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Niklas Gohl

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University of Potsdam
August-Bebel-Straße 89, 14482 Potsdam
Tel.: +49 331 977-3225
Fax: +49 331 977-3210
E-Mail: dp-cepa@uni-potsdam.de

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Niklas Gohl
University of Potsdam, Berlin School of Economics, DIW

ABSTRACT

Leveraging two cohort-specific pension reforms, this paper estimates the forward-looking effects of an exogenous increase in the working horizon on (un)employment behaviour for individuals with a long remaining statutory working life. Using difference-in-differences and regression discontinuity approaches based on administrative and survey data, I show that a longer legal working horizon increases individuals' subjective expectations about the length of their work life, raises the probability of employment, decreases the probability of unemployment, and increases the intensity of job search among the unemployed. Heterogeneity analyses show that the demonstrated employment effects are strongest for women and in occupations with comparatively low physical intensity, i.e., occupations that can be performed at older ages.

Keywords: retirement policies, employment, DiD
JEL Codes: J24, J26, H21

Corresponding author:
Niklas Gohl
University of Potsdam
Chair of Public Sector, Finance and Social Policy
August-Bebel-Str. 89
14482 Potsdam, Germany
E-mail: ngohl@uni-potsdam.de

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1 Introduction

Rising life expectancy is putting pressure on pay-as-you-go pension systems in most developed economies. In response, over the past three decades, many OECD countries have raised their statutory retirement ages to extend the working lives of their citizens and reduce the fiscal burden of an aging society. Many of these pension reforms were announced before the affected individuals actually reached the retirement threshold, which can lead to shifts in expected work horizons and affect employment outcomes and job search behaviour among relatively young individuals with long remaining working lives. This study explicitly examines these potential forward-looking effects of pension reforms. To this end, it uses both difference-in-differences (DiD) and regression discontinuity (RDD) approaches, analyzes high-quality administrative and survey data, and exploits cohort-specific pension reforms in Germany to estimate the effects of an exogenous extension of statutory working lives on individual pension expectations, anticipatory employment responses, and job search behaviour.

The study documents four main findings. First, I show that even small changes in the statutory working life caused by pension reforms lead to a shift in the length of individuals' subjectively expected working horizon. Second, a longer working horizon significantly increases employment probabilities of relatively young individuals and has a negative effect on the probability of being registered as unemployed during a given year. The positive employment effects are driven by inflows into employment as well as lower outflows out of employment. Third, the paper provides suggestive evidence that individuals with a longer working horizon have a higher job search intensity and receive more job offers by job agency officials. Fourth, exploiting the large sample size of the administrative data set used in this study, I show that the documented employment effects are driven by individuals in jobs with lower physical intensity, i.e., jobs that can be exercised at older ages.

The exogenous variation used to identify the described effects stems from two pension reforms in Germany.[1] First and foremost, I study the 2007 pension reform, which step by step shifted the normal retirement age (NRA) from 65 to 67 for cohorts 1947-1964.

[1] In Germany, state pension rules do not only provide economic incentives but, as documented by Seibold (2021), set the legal framework and social norm for the duration of working life. The pension reforms analysed in this paper therefore generate variation in the expected duration of the working life, ultimately allowing to study the forward-looking effects of the pension reform.

In order to isolate causal estimates I employ a difference-in-difference inspired approach, exploiting the variation in the treatment intensity between adjacent cohorts. The reform was highly debated and very salient across German media sparking a general debate about longer working horizons of younger generations. Further, it affected a large set of individuals and given the large sample size of the administrative data set used in this study is well-suited to conduct an extensive set of heterogeneity analyses. Second, to supplement the results of the 2007 pension reform I analyse the 1999 pension reform using a regression discontinuity approach. The reform amongst others implied an immediate, exogenous cohort-specific and sizeable shift in the early retirement age (ERA) for women born after 1951 from 60 to 63. Crucially, the reform was announced in 1998 and passed in 1999 when the affected women, i.e. women born in 1952 and aged 47, still had a long remaining working life - another setting well-suited to study forward-looking effects.

A range of theoretical considerations point towards plausible forward-looking effects of pension reforms. First, taking a life-cycle perspective, an increase in the retirement age can be interpreted as a negative wealth shock since, ceteris paribus, it reduces the time over which pensions are paid and accordingly individuals' social security wealth. Non-myopic individuals might respond to such a negative wealth shock by increasing their current labour supply, (see e.g. French (2005); Geyer and Welteke (2021)).[2] Second, human capital theory, starting with Becker (1962) and Ben-Porath (1967), predicts that the value of human capital investment increases with the payout period of the investment.[3] An increase in the pension age extends the payout period and thus may induce more investment into human capital. Indeed, recent literature provides empirical causal evidence that exogenous increases in the pension age positively impact human capital investment for the employed prior to reaching retirement (Gohl et al., 2020; Bauer and Eichenberger, 2017; Brunello and Comi, 2013), which may increase employability. In theory, similar results may hold for the unemployed when facing a shift in their working lives. In practice, there are different ways in which the unemployed can accumulate human capital. For example, human capital could be accumulated via the

[2]Note, however, that an increase in the working horizon from a life-cylce point of view may also reduce the incentive to work, as given the longer contribution period, individuals can collect the same level of pension benefits with fewer contributions in younger years (Geyer and Welteke, 2021).

[3]Generally, this is shown by the strong positive relationship between life expectancy and human capital investment such as school education and/or participation in vocational training documented in most countries over the past century, see e.g. Soares (2005); Cervellati and Sunde (2013).

participation in active labour market programs such as vocational training measures or via an increase in job search activity.[4] Hairault et al. (2010), for example, calibrate and simulate a model where job search is dependent on the distance to retirement. In their model search effort is costly and increases with the remaining duration of the ensuing job, implying a shorter unemployment duration for individuals with a longer working horizon. Moreover, not only the unemployed themselves but also job agency officials have an incentive to consider legal working life requirements. For example, one goal of employment agencies could be to reduce the permanent financial pressure on unemployment insurance by arranging more transitions to employment and/or offering more training opportunities in the hope that this will ultimately increase employment.[5] Last, from an employers' perspective, firms may have a stronger incentive to hire employees that have a longer remaining working life, as this potentially reduces costly job fluctuations and the cost of rehiring and retraining.

Despite these theoretical considerations, forward-looking employment effects of pension reforms are relatively under-studied in economic literature. Existing empirical research predominantly focuses on direct employment effects of pension reforms, generally showing that employment rates between the old and new retirement thresholds increase for individuals with longer statutory retirement ages (Staubli and Zweimueller, 2013; Geyer et al., 2020; Manoli and Weber, 2016; Atalay and Barrett, 2015; Inderbitzin and Zweimüller, 2016; Geyer and Welteke, 2021; Lalive and Staubli, 2015). Additionally, some studies find evidence for program substitution such as an increase in the take-up of unemployment or disability benefits after the former retirement age in order to substitute for the abolished retirement option (Staubli and Zweimueller, 2013; Atalay and Barrett, 2016).

In contrast, empirical literature focusing on identifying forward-looking effects of pension reforms on (un)employment and job search responses is scarce and the documented evidence mixed. Hairault et al. (2010), using a difference-in-differences approach to supplement their simulation model, find positive albeit insignificant employment effects of a pension reform in the 1990s in France for men with different distances to retirement. Engels et al. (2017) focus

[4]Vocational training measures have been shown to entail a short term negative effect on employment, as search intensity and job offers from job center officials decrease during the period of training (van Ours, 2004). In the medium to long term, however, there is wide spread evidence of increased employment due to training programs (Card et al., 2010).

[5]In particular, this goal may be more pronounced for individuals with a longer working horizon, as longer working requirements may imply prolonged unemployment, which in turn implies a longer financial burden on the unemployment insurance system.

on a cohort-specific reduction in the generosity of the early retirement option for women born in the cohorts 1938-1944 in Germany. They show that a stronger penalty for early retirement reduces duration of unemployment before reaching the early retirement threshold. French et al. (2022) analyse how changes in the way pension contributions are calculated impact employment outcomes before reaching retirement, documenting an increase in labour supply. Geyer and Welteke (2021) focus on individuals just before the retirement threshold and do not find any evidence for forward-looking effects stemming from an increase in the early retirement age. Gohl et al. (2020), using the same reform as Geyer and Welteke (2021) but focusing on younger individuals, find some albeit not very strong evidence for forward-looking employment effects. Frimmel (2021), using a pension reform in Austria, documents increases in unemployment training participation for men and employment probabilities for both men and women in response to a shift in the retirement age. Studying a pension reform in Italy, Carta and de Philippis (2021) analyse an increase in the full statutory retirement age from 60 for women and 65 for men to 67 for both using a difference-in-differences inspired approach. They find significant labour force participation effects for women aged 45-59. In particular, they find that younger women exit inactivity to enter unemployment whereas older women exit unemployment into employment in response to the reform. For men they find small and insignificant effects.

This paper contributes to the existing literature in various ways. First, it expands the scarce and mixed empirical literature focusing on forward-looking effects of pension reforms in response to an increase in the statutory retirement age. In particular, it studies the forward-looking employment and unemployment responses to a pension reform that increased the retirement age incrementally and by a small margin from cohort to cohort. Crucially, it confirms theoretical predictions based on human capital theory and existing empirical evidence dealing with large increases in the retirement age by providing new causal empirical evidence that pension reforms that raise the retirement age, even if only by a small margin across cohorts, do indeed increase individuals' subjective expectations about the length of their working lives and imply forward-looking employment effects. Second, this paper explicitly examines heterogeneous outcomes and, as an important new result, shows that employment effects are particularly pronounced for individuals in service sector occupations that require lower physical intensity and can therefore be performed at older ages. Third, it provides novel empirical evidence for potential behavioural mechanisms driving the documented em-

ployment effects. In particular, it provides suggestive evidence supporting the notion that the expected retirement age plays a key role in job search behaviour.

