
Severely handicapped retirement (0/1)	0.1092	0.3118	0	1	308,376
Disability retirement (0/1)	0.0913	0.2881	0	1	308,376
Independent variables					
Reform indicators					
Men (0/1)	0.5183	0.4997	0	1	308,376
Age fixed effects					
Age1-Age36 (discrete)	61.4583	0.8657	60	62.9167	308,376
Men x Age60 - Men x Age62 (discrete)	31.6180	30.4852	0	62	308,376
Post-reform effects					
Post-reform (0/1)	0.7314	0.4432	0	1	308,376
Men x post-reform (0/1)	0.3800	0.4854	0	1	308,376
Post-reform x Age60 - Post-reform x Age62 (0/1)	44.6142	27.0468	0	62	308,376
Birth fixed effects					
Birth1-Birth60 (discrete)	23.6623	13.9459	1	48	308,376
Men x Birth1945 -Men x Birth1948 (discrete)	1,008.8981	972.5709	0	1948	308,376
Birth1945 x Age60 - Birth1948 x Age62 (discrete)	118,733	1,590.7455	116,700	120,776	308,376
Treatment effect indicators					
Men x post-reform x age (0/1)	0.1630	0.3694	0	1	308,376
Intensity	0.1700	0.4809	0	3	308,376
Individual characteristics					
Past earnings	24.4363	18.1024	0	61.9806	308,376
Months poor health	5.6866	9.8769	0	116	308,376
Months tenure at age 55	186.8324	143.9480	1	494	308,376
Insurance group (categorial)	1.4512	0.5559	0	2	308,376
Institutions					
UB months	22.4116	3.7076	18.0000	32.0000	308,376
Time trend					
Time trend	41.1623	17.3898	1	83	308,376
Time trend x men	21.3221	24.0672	0	83	308,376
Time trend x post-reform	34.8498	24.5308	0	83	308,376
Time trend x men x post-reform	18.0749	24.7864	0	83	308,376
Time trend x men x post-reform x age	9.0400	19.7282	0	83	308,376

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.

Table A.7 Intensity of reform 1 by retirement age and date of birth

Year and months of birth																																			
Retirement age		1937												1938												1939											
Year	Months	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12
60	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
60	1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
60	2	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
60	3	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
60	4	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
60	5	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
60	6	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
60	7	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
60	8	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
60	9	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
60	10	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
60	11	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
61	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
61	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
61	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
61	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
61	4	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
61	5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
61	6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
61	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
61	8	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
61	9	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
61	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
61	11	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13
62	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12
62	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11
62	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10
62	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9
62	4	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8
62	5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7
62	6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6
62	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5
62	8	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4
62	9	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3
62	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2
62	11	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1

Notes: The cells show the number of months for which an individual has to accept benefit discounts depending on retirement age and date of birth. As an example, individuals born in December of 1937 have to accept 12 months of discount (0.3 % each, totaling 3.6%) if they retire as soon as they reach age 60 (0 months).

Source: SGB VI Anlage 19, own calculations.

Table A.8 Intensity of reform 2 by retirement age and date of birth

Year and months of birth																																			
Retirement age		1946												1947												1948											
Year	Months	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12
60	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
60	1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
60	2	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
60	3	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
60	4	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
60	5	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
60	6	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
60	7	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
60	8	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
60	9	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
60	10	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
60	11	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
61	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
61	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
61	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
61	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
61	4	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
61	5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
61	6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
61	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
61	8	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
61	9	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
61	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
61	11	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12	13
62	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11	12
62	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10	11
62	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9	10
62	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8	9
62	4	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7	8
62	5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6	7
62	6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5	6
62	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4	5
62	8	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3	4
62	9	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	3
62	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2
62	11	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1

Notes: The cells show the number of months by which the first option for early retirement at a given age has been postponed after reform 2 depending on retirement age and date of birth. As an example, individuals born in December of 1946 who wanted to retire exactly up reaching age 60 have to wait 12 months after the reform until early retirement becomes available to them for the first time.

Source: SGB VI Anlage 19, own calculations.