On a more general level, this paper shows how pension policies, job search and active labour market policies interact. This is crucial as understanding the interaction between these policy tools is key to promoting employment amongst middle-aged and older workers, while simultaneously lifting pressure from pay-as-you go pension systems. In particular, the heterogeneous results documented in this paper highlight that pension reforms affect individuals and occupations differently. To prevent pension reforms and the extension of working lives from exacerbating inequality in old age, policymakers can actively take into account different responses to the rising retirement age when promoting employment in old age through pension reforms or labor market policies. Last, the paper adds to the broader literature examining the relation between pension ages and employment responses in general.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 describes the data and sample selection. Section 4 presents the empirical setting of the study and Section 5 presents the main results as well as corresponding robustness checks and alternative specifications. Section 6 sheds light on possible mechanisms for the documented effects and Section 7 and 8 analyse results for further outcome variables as well as heterogeneous effects. Section 9 discusses the results and potential policy implications. Finally, Section 10 concludes.

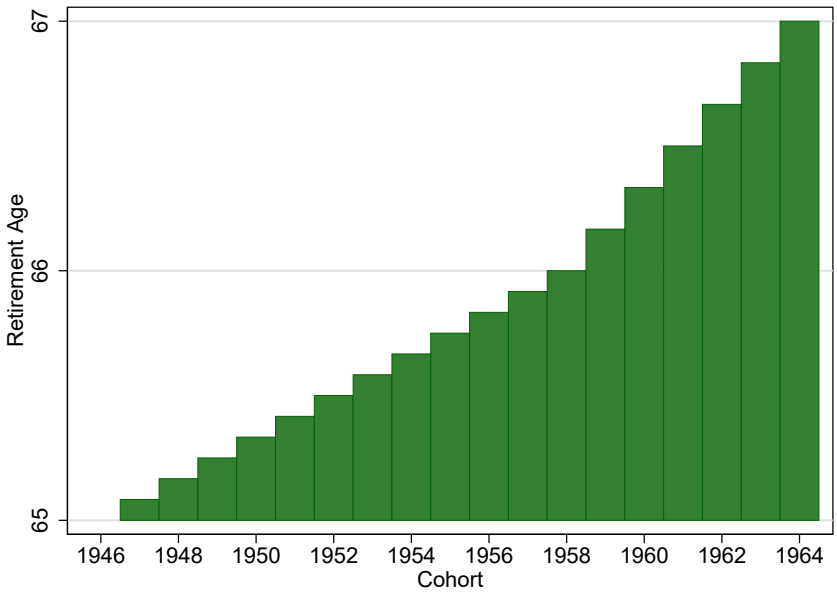
2 Institutional Setting

Before turning to the empirical analysis, this section provides a brief overview of the relevant aspects of the German pension system and the 2007 and 1999 pension reforms. Further, it provides an overview of the German unemployment insurance system and its rules, as they might shape age-specific individual employment behaviour and thus need to be taken into account.

The German Pension System The statutory public pension system is the central part of the retirement system in Germany. It covers more than 80% of the workforce with the exceptions of groups that are not subject to compulsory pension insurance, most importantly civil servants and self-employed. It includes old-age pensions, disability pensions, and

survivors benefits. The system is financed as a pay-as-you-go (PAYG) scheme and has a strong contributory link: pension benefits depend on the entire working history. The pension system provides several pathways into early retirement, i.e. claiming retirement benefits before reaching the normal retirement age with actuarially fair deductions of 0.3 percent per month.[6] Throughout the 1990s and 2000s the German public pension system was subject to a range of reforms. The paper mainly focuses on the 2007 reform and as an extension the 1999 pension reform.

Figure 1: Increase in NRA induced by 2007 Pension Reform

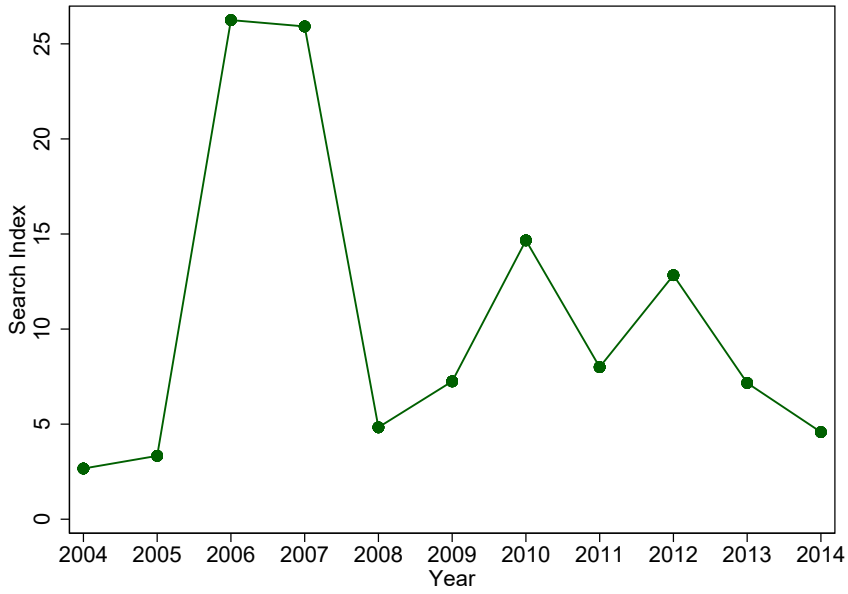


Notes: Figure 1 depicts the new normal retirement age (on the y-axis) by cohort (on the x-axis) as stipulated by the 2007 pension reform.

The 2007 Pension Reform In April 2007, Germany officially raised the statutory normal retirement age (NRA) from 65 to 67. The NRA was increased step-by-step for cohorts born between 1947 and 1964. Figure 1 illustrates the incremental increase. Starting with cohort 1947 the normal retirement age was raised by one additional month for each subsequent

[6] Actuarial fairness stipulates that the present value of lifetime pension benefits equals the present value of lifetime pension contributions. Actuarially fair deductions ensure that this holds for early retirement entries.

Figure 2: Google Search Trends



Notes: Figure 1 depicts the yearly average index value for Google search trends of "retirement with 67". The index can range from 100 to 25 and was aggregated from a monthly to a yearly summary statistic in the figure above.

cohort up until cohort 1958. For cohorts 1959-1964 the NRA was increased by two additional months for each consecutive cohort. The reform effectively implied that from 2012 onward individuals could no longer retire at age 65 without deductions. Generally, the reform was very salient and strongly discussed in public. Figure 2 plots the Google search trends for "retirement with 67", showing clear peaks in Google searches in 2006-2007 and 2010-2012, the years in which the reform was most heavily debated: discussions and media coverage of the reform started in 2006 and continued throughout 2007. In 2010, the debate concerning retirement with 67 picked up again, with the Social Democrats, the junior party in Germany's grand coalition government at the time, entertaining the idea of postponing the rise in the NRA, which eventually did not happen.[7] In 2012, the first affected cohort, i.e. cohort 1947, retired at an age higher than 65, which again gained strong media coverage and public attention, as reflected in another increase in Google searches.

[7]See for example FAZ.Net (2010)

The 1999 Pension reform To supplement the results from the main analysis using the 2007 pension reform, I additionally consider the 1999 pension reform, which officially came into force on 1st January 1999 and, amongst others, abolished the *pension for women* for cohorts born after 1951. Prior to the reform, this early retirement option allowed women to enter retirement from age 60 onward.[8] The reform thus effectively raised the early retirement age (ERA) for most women from age 60 to age 63 and therefore increased the working life. Crucially, the reform was cohort-specific. Women born before 1952 could still claim the *pension for women* with the same qualifying conditions as before the reform. The eligibility criteria were: (i) at least 15 years of pension insurance contributions; and (ii) at least 10 years of pension insurance contributions after the age of 40. According to Geyer and Welteke (2021), about 60% of all women born in 1951 were eligible for the old-age pension for women. Self-employed women and women working in the civil service by default were not eligible for the old age pension for women. Women born after 1951 were now required to have a total of 35 contribution years in order to qualify for early retirement at the age of 63.

In contrast to the 2007 pension reform, the direct effects of the 1999 pension reform have been extensively studied. For example, Geyer and Welteke (2021) and Geyer et al. (2020) evaluate the labour market effects of the 1999 pension reform, focusing on employment effects between the old and new retirement thresholds. They document that the increase in the ERA has a sizable positive effect on the working life of individuals. More precisely, employment rates for eligible women aged between 60 and 62 increase by about 15 percentage points. The combined effect on inactivity and unemployment has a similar size with about 12 percentage points.

The German Unemployment System Besides the public pension system, the German unemployment system, its benefit payment scheme, and reforms are of central interest to this paper, as they potentially influence employment behaviour and decisions. In Germany, individuals are entitled to unemployment benefits if they have contributed to unemployment

[8] Additionally, the *pension after unemployment or after old-age part-time work* was abolished for individuals born after 1951 at the same time as the *pension for women*. Unemployed individuals born before 1951 could enter early retirement at the age of 63 whereas unemployed individuals born after 1951 could no longer do so. For more details see Geyer et al. (2020). For a more general description on the German pension system, see Boersch-Supan and Wilke (2004).

Table 1: Benefit Duration (in months) by Age Group

Age	Before 02/2006	After 01/2006	After 12/2007
< 45	12	12	
45-46	18	12	
47-51	22	12	
50-54			15
52-54	26	12	
55-56	26	18	
57	32	18	
≥ 58	32	18	24

Source: Dlugosz et al. (2009), Table 1 depicts the different benefit duration rules in each period and for each age group (i.e. before and after the respective reforms). Note that in addition to individuals' age their work experience during the previous years decided eligibility to unemployment benefits.

insurance for a minimum time of twelve months during the last two years before unemployment. The contribution rate to the unemployment insurance is 2.4 percent of a person's gross earnings, and contributions are usually shared equally between employer and employee. The replacement rate for singles lies at 60 percent of the average net wage in the last 12 employed months before unemployment. The maximum duration of unemployment benefit payments is 12 months for individuals below the age of 50. For individuals above the age of 50 this period further increases to 15 months. For individuals between 55 and 57 the maximum duration of unemployment benefits is 18 months, and individuals above the age of 58 can obtain unemployment benefits for a maximum duration of 24 months.[9] Once the claim to unemployment benefits expires, individuals are entitled to social assistance.[10] The paper focuses on a period beginning in 2000, so it is critical to consider changes in benefit duration rules during the period under study. The rules described above were adopted with the Hartz reforms, which were ratified at the end of 2003 and came into force from February 2006. Further, there was a second change in rules in December 2007, marginally adjusting the 2006 rules. Prior to these reforms, the benefit scheme was more generous. For example, already from the age of 52 conditional on having worked for 48 months within the last 7 years individuals could claim unemployment benefits for a maximum duration of

[9] In theory, women who enter unemployment at the age of 58 and are eligible for early retirement at 60 thus can bridge the transition into retirement with unemployment benefits.

[10] Arbeitslosengeld II from 2005 onwards, before 2005 Arbeitslosenhilfe and Sozialhilfe.

24 months.[11] Table 1 illustrates the different benefit duration rules for each period/policy regime. The empirical strategy in this paper fully accounts for differences in claim durations across age groups and changes in benefit duration rules throughout the sample period, as described in more detail below.

3 Data and Sample

I use three different data sets. First, I use the Sample of Integrated Employment Biographies (SIAB). Second, I supplement this administrative data set with data from the SAVE Survey and third I use the IZA Evaluation Dataset Survey.

1) Sample of Integrated Employment Biographies The SIAB data set is a two percent random sample drawn from the Integrated Employment Biographies (IEB) of the German Institute for Employment Research (IAB). The IEB consist of all individuals in Germany who can be described by at least one of the following employment statuses: employment subject to social security contributions[12], marginal employment [13], unemployed and entitled to benefits or basic support, and participation in programs of active labour market policies. Based on these categories, the data documents detailed employment histories from 1975 onward for West Germany and from 1992 onward for the whole of Germany. Following Dauth and Eppelsheimer (2020), I create a yearly panel data set from 2000-2014. Crucially, as key outcome variables I observe whether an individual in a given year is employed and thus contributing to social insurance or registered as unemployed. Further, I observe marginal employment, participation in other active labour market programs or whether individuals received funding to set up their own business.

I explicitly focus on the time period starting from 2000. Before 2000, some of the variables of interest were not recorded coherently[14] and un/employment spells in some regions were incorrectly counted/registered. Further, in my main specification I focus on years up until 2014 in order to purely isolate the effect of the 2007 pension reform. In 2014, there was

[11] For a detailed description of unemployment rules prior to the Hartz reforms see Caliendo et al. (2013)

[12] This excludes civil servants and the self-employed.

[13] Employment in so called mini-jobs that are not contributing to social insurance payments.

[14] For example, participation in active labour market programs such as training for the unemployed was not recorded before 2000.

another pension reform adjusting the retirement age for an early retirement option, which also differently affected the cohorts under examination. In a robustness check I extend the period to 2017, the last year I observe in the data.

2) Saving and old-age provision in Germany (SAVE) The SAVE Survey is a repeated cross sectional survey, which took place in Germany from 2001-2013 at irregular intervals. Originally launched in 2001, it was repeated in 2003/2004 and then at regular annual intervals until 2013. Crucially for this study, the survey elicits subjective beliefs about retirement ages by asking "*What do you expect - at what age will you presumably retire or receive pension payments*". Additionally, it collects a range of socioeconomic background information such as age, gender and educational background. For a detailed description of the survey see Börsch-Supan et al. (2008).

3) IZA Evaluation Dataset Survey The IZA Evaluation Panel is survey data tracking employment history, behaviour and individual traits for an inflow sample of more than 17,000 unemployed individuals born from 1952 onward. The data was collected for unemployment entries between June 2007 and May 2008, i.e. just after the pension reform. Individuals were tracked for three years after their initial unemployment entry. Crucially, it contains information about the job search behaviour of newly unemployed individuals within the first two months of unemployment, such as the number of applications and job offers received by job agency officials. Further, as described above it tracks unemployed individuals for up to three years, allowing to analyse re-employment probabilities within three years.

Sample In order to analyse employment effects of the 2007 pension reform, I focus on a sample of individuals born in the cohorts 1959-1964 for whom the retirement age was gradually increased in two month steps for each additional cohort. Individuals in these cohorts were aged 44-55 in the post reform period, i.e. 2008-2014. The reason for exclusively focusing on these cohorts is threefold: first, the unemployment benefit duration reforms described above were implemented shortly before and after the pension reform 2007, i.e. in 2006 and 2008. Due to age-specific changes in the generosity of benefit payment lengths induced by the reforms, they might have affected cohorts differently, thereby potentially undermining identification. For example, **?**Dlugosz et al. (2014) show that younger age groups in cohorts

1959-1964 did not adjust their employment responses to the 2006 unemployment benefit duration reform, whereas older individuals in earlier cohorts did. Further, due to their relatively young age, individuals in cohorts 1959-1964 were not directly affected by the second reform in 2008, which has been shown to have decreased the job search effort of individuals in earlier cohorts who were directly affected by the reform and whose benefit duration was increased by the reform (Lichter and Schiprowski, 2021). Focusing exclusively on younger cohorts, i.e., those born between 1959 and 1964, thus allows excluding potential confounding effects created by age-specific reforms in the duration of unemployment benefits and their timing, since individuals in these cohorts did not react differently to or were affected by the reform.[15] Second, the shift in the NRA for cohorts 1959-1964 differed by two months for adjacent cohorts, thereby providing stronger variation in the treatment intensity between cohorts, which will be used in the empirical strategy of the paper. Last, by restricting the analysis to cohorts that are relatively similar in age, I ensure that I only compare individuals whose employment biographies would have evolved similarly in the absence of treatment, a key requirement for identification, as described in more detail in Section 4.

As previously described, Carta and de Philippis (2021) find that younger women select out of inactivity into unemployment and older women into employment in response to a pension reform in Italy. Unlike Carta and de Philippis (2021), this paper, in its main specification, only uses variation from individuals that were part of the SIAB dataset already before the reform, i.e. between 2000 and 2007, thereby capturing the response of individuals actively participating in the labour market. In addition, to account for possible selection into employment, I condition on individuals having at least one spell of employment or unemployment prior to 2006, the year in which the reform was first discussed. This ensures that I observe individuals that are likely to actively participate in the labour market and do not select into activity (employment or unemployment) once the reform was first discussed and then passed.

Last, in order to confirm my main results I extend the analysis of forward-looking pension reform effects to the pension reform 1999 using the SIAB data. Here I focus on a sample of

[15]The empirical strategy will also control for age group fixed effects ensuring that only individuals from age groups that receive the same benefits are compared with one another. However, these age group fixed effects do not pick up changes over time in (un)employment behaviour in response to reform and thus the sample restrictions described above are necessary.

women born in 1951 and 1952 observed between 2000-2008 aged 50-58.[16]

4 Empirical Approach

In order to analyse the 2007 pension reform, I exploit the variation in treatment intensity between cohorts 1959-1964 in an approach similar to Carta and de Philippis (2021). In particular, I implement a DiD estimator with a multi-valued treatment variable (Callaway et al., 2021) capturing the reform-induced change in the normal retirement age measured in months. Effectively, this approach compares outcome values before and after the reform among groups with different treatment doses, thereby estimating the average causal response of the outcome variables of interest to a one-month increase in the NRA. Equation 1 describes the approach in more detail.

$$y_{it} = \beta D_i * post_t + \phi_{tb} + \zeta_{i/c} + \alpha_a + X_i'\delta + \epsilon_{it} \tag{1}$$

y_{it} stands for a range of outcome variables such as retirement expectations or employment and unemployment indicators for individual i in year t. D_i can be thought of as the treatment dosage, which in this setting is a time-invariant variable denoting an individuals' change in the statutory retirement age measured in months induced by the reform. More precisely, D_i is given by $D_i = NRA_{>2007,i} - NRA_{<2007,i}$ where $NRA_{>2007,i}$ is an individuals' normal retirement age after the 2007 pension reform and $NRA_{<2007,i}$ is an individuals' normal retirement age before the reform. Table A.1 in the Appendix details the statutory retirement ages and changes induced by the reform for each cohort respectively. $post_t$ is an indicator variable that is equal to one in the post-reform period and zero otherwise. ϕ_{tb} are bi-cohort, b, times year fixed effects accounting for any time trend that similarly affects adjacent cohorts, i.e. individuals that are very similar in age and thus likely to be affected by macroeconomic trends in the same way.[17] $\zeta_{i/c}$, depending on the data set and specification used, are either individual i fixed effects, controlling for any time-invariant unobservables at the individual level or cohort c fixed effects, controlling for any time-invariant cohort-specific

[16] I explicitly exclude women above the age of 58 as these women in theory could directly enter retirement at 60 after claiming benefits for two years.