Table A.9 Reform 1 and 2 – List of variables for estimations with months (section 4.5)

Variable	Description
Dependent variables	
Months in…	Number of months considered in three different age ranges (57-59, 60-62, and 57-62) in state…
...employment	...employment
...unemployment	...unemployment
...old-age retirement	...old-age retirement
...severely handicapped retirement	...severely handicapped retirement
...disability retirement	...disability retirement
Independent variables	
Reform indicators - Reform 1	
Men (0/1)	Men (ref. = women)
Post-reform effects	
Post-reform (0/1)	Individuals born in 1937-39 (ref. = 1935-36)
Birth fixed effects	
Cohort1-Cohort5 (0/1)	Year of birth from 1935 to 1939 (ref. = 1935)
Treatment effect indicators	
Men x post-reform (0/1)	Men times individuals born in 1937-39 (ref. = women born in 1935-39, and men born in 1935-36, and men born in 1937-39 with protected 'legitimate expectations')
Intensity	Increase of NRA/deductions for early retirement for men born in 1937-39 measured in years (status at age 60, ref. = women born in 1935-39, men born in 1935-36, and men born in 1937-39 with protected 'legitimate expectations')
Reform indicators - Reform 2	
Men (0/1)	Men (ref. = women)
Post-reform effects	
Post-reform (0/1)	Individuals born in 1946-48 (ref. = 1945)
Birth fixed effects	
Cohort1-Cohort4 (0/1)	Year of birth from 1945 to 1948 (ref. = 1945)
Treatment effect indicators	
Men x post-reform (0/1)	Men times individuals born in 1946-48 (ref. = women born in 1945-48, men born in 1945, and men born in 1946-48 with protected 'legitimate expectations')
Intensity	Increase of ERA for men born in 1946-48 measured in years (status at age 60, ref. = women born in 1945-48, men born in 1945, and men born in 1946-48 with protected 'legitimate expectations')
Institutions - Reform 1	
Deductions disability retirement	(Potential) deductions of the pension benefit for disability retirement in percent (mean for respective age range)
Institutions - Reform 2	
UB months	Maximum length of (potential) unemployment benefit (UB) receipt (mean for respective age range)
Individual characteristics	
Past earnings	Earning points (pension calculation) until age 55
Months poor health	Months in disability/sickness until age 55
Months tenure	Months of last employment until age 55
Blue collar, White collar, and other (0/1)	Insurance group at age 55 (ref. = White collar)

Table A.10 Reform 1 and 2– Descriptive statistics of variables as described in **Table A.7**

	Sample reform 1		Sample reform 2	
	Mean	SD	Mean	SD
Dependent variables				
Months in employment				
Age 57-59	20.2392	15.7508	22.6653	16.1399
Age 60-62	7.5568	13.2244	14.3165	16.3012
Age 57-62	27.796	25.5315	36.9818	29.6036
Months in unemployment				
Age 57-59	8.4468	13.0634	5.9608	11.5827
Age 60-62	1.492	4.5235	3.2008	8.2374
Age 57-62	9.9388	13.825	9.1616	16.379
Months in old-age retirement				
Age 60-62	17.6355	16.7271	8.8503	14.254
Months in severely handicapped retirement				
Age 60-62	2.946	9.3273	3.9295	10.4985
Months in disability retirement				
Age 57-59	3.5988	9.7044	1.7996	7.3062
Age 60-62	4.8373	11.8516	3.2881	9.9548
Age 57-62	8.4361	19.9409	5.0877	15.6595
Independent variables				
Reform indicators				
Men (0/1)	0.5398	0.4984	0.5183	0.4997
Post-reform effects				
Post-reform (0/1)	0.6158	0.4864	0.7314	0.4433
Birth fixed effects				
Cohort1-Cohort5/Cohort1-Cohort4 (discrete)	3.0454	1.4110	2.4465	1.1231
Treatment effect indicators				
Men x post-reform (0/1)	0.2168	0.4121	0.3234	0.4678
Intensity	0.3456	0.7700	0.4890	0.8605
Individual characteristics				
Past earnings	32.7003	12.4672	24.4363	18.1034
Months poor health	4.5265	7.7554	5.6866	9.8775
Months tenure at age 55	197.6244	144.3490	186.8324	143.9562
Insurance group (categorial)	1.4617	0.5451	1.4512	0.5559
Institutions				
Ref. 1: Deductions disability retirement/ Ref. 2: UB months	0.3296	0.6029	22.4116	1.2432

Notes: Sample of reform 1 consists of 11,240 observations and sample of reform 2 of 8,566 observations. Statistics of "Deductions disability retirement" and "UB months" relate to the observations in age range 60-62.

Source: SUFVSKT2002_FAU_Schrader-SUFVSKT2013_FAU_Schrader, own calculations.