[17] Note that when using the SAVE data, I solely use year fixed effects, as the number of observations is relatively low and the inclusion of bi-cohortly year fixed effects requires a relatively large number of observations in order to ensure sufficient variation.

unobservables.[18] α_a are age group fixed effects for each age group that receives the same length of benefit entitlement in a given year (see Table 1). The inclusion of these fixed effects ensures that only individuals with the same statutory benefit entitlement length are compared to each other. Depending on the specification and the data set, I also control for a range of covariates. In particular, when the inclusion of individual fixed effects is not possible, I include individual-specific socioeconomic covariates such as educational attainment and a female indicator. Finally, ϵ_{it} is an error term which is clustered at the individual level.

Conditional on the included fixed effects and covariates the point estimate of β identifies the average causal effect of an additional one month increase in individuals' normal retirement age if the parallel trend assumption holds: in the absence of the reform the outcomes of individuals with different treatment doses, i.e. here different changes to their retirement age, would have evolved parallely over time. Additionally, recent advances in the DiD literature have shown that in order for the empirical strategy to truly identify the average causal response on the treatment group (ACTR), it also needs to hold that if groups would have received any level of the same dosage their outcomes would have evolved paralelly (Callaway et al., 2021). This somewhat stronger assumption is only likely to hold for cohorts that are very similar in unobservable and observable characteristics, i.e. adjacent cohorts that are close in age and likely to face similar career and life circumstances. The inclusion of the year times bi-cohort fixed effects, the individual fixed effects (when possible) as well as the general sample restriction to cohorts 1959-1964 ensures that the empirical strategy solely exploits variation between cohorts that are indeed close in age, controls for any time-invariant individual observables and accounts for age-specific macroeconomic trends.

Further, in order to test for common pre trends, I implement an approach similar to an event study design and run a regression with interaction terms between D_i, the multi-valued treatment variable, and each respective year from 2000-2014. This also allows to analyse the dynamic response to the reform. Equation 2 describes the approach in more detail. All variables are the same as in Equation 1 and $\sum_{t=2000}^{2014} \beta_t D_i * \phi_t$ stand for the described interaction terms of D_i and year indicators ϕ_t.

$$y_{it} = \sum_{t=2000}^{2014} \beta_t D_i * \phi_t + \phi_{tb} + \zeta_{i/c} + \alpha_a + X'_{it}\delta + \epsilon_{it} \tag{2}$$

[18] The SAVE data essentially is a repeated cross section where only relatively few individuals are surveyed repeatedly and thus the inclusion of individual fixed effects is not possible.

5 Results

The following section presents the main results and corresponding robustness checks for the pension reform 2007. First, I focus on individuals' subjective retirement beliefs and then analyse individuals' (un)employment responses to the pension reform. Last, as a supplementary extension I will present results for the 1999 pension reform.

Expectation Results Only if individuals perceive the reform can they react to it. Therefore, for the approach to truly reveal individuals' reactions to pension reform, it is first necessary to analyze whether individuals actually perceive the reform as a shift in their expected work horizon. In order to do so, I use the repeated cross-sectional data provided by the SAVE survey and individuals' expected retirement age as the key outcome of interest. For the relevant cohorts, i.e. cohorts 1959-1964, I observe approximately 3,000 survey responses. Before the reform between 2001 and 2006 individuals in the affected cohorts on average expect to retire at age 63.5. The post-reform average is substantially higher with an average expected retirement age just above 65.[19] This simple before-after comparison does not isolate the causal effect of the reform on expectations. I thus implement the DiD approach described above and cluster standard errors at the individual level, as some respondents are interviewed repeatedly in consecutive years. Crucially, the DiD approach allows to identify the causal effect of an additional one month increase in individuals' normal retirement age on their retirement expectations. Note that for the SAVE data I can only include cohort fixed effects, as the majority of individuals is not observed repeatedly. The results are depicted in Table 2. Specification (1) shows the results without covariates. The point estimate for the interaction term implies that a one month increase in the NRA induced by the 2007 pension reform, on average, raised the expected retirement age by approximately 1.2 month. The result holds in specification (2) when controlling for covariates such as educational attainment and a female indicator variable.

Specifications (3) and (4) replicate the estimation for individuals in cohorts 1964-1969, who were not differentially affected by the reform. Crucially, I re-assign D_i as if these cohorts were treated and raise their placebo retirement age by additional two months for

[19] Individuals state retirement expectations substantially below the statutory normal retirement age. One reason for these lower expectations are potential early retirement options, allowing individuals to retire before the NRA at age 63.

Table 2: Expectation Results

	(1)	(2)	(3)	(4)
Interaction	1.2292***	1.2333***	0.0005	0.0005
	(0.4363)	(0.4370)	(0.0006)	(0.0006)
Covariates	-	✓	-	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Reform	True	True	Placebo	Placebo
Cohorts	1959-64	1959-64	1964-69	1964-69
N	2,917	2,917	2,845	2,845

Note: Source: SAVE data, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table displays regression results using the expected retirement age measured in months as outcome variable for SAVE Surveys between 2001 and 2013. Before the reform the average expected retirement age was 63.5 among the cohorts under examination. Note that the first post reform year in this specification is 2007, as the survey was conducted from April-August 2007, i.e. when the reform was already in place.

each cohort. The results clearly show that cohorts that were not affected by the reform did not adjust their expected working horizon in response to the reform, while the actually affected cohorts adjust their expectations in line with the increase in the statutory retirement age.[20] Finally, I check for dynamic patterns and common pre-trends by estimating Equation 2. Figure A.1 in the Appendix shows the results. Note that due to the relatively low number of observations standard errors are relatively large and estimating such a dynamic specification is not ideal. Nonetheless, the graph allows to provide suggestive evidence on the common pre-trend assumption needed for identification. The results clearly show that before the reform there is no significant difference between cohorts' expectations. In the year of the reform, i.e. 2007, expectations are significantly higher. Similarly, there is a jump in expectations in 2011, which likely is a derivative of increased media coverage of the reform between 2010-2012 (see Section 2).

[20]The point estimate of 1.2 even suggests a slightly higher rise in expectations than the actual increase in the NRA stipulated by the reform. One potential explanation for this may be that affected individuals anticipate future increases in the NRA for their cohort.

Employment Results If individuals adjust expectations about their working horizon, this potentially translates into their (un)employment outcomes. The following section therefore analyses the forward-looking employment responses to the 2007 pension reform using the SIAB data. Table 3 presents regression results for the main specification using individual fixed effects for the two main outcomes: employed at some point in a given year and contributing to social insurance, and registered as unemployed at some point in a given year.[21] Note that the employment and unemployment states are not mutually exclusive. An individual can be both employed and unemployed in a given year. Therefore the point estimates do not need to be equal in absolute value. Further, there are other outside options such as marginal employment, which will be analysed in Section 8.

The results show a clear increase in the probability to be employed in a given year following the reform: a one month increase in the NRA raises the probability by approximately 0.18 percentage points. The point estimate is significant at the one percent level. The probability of being registered as unemployed at some point during a given year decreases by approximately 0.11 percentage points in response to the reform. The point estimate, however, is only significant at the ten percent level. Overall the reform therefore increases the probability of being employed and to a certain extent also the probability of being unemployed.

To give an idea of the relative size of the effect, the average employment rate in the pre-reform period across all cohorts was 85.5 % and the unemployment rate was 18.3 %. Taking the pre-reform mean as the baseline, this translates into an effect size of 0.21 percent in the probability to be employed and a 0.6 percent decrease in the probability to be unemployed during a given year for each additional monthly increase in the NRA.

On a more general level, it should be noted that the pension reform might have created general equilibrium effects on labour demand and/or wages, thereby potentially creating spillover effects affecting individuals' labour market outcomes.[22] It is therefore important to highlight that the point estimates presented here only capture net of general equilibrium effects, as long as these potential spillover effects do not impact individuals with a two month

[21] The inclusion of individual fixed effects ensures that only individuals observed before and after the reform are used in the estimation approach, i.e. individuals that are actively participating in the labour market. Further, they allow controlling for all time-invariant confounding factors at the individual level.

[22] For example, individuals with a longer working horizon may be more likely to find a job, thereby impacting employment chances of individuals with a lower NRA and thus their post reform outcomes.

Table 3: Main Results

	(3) Employment	(4) Unemployment
Interaction	0.00178***	-0.00114*
	(0.00064)	(0.00068)
Bi-Cohort Year FE	✓	✓
Individual FE	✓	✓
N	1,763,754	1,763,754

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Note that the first post reform year in this specification is 2008, as the reform was passed during the year 2007 and employment responses might take time to manifest.

difference in their statutory retirement age differently.

In a next step, I focus on dynamic reform responses by estimating Equation 2. Figures 3-4 depict the corresponding point estimates and their 95 % confidence intervals of the respective year-change in retirement age interaction.[23] For the employment outcome, there clearly is no difference between cohorts before the introduction of the reform, supporting the necessary common pre-trend assumption. Directly after the reform the point estimates are positive albeit not significantly different from zero. From 2011 on point estimates rise and become significant. One obvious explanation for this dynamic pattern is that between 2010-2012 there was a strong discussion about the reform, as in 2012 the first cohort affected by the reform (cohort 1947) retired at an age older than 65 years, which was strongly covered by the media and discussed by the general public (see Figure 2). For the unemployment outcome, there again is no significant difference between cohorts before the reform. Note, however, that the point estimate for the year 2004 is relatively large and significant at the ten percent level. After the reform, point estimates become smaller and in most cases negative, however, individually remain insignificant at any conventional level. This is unsurprising as the point estimate in the pooled specification shown in Table 3 is only significant at the ten percent level.

[23] As a baseline, I here choose the year 2007, as 2008 is the first full year under the new pension rules and employment/unemployment responses might take time.

Figure 3: Outcome: Employment

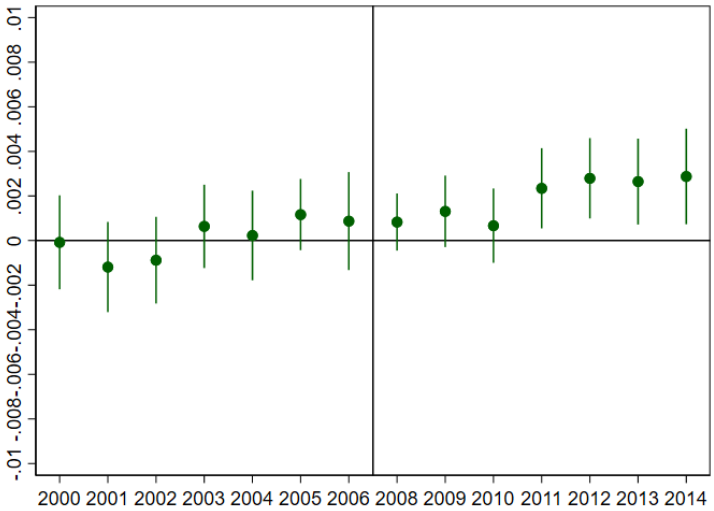
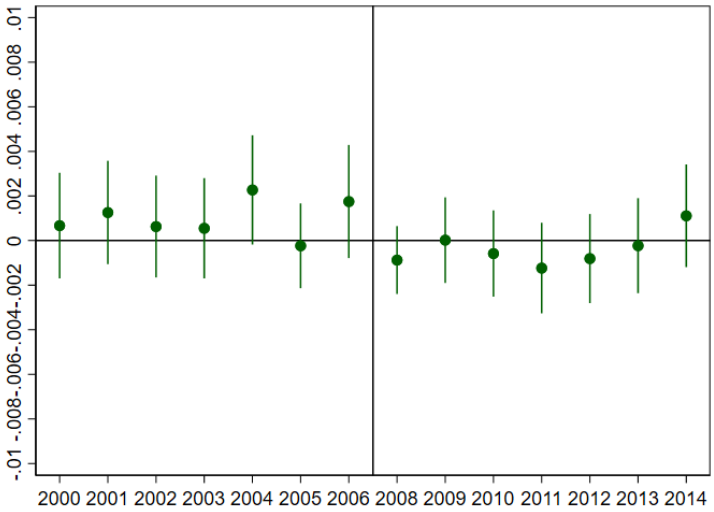


Figure 4: Outcome: Unemployment



Note: Figures 3 and 4 depict point estimates and corresponding 95% confidence intervals obtained from the dynamic specification described in Equation 2 for the outcome variables employment and unemployment throughout a given year controlling for individual fixed effects and bi-cohort times year fixed effects. The y-axis depicts the coefficient value and the x-axis the corresponding year. Note that the first post reform year in this specification is 2008, as unlike for the expectation data, observations in 2007 can stem from the pre-treatment period.

Robustness A crucial assumption for the main approach to yield causal estimates is the common pre-trend assumption, which Figures 3 and 4 support. Crucially, there is no significant difference between cohorts before the reform period. In order to further test that the main approach is not simply picking up differential age patterns between cohorts, I implement a placebo test. As for the expectations outcome, I repeat the estimation for cohorts 1964-1969, which were not differentially affected by the reform and re-assign D_i as if these cohorts were treated, raising their placebo retirement age by an additional two month for each consecutive cohort. Table A.2 in the Appendix clearly shows that there is no significant effect of this placebo treatment on any of the two outcomes, thereby supporting the notion that the main specification identifies causal reform effects.

Another concern might be that the unemployment benefit duration reform in 2006, described above, due to its age specific nature has differentially impacted the cohorts analysed in the main specification. For example, cohorts that were aged below 45 did not experience a reduction in their benefit entitlement duration throughout the years 2006 and 2007, whereas individuals aged 46 or 47 experienced a substantial reduction by more than 6 and 10 months respectively. As discussed above, **?**Dlugosz et al. (2014) show that exclusively older individuals, i.e. individuals from 50 onward and therefore not the cohorts under examination in this paper, reacted to shorter benefit durations by selecting into unemployment just before the reform was passed and by being less likely to become unemployed directly after the reform was in place. The cohorts under examination in this paper thus in practice have not differentially reacted to the reform, as they were aged 42-47 in 2006, the year of the reform. On paper, however, cohorts 1959, 1960, 1961 and 1962 were also directly and differentially affected by the reform. In the main specification, I address this by including age group fixed effects, which ensure that only individuals who are in the same entitlement length age group in a given year are compared to one another. In an additional robustness check, I now exclusively focus on the cohorts 1963 and 1964. Individuals in these cohorts were aged 42 and 43 in 2006 when the benefit reform was passed and thus not affected by the reform. Since I only observe two cohorts for this additional estimation, I implement a classic DiD design and replace the multi-valued treatment with a dichotomous indicator if individuals were born in cohort 1964. The point estimate thus measures the impact of a two month longer statutory working horizon. Table A.3 shows that the inference remains the same when only focusing on these two cohorts: individuals with a longer working horizon

are more likely to be employed in a given year. For the unemployment outcome, the effect is negative albeit insignificant. Figures A.2 and A.3 depict the results for the dynamic specification and clearly show that there is no evidence for differential pre-trends. Further, as in the main specification the positive employment effect is driven by point estimates from 2011 onward.

Next, I adjust the reform date. In the main specification using the SIAB data I specified 2008 as the first year of the post-reform period since the pension reform was passed during the year 2007. In a alternative specification, I treat the year 2007 as the first year in the post reform period. Table A.4 shows the results. Inference across outcomes remains the same. The employment effect is smaller in absolute size. This is intuitive as the dynamic specification clearly shows that for the employment outcomes, the point estimates only start to increase from 2011 onward.

Further, in the main specification I restrict my estimation to the years 2000-2014. In June 2014 there was another reform, which essentially reduced the early retirement age for the *particularly long-term insured*. Conditional on having contributed 45 years, individuals born before 1953 were now able to enter early retirement at age 63. For cohorts 1954-1964, this early retirement age, however, was increased step by step for each cohort, in line with the increase induced by the 2007 pension reform. Consequently, individuals in cohort 1964 that have accumulated 45 years of pension contribution could therefore only use this early retirement option at age 65. In summary, the 2014 pension reform therefore is likely to have reinforced the effects of the 2007 pension reform. Table A.5 in the Appendix repeats the main estimation this time including the years 2000-2017. As expected, the inference remains the same with slightly more pronounced employment effects.

Extension: The 1999 Pension Reform In order to supplement my results for the 2007 pension reform, I use the 1999 pension reform as a second source of quasi-random variation. Gohl et al. (2020), also using the 1999 pension reform, mainly focus on forward-looking human capital effects but also provide some albeit not very strong evidence on forward-looking employment effects for a sample of older women starting in 2005 using German Microcensus data (see Chapter **??**). The 1999 pension reform effectively increased the ERA for women born after 1951 by three years from age 60 to age 63. I therefore exclusively focus on women in the cohorts 1951 and 1952 and employ a regression discontinuity design

focusing on observations from 2000 to 2008, i.e. on individuals aged 48 to 58.[24] Equation 3 details the approach.

$$y_{it} = \alpha_0 + \gamma_1 D_i + \gamma_2(B_i - c) + \gamma_3 D_i(B_i - c) + X_i'\delta + \alpha_a + \phi_t + \eta_f + \varepsilon_{it} \tag{3}$$

y_{it}, depending on the specification, stands for the same outcome variables as before. D_i is a dummy specifying treatment, that is equal to one if a woman is born after 1.1.1952 and 0 otherwise. Formally, D_i is given by:

$$D_i = \begin{cases} 1, & if\ B_i \geq c \\ 0, & if\ B_i < c \end{cases} \tag{4}$$

where B_i is a woman's month of birth and c is the cut-off date for the increase in early retirement age, ERA (January 1, 1952). In the main specification, one cohort is included on each side of the cut-off. More precisely, I include the cohort 1951 below the cut-off and the cohort 1952 above the cut-off. The difference between a woman's birth month and the cut-off, $B_i - c$, gives the running variable. The running variable is interacted with the treatment variable D_i to allow for different slopes before and after the cut-off. Depending on the specification and to account for potential different functional forms, I include a linear or quadratic specification of the running variable and the interaction term. Further, I account for year fixed effects, ϕ_t, age group fixed effects, α_a, federal state fixed effects, η_f and observable characteristics, X_{it}. In particular, I include educational attainment and the number of days in employment at the beginning of the observation period in 2000 as a measure of total work experience. Last, ε_{it} is the error term. Following Geyer and Welteke (2021), I cluster standard errors at the birth month level to account for correlation between observations for the same individual or individuals born in the same month.

The above approach isolates the local treatment effect via the estimate of γ_1 conditional on the assumption that observed and unobserved characteristics around the cut-off are not systematically different between the treated and the control group. In other words, individuals close to the cut-off would have behaved identically in the absence of treatment conditional on the included fixed effects and covariates. In Appendix B, I provide supporting evidence

[24] As described previously, data before 2000 is prone to recording errors. Women after the age of 58 are explicitly excluded, as on paper they can bridge the time into retirement by claiming unemployment benefits, which potentially may lead to mechanical and not behavioural employment responses.

Table 4: RDD Results

	(1) Employment	(2) Unemployment
Panel A: Linear Running Variable		
Treatment	0.0188***	-0.0089*
	(0.0067)	(0.0046)
Panel B: Quadratic Running Variable		
Treatment	0.0264***	-0.0167***
	(0.0072)	(0.0044)
Covariates	Yes	Yes
Year FE	Yes	Yes
Federal State FE	Yes	Yes
N	136,199	136,199

Note: Source: SIAB data, own calculations. Standard errors in parentheses are clustered at birth month level. Results obtained from local regressions. Panel A shows the results for a local linear and Panel B for local quadratic specification; significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

for this assumption based on balancing tests of covariates as well as by moving the cut-off to a hypothetical placebo date. Table B.2 confirms that pre-determined observable covariates such as educational attainement do not jump around the cut-off. Additionally, Table B.1 shows that cohorts 1952 and 1953, which were not differentially affected by the reform, do not show differences in (un)employment outcomes, supporting the notion that, in the absence of the reform, adjacent cohorts behave similarly. Moreover, there were no other relevant policy reforms between 2000 and early 2007 that affected women in the 1951 and 1952 cohorts differently. As described above, the 2007 pension reform shifted the NRA for the 1952 cohort by an additional month compared with the 1951 cohort, so for the last two years the estimate also captures this additional reform effect.

Table 4 shows the results for the RDD approach using local linear and quadratic regressions and Figures B.1 and B.2 in Appendix B show the typical RDD graphs plotting the running variable on the x-axis and the corresponding averages of outcome variables on the y-axis, fitting local regressions using a triangular kernel. For the employment outcome, depending on the specification, point estimates range from just under 1.9 percentage points to more than 2.6 percentage points and are significant at the one percent level across specifications. For the unemployment outcome, I find negative effects ranging from approximately 0.9 percentage points to 1.7 percentage points, which are significant at the ten and five percent level respectively. Overall, the results are in line with the findings for the 2007 pension reform and support the notion that pension reforms imply positive forward-looking employment effects. Unsurprisingly, effect sizes are larger than for the 2007 reform, as this reform implied a larger shift of the retirement age by, i.e., by three years.

In summary, both the 2007 and the 1999 pension reform indeed increased the employment probability for individuals with a longer expected working horizon. For the remainder of the paper, I focus on the 2007 pension reform, as additional data needed for further analyses such as the IZA Evaluation Panel is only available for the cohorts affected by this reform. Further, the larger number of observations when using the SIAB data for this reform allows the implementation of an extensive set of heterogeneity analyses. The following sections will first focus on the mechanisms driving the documented results, to then analyse potential reform effects on other outcome variables as well as heterogeneous results.

6 Mechanisms

The documented employment effects of the 2007 reform in theory can be driven by a range of combinations of inflows and outflows from one state to another. For example, individuals with a longer expected working horizon might be more likely to remain employed, and thus might be less likely to enter unemployment. Similarly, unemployed individuals with a higher NRA might be more likely to find re-employment.

In order to approximate inflow and outflow patterns from one state to another, I estimate year-to-year transitions using the main DiD specification. More precisely, I repeat the same estimations as before but condition on an individuals' observed labour market state in year t and consider outcome variables that are indicators equal to one if in the following year, i.e. $t+1$, the observed individual is in one of three possible outcomes states.[25] In addition to the employment and unemployment outcomes used above, I include a third outcome category that captures all individuals that are neither employed nor unemployed throughout a year but are observed in the data set. For example, these are individuals that participate in active labour market programs, such as unemployment training. For each outcome state, I thus condition on being employed, unemployed or in the third category, i.e. *Other*, at time t. Table 5 therefore mimicks a transition matrix for the reform effect where the rows show the original state and the columns the state in the next period. Naturally, these transition rates solely serve a descriptive purpose, as the states I condition on are outcomes by themselves. However, they provide suggestive evidence on the drivers of the documented employment and unemployment effects.[26]

The results in the first row of Table 5 show that employed individuals with a higher increase in the NRA are more likely to remain in employment in the following year. The point estimate is significant at the ten percent level. Further, the results indicate that employed individuals with a stronger increase in their NRA are also less likely to be unemployed or in the *Other* category in the following year. Note, however, that both point estimates are insignificant at any conventional level. The results in the middle row point towards a

[25] Again, employment and unemployment are not mutually exclusive, as throughout a year individuals can be both employed and unemployed. Solely focusing on individuals who are either employed or unemployed in a year would imply that a large part in the variation in employment/unemployment flows would not be captured by the estimation, as individuals who enter either of the states do so throughout a given year.

[26] Also note that the sum of point estimates does not need to be equal to zero, as the observed states are not mutually exclusive, i.e. individuals can be employed as well as unemployed in a given year.

Table 5: Transition Results

Rows: State t / Columns: State t+1	(1) Employment	(2) Unemployment	(3) Other
Employment	0.00059*	-0.00025	-0.00022
	(0.00016)	(0.00063)	(0.00017)
Unemployment	0.00467*	-0.00201	-0.00018
	(0.00247)	(0.00170)	(0.00083)
Other	0.00498*	0.00215	-0.00660**
	(0.00302)	(0.00168)	(0.00332)

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Observations in first row: 1,393,145; second row: 272,087; third row: 86,453. The table depicts the point estimates from regressions using bi-cohort year and individual fixed effects and conditioning on employment, unemployment or all remaining possible states observed in time period t (rows) using the observed states in period t+1 as outcomes (columns). Point estimates do not need to cancel out along rows, as the observed states are not mutually exclusive, i.e. individuals can be employed and/or unemployed and/or in the *Other* category in a given year.

significantly higher probability (10 % level) to find re-employment for unemployed individuals with a longer working horizon. Further, unemployed individuals with a higher NRA are less likely to remain in unemployment and to enter active labour market programs captured by the *Other* category. The point estimates for these latter two outcomes, however, are not significantly different from zero. Last, individuals with a longer NRA in other recorded categories such as active labour market programs are less likely to be in the same category in the following year (significant at the 5% level) and more likely to be employed in the following year with a positive point estimate significant at the ten percent level. The point estimate for the unemployment outcome is positive albeit not significantly different from zero.

In summary, Table 5 provides suggestive evidence that employed individuals with a reform-induced longer working horizon are more likely to remain in employment. Further, unemployed individuals and individuals in active labour market or other programs are more likely to find re-employment. One possible explanation for the observed higher probability of staying in the labour force can be found in the empirical literature documenting positive effects of raising the retirement age on investment in on-the-job training, which could have an impact on employability and job security, see e.g. (Gohl et al., 2020; Brunello and Comi, 2013). Similarly, a greater inflow into employment from unemployment or active labor market programs may be explained by firms' possible preference to hire individuals with longer legal employment horizons and/or by an adjustment in individual behaviour. Hairault et al. (2010), for example, suggest an increase in the intensity of job search by the unemployed in response to a longer remaining working life as a key mechanism.

In order to analyse whether there is an adjustment in individual behaviour and thus a change in the labour supply response, I use the IZA Evaluation data set, which focuses on unemployment inflows in a time frame immediately after the reform, i.e. from June 2007-May 2008 (see Section 2). Crucially, the data set includes information about job search behaviour at the beginning of the observed unemployment spell and also allows to observe whether individuals found re-employment within three years of first entering unemployment. In order to measure job search intensity, I focus on the number of applications and job offers received within the first two months of unemployment. More precisely, I construct dichotomous variables that are equal to one if an individual send out or received more than the average number of job applications or offers.

In total, I observe more than 2,500 individuals in the cohorts 1959-1964 who entered unemployment. The data does not include observations from the pre-reform period and the DiD inspired research design used so far is no longer feasible. I therefore implement a design similar to Frimmel (2021). Note that it can not account for possible selection into unemployment, which according to the main results above does exist to a certain extent. The results are therefore of suggestive nature, aiming to show whether individuals who become unemployed and have a longer statutory working horizon, behave differently. The approach is fully detailed by Equation 5.

$$y_i = \beta_0 + \beta_1 D_i + \zeta_{bc} + \alpha_a + X'_{it} + \epsilon_{it} \tag{5}$$

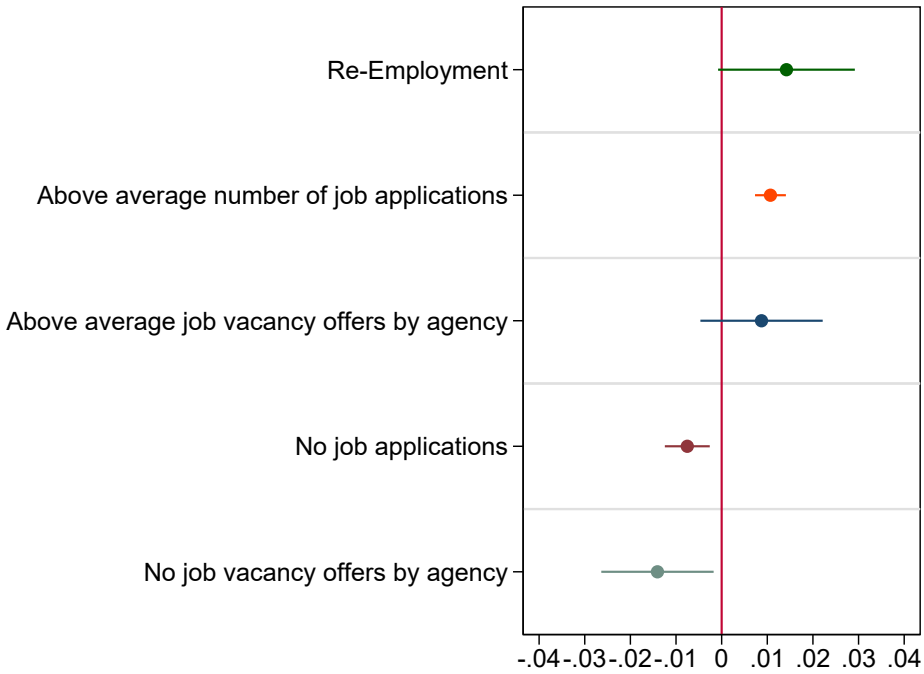
The approach uses the variation in the statutory retirement age between cohorts. y_{it} is the outcome variable of interest (see above). β_0 is an intercept term and D_i is, as in the main approach, the change in the retirement age induced by the reform. ζ_{bc} are bi-cohort fixed effects, i.e. indicator variables equal to one for cohorts 1959 and 1960, then for cohorts 1961 and 1962 and so on. α_a are age group effects accounting for differences in the entitlement length to unemployment benefits. Both the bi-cohort fixed effects and the age group fixed effects ensure that first I only compare individuals who are one year apart in age and second individuals who share the same rule framework in terms of unemployment benefits. X'_i is a vector controlling for individual characteristics such as gender, educational attainment, the net income received from the last known employment, an indicator whether the individual receives unemployment benefits, a year indicator for the year 2008 and regional control variables such as the local unemployment and vacancy rates. Last, ϵ_{it} is an error term clustered at the cohort level.

Figure 5 depicts the point estimates and their corresponding 95% intervals for the coefficient of the change in the retirement variable, thus showing how different outcome variables react to a one month increase in the retirement age. First, the results show a higher probability to be re-employed within three years after individuals first entered the sample. The point estimate is significant at the ten percent level. Second, individuals with a longer working horizon are more likely to have sent out more than 14 applications during the first two months of unemployment, i.e. the average number of sent out applications, and less likely to have sent out no job application at all. Similarly, they are more likely to have received an above average number of job offers by the job agency, i.e. more than one job offer. Note,

however, that the corresponding point estimate is not significant at any conventional level. Further, they are less likely to have received no offer at all.

All in all, then, these suggestive results do indeed point to an increase in search intensity as advocated by Hairault et al. (2010). Moreover, the results indicate that individuals with a longer working horizon are actively considered by job agencies and their officials. The next sections will continue exploring possible mechanisms and additional responses to the reform by analysing further outcomes and providing heterogeneous results for the main specification.

Figure 5: Job Search Outcomes, IZA Evaluation Survey



Note: Figure 5 depicts the point estimates and their corresponding 95 % confidence intervals obtained from regressions of the respective outcome variable displayed on the left hand side of the Figure on a variable measuring the change in the statutory retirement age induced by the 2007 reform for an unemployment inflow sample from June 2007-May 2008 of 2,554 individuals born in the cohorts 1959-1964. All regressions control for the covariates such as gender, educational attainment, the net income received from the last known employment, an indicator whether the individual receives unemployment benefits, a year indicator for the year 2008 and regional control variables such as the local unemployment and vacancy rates as well as bi-cohort and age group fixed effects.

7 Further Outcomes and Job Sorting

In this section, I analyse additional outcome variables that could be affected by the shift in the work horizon. First, I focus on outcomes related to employment or unemployment responses such as employment adjustments on the intensive margin or yearly earnings. Second, I analyse whether individuals sort into particular occupations in response to the reform.

Additional Outcomes Table 6 depicts the results for a range of different outcome variables using the preferred DiD specification on the SIAB data. There are a range of other outcome variables that may also be impacted by the reform. For example, the paper so far has considered employment responses at the extensive margin. However, individuals might also adjust their behaviour on the intensive margin by working more hours. First, in column (1) I focus on a part time indicator that is equal to one if a person is employed in part time and zero otherwise. The point estimate is small, positive and insignificant, indicating that there is no overall effect on part-time work. Unfortunately, the SIAB data does not provide information on the actual number of hours worked. Therefore a more precise analysis of responses along the intensive margin focusing on hours worked is not possible. Second, I use marginal employment as an outcome variable. Crucially, marginal employment does not count towards pension insurance. Individuals with a longer working horizon might thus be less likely to be be marginally employed and be more likely to switch to a job contributing to pension insurance contributions. This indeed seems to be the case, as the corresponding point estimate is small, negative and significant at the ten percent level.

In column (3), I look at individuals' yearly labour earnings. In particular, the documented employment effects of the reform may also translate into different yearly earnings. Indeed, I find a positive effect of more than 137 Euros per year, which is significant at the one percent level. This effect corresponds to a 0.55 percent relative increase in yearly earnings using average pre-reform earnings as the baseline and is likely partly driven by the documented employment effects. Further, Gohl et al. (2020) and Gohl et al. (2023) show that individuals with a longer working horizon invest more in human capital accumulation and are more likely to be promoted, which may additionally translate into increases in earnings.[27]

[27] When conditioning on employment and repeating the estimation the inference remains the same: yearly earnings are higher for individuals with a longer working horizon. However, the size of the point estimate decreases to approximately 120.

Next, in columns (4)-(6) I examine outcomes related to unemployment. First, I use participation in an *unemployment training program* as an outcome variable. The data used in this study allows to identify participation in *measures of vocational training*. These measures are by law required to be linked to a specific vocational goal. For example, this goal can consist of computer and software courses to maintain and update existing vocational skills, but can also consist of retraining for a new profession and/or training for an additional vocational profession or exam. The results show that individuals with a longer working horizon are less likely to participate in these training measures, which is likely driven by lower inflows into unemployment of those individuals. The same holds for days spend in unemployment per year, which shows a negative point estimate that is significant at the ten percent level.[28] Further, the data allows to observe whether individuals obtain a subsidy for setting up their own business and thus enter self-employment. The point estimate is very close to zero and insignificant. This is not a perfect measure of self-employment, as it just records subsidized self-employment, however, might serve as a suggestive proxy.[29]

Last, exits into inactivity may play a role. Since inactivity is not directly observed in the data, I focus on an inactivity proxy. More precisely, I use an indicator variable that is equal to one if an individual was previously observed in the data but no longer is in the current year. This proxy measure thus does not only include individuals entering inactivity but also other outcome states not captured by the SIAB data, such as civil service or unsubsidized self-employment. The results show that there is a decrease in the probability to enter inactivity, which is significant at the one percent level. This is in line with the main results, showing that a longer legal work horizon increases the probability to be employed while reducing the probability to be in unemployment as well a inactivity.

Sorting into Occupations Another possible response to the reform may be to sort into jobs that can be exercised at older ages and thus enable individuals to remain in employment for a longer period. Jobs with a lower level of psycho-social or physical stress are likely

[28]For any of the outcomes related to unemployment, I can repeat estimations conditioning on unemployment. Point estimates for the training outcome and days spend in unemployment remain negative, however, are statistically not different from zero.

[29]When conditioning on unemployment the point estimate for the self-employment outcome remains negative insignificant.

Table 6: Other Outcomes

	(1) Part time	(2) Marginal Emp.	(3) Yearly Earnings	(4) Unemp. Training	(5) Days in Unem.	(6) Self-Emp	(7) Inactivity
Interaction	0.00030	-0.00098*	137.44374***	-0.00071**	-0.32385*	-0.00002	-0.00144***
	(0.00081)	(0.00051)	(24.12624)	(0.00033)	(0.18925)	(0.00002)	(0.00033)
Bi-Cohort Year FE	✓	✓	✓	✓	✓	✓	✓
Individ. FE	✓	✓	✓	✓	✓	✓	✓
N	1,763,754	1,763,754	1,763,754	1,763,754	1,763,754	1,763,754	1,763,754

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table repeats the main specification including individual and bi-cohort year fixed effects using a range of different outcomes depicted in the first row of the table.

to fulfill this criterion. Differences in physical intensity and stress levels associated with a job indeed strongly affect the likelihood of being employed at older ages (Zwick et al., 2022; Vermeer et al., 2016), as well as actual subjective expectations of being able to work until the official retirement age, and could therefore also play a role in the response to a postponement of the statutory retirement age. To illustrate this, in Figure A.4 in the Appendix I additionally provide descriptive evidence obtained from a survey commissioned by the DGB (Deutsche Gewerkschaftsbund), one of Germany's largest trade union umbrella organisation.[30] Crucially, the survey elicits subjective beliefs about individuals' capacity to work until the statutory retirement age. Individuals were asked whether they expect to work until the statutory retirement age without any restrictions given the requirements of their current job. On average 47 % percent of all individuals answered with yes. Figure A.4 depicts answers to the same question stratified by different physical and psycho-social stress indicators. The results show that for jobs with a high level of physically demanding tasks the percentage of people answering with yes (23 %) is substantially below the overall average rate of 47 %.

In order to explicitly analyse whether job types and the corresponding stress exposure of a given job play a role in the response to the pension reform, I use the *classification of occupations 2010* (*Klassifikation der Berufe 2010*) provided for each observation in the SIAB data.[31] As a first step, using the 2010 occupational classifications allows to differentiate

[30]The survey includes 31,164 workers (members and non-members in the union) across all sectors and regions in Germany between 2012-2016. The summary statistics are taken from Gewerkschaftsbund (2016) as direct access to the data is not possible

[31]For a detailed description of the classification procedure see Paulus and Matthes (2013). The classifi-

between nine so called occupational areas. Table A.6 in the Appendix lists these areas, ranging from occupation areas in the agricultural and manufacturing sector to occupations within the service sector. In order to asses whether individuals sort into specific occupation areas in response to the reform, I regress indicator variables, which are equal to one if an individual newly entered an occupation area, on the main DiD specification. Table A.7 in the Appendix clearly shows that there are no sorting patterns into any of the observed occupation areas.[32]

As a second step, in order to more explicitly analyse the role of stress exposure both physical and psycho-social, I use exposure indices developed by Kroll (2011) and recently used by Zwick et al. (2022); Mazzonna and Peracchi (2017).Kroll (2011) defines exposure as "conditions with potential physiological and/or psychological effects on the human organism resulting from the characteristics of the activity itself or from its external conditions." Crucially, the indices are available for 138 occupation groups following the *classification of occupations 2010* and therefore allow to study sorting patterns at a more granular level than the nine occupation areas studied above.[33] In total, Kroll (2011) provides four exposure indices with values ranging from zero to ten: (1) a composite overall index of exposure to environmental, temporal, carcinogenic, ergonomic, physical and psycho-social stress indicators and (2) separate indices for physical or psycho-social exposure as well as a heavy work index.[34] For each year I merge the respective index value to each observation using

cation system is based on employers reporting a certain job key when registering the employee. Before the switch to the current system in 2011, there was a different registration key. For employees registered before 2011 the old key is transformed into the new classification system.

[32]Note that for the agricultural occupation area the point estimate of the interaction term is negative and significant at the ten percent level. For all other occupation outcomes point estimates are small and insignificant.

[33]The original indices are calculated at the five and four digit level of the classification system. Since in the SIAB data I only observed classifications based on three digits, I take average values of the index values for 138 three digit occupation groups. The indices are based on responses from a large-scale representative survey with approximately 20,000 participants conducted by the German Federal Institute for Vocational Education and Training (BIBB). The original paper uses the German BIBB/ BAuA-workforce survey 2006 and was later updated for the German BIBB/ BAuA-workforce survey 2011. The index for different job classifications can be retrieved Gesis Archiv.

[34]For example, variables used to construct the physical index ask for the frequency of fulfilling tasks standing or sitting and how often individuals need to lift heavy. For the psycho-social index individuals are, amongst others, asked about time pressure and the general work atmosphere. In total, the indices are constructed using 39 survey items.

an individual's current jobs' three digit occupation classification.[35] I then construct three different outcome variables for each indicator: (1) the immediate index value ranging from 0-10, (2) a dichotomous outcome variable that is equal to one for an above average index value and zero otherwise, and (3) a dichotomous outcome variable that is equal to one if the given index and hence an individual's exposure decreased in comparison to the previous year. Figure A.5 in the Appendix depicts the results. For example, graph [c] shows the results for the last outcome variable, i.e. the indicator variable for a decrease in the index. There is no evidence for sorting into jobs with a lower stress exposure, as point estimates remain insignificant across outcomes and index type. This holds when changing the definition of the outcome variable using the immediate index value as outcome or an indicator variable that is equal to one for an above average index value. For each specification, point estimates remain insignificant.

Overall, there is no evidence for occupational sorting patterns in response to the reform, implying that individuals do not retrain and switch into different occupational areas that may be easier to exercise at an older age. This potentially is explained by the age of the observed individuals who are 44 and older in the post-reform period and thus likely to be relatively settled in their career path and occupational area.

8 Heterogenous Results

In this section, I explore whether the effects of the 2007 pension reform vary along a range of dimensions to test for heterogeneous employment and unemployment responses to the reform. In particular, after studying heterogeneous responses along sex, region and education, I revisit the role of occupation types and exposure, testing for different reform effects across occupation areas and jobs' stress exposure. Similar to the argument made above, jobs in certain occupational areas may be more suitable to exercise at an older age, and thus individuals employed in or with experience in certain professions may be more likely to adjust their employment behaviour in response to pension reforms.

Heterogeneous Results: By Sex First, I split the sample into men and women. Table 7 shows the results. Specifications (1)-(3) exclusively focus on women and (4)-(6) on men.

[35] In the case of individuals who were not employed in a given year, I use their last observed place of work.

Overall, the results show that the (un)employment effects are strongly driven by women. This finding is in line with recent results by Carta and de Philippis (2021). One potential explanation for the difference between men and women may be the widely accepted notion that labour supply is less elastic for men. In particular, men tend to be more attached to the labour market and therefore may have less room to increase their labour supply in response to the reform (Carta and de Philippis, 2021).

Table 7: Heterogeneity: Women vs. Men

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment
Interaction	0.00333***	-0.00190*	0.00049	-0.00051
	(0.00107)	(0.00101)	(0.00076)	(0.00091)
Bi-Cohort Year FE	✓	✓	✓	✓
Individ. FE	✓	✓	✓	✓
Group	Women	Women	Men	Men
N	834,666	834,666	929,088	929,088

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) show the point estimates obtained from estimations including bi-cohort year fixed effects and individual fixed effects for women. Columns (3)-(4) show the results for men.

Heterogeneous Results: East vs. West Another possible dimension for heterogeneity are different regional responses. In particular, there may be differences between former East and West Germany. For example, labour market attachment of women in former East Germany tends to be higher, as historically East Germany had very high female labour force participation rates (Bonin, 2005). Table A.8 in the Appendix thus splits the sample along this regional dimension.[36] The results show that the estimated point estimate for the employment outcome is slightly larger and remains significant for a regression focusing solely on West Germany. For East Germany, the point estimate is slightly smaller and insignificant, which might be due to the substantially lower number of observations and thus a lack of

[36]Crucially, there is not sorting along this regional dimension. A regression of an east indicator variable on the main DiD specification renders small point estimates that are not significantly different from zeo.

statistical power for East Germany. Alternatively, higher labour market attachment among women in East Germany could explain the slightly lower effects documented for the East. However, note that for the unemployment outcome both point estimates are negative and insignificant with the estimate for East Germany larger in absolute size.[37]

Heterogeneous Results: By Education I then look at heterogeneous results by education and split the sample into individuals with no vocational training, vocational training and a college degree. Table A.9 in the Appendix shows the results. Point estimate for the employment outcome are relatively large and significant for individuals with a college degree. For individuals with some form of vocational training, they are less pronounced and only significant at the ten percent level. Point estimates for unemployment outcome are insignificant for both groups. For individuals without any form of vocational qualification, the effects are insignificant, albeit in case of the employment outcome positive and sizeable. Overall, there is no clear heterogeneous pattern across education groups.

Heterogeneous Results: By Occupation Area In a next step, I analyse heterogeneity across occupation areas. Crucially, while the above analysis of job sorting mechanisms (see Section 7) has shown that there is no evidence for individuals selecting into specific occupation areas or jobs that can be exercised at an older age, there may be differential reform effects between individuals whose last recorded job was in a specific occupational areas. For example, the effects may be larger for individuals with experience working in occupational areas associated with the service sector, since service-oriented jobs may be easier to exercise at an older age.

As described above, the 2010 occupational classifications allow to differentiate between nine so called occupational areas.[38] In order to study heterogeneous responses to the reform, I split the sample along these groups and thus focus solely on individuals whose last recorded job was within a given occupation area.[39] Table A.10 to A.11 in the Appendix depict the

[37] I also test for potential regional sorting patterns using an East German indicator variable as the outcome. There is no evidence for significant regional sorting patterns, thus the results are not displayed here but can be included upon request.

[38] Table A.6 in the Appendix lists these areas.

[39] Crucially, as described above Table A.7 in the Appendix shows that there are no sorting patterns into the occupation areas, which allows to study heterogeneous reform responses for each occupation area by splitting the sample along these categories.

results for each respective outcome variable, i.e. employment and unemployment. Point estimates for the employment outcome are negative and insignificant for the first two areas, i.e. the agricultural and production/manufacturing sector. For the areas *Construction, architecture, surveying and building technology* (3), *Science, geography and computer science* (4) and *Traffic, logistics, protection and security* (5) the point estimates are relatively small and insignificant. For all occupation areas in column (6)-(8) point estimates are positive and either significant or not far off from significance. These occupational areas are traditionally associated with parts of the service sector and include *(6) Commercial services, goods trading, sales, hotel and tourism*, *(7) Business organization, accounting, law and administration* and *(8) Health, social issues, teaching and education*. For category (9) *Linguistics, literature, humanities, social and economic sciences, media, art, culture and design* the point estimate again is small and insignificant. For the unemployment outcome (see Table A.11) the pattern is similar: for occupational areas (6)-(8) point estimates are negative and in case of the latter two significant. Overall, the documented employment effects are thus strongest for subgroups of occupations in the service sector.

The differences in occupational areas could be due to a number of explanations. In particular, individuals' and employers' perceptions of their ability to work in a particular occupation at a particular age are likely to play an important role. Jobs in certain occupational areas or with certain skill requirements might be better suited for employment at older ages, which in turn may influence the employment response to pension reform. For example, if individuals in certain occupations do not expect to work until the statutory retirement age anyway, they may not have an incentive to respond to pension reform. This may be the case in particular for jobs with a high degree of physical and psycho-social stress (see Figure A.4), which is analyzed in more detail below.

Physical and Psycho-Social Stress Exposure In order to analyse to what extent differences in stress exposure play a role in the documented forward-looking employment effects of pension reforms, I again use the stress exposure indices provided in Kroll (2011) and split the sample into individuals whose last recorded occupation has an index value above or below/equal to the mean occupation index value, thereby measuring whether the documented reform responses are more pronounced in jobs with a low stress intensity.

The results are depicted in Table 8. Columns (1)-(2) present the results for individuals in

a job with an above mean exposure and columns (3)-(4) with an exposure equal or below the mean. Panel A focuses on the overall exposure of a given job, measured by the *Overall Job Index (OJI)*, and shows that (un)employment responses to the reform are more pronounced for individuals whose last recorded job had a relatively low exposure. Panel B and Panel C specifically focus on the overall physical exposure (*OPI*) and the heavy work exposure *(HWI)* of a job. As for the general index, the results are driven by jobs with a below mean index value, i.e. jobs with a low physical intensity. Last, Panel C splits the sample along a *Psycho-Social Exposure Index (OJI)*. As for the other indices the effects are more pronounced and significant in columns (3)-(4), i.e. for individuals in jobs with a below mean value. However, point estimates in both groups are relatively similar in size. In summary, particularly physical stress in a specific occupation influences individuals' responses to the reform.

Last, in order to analyse whether these heterogeneous results again are exclusively driven by women, I also stratify the sample along sex as well as above and below/equal index values for each respective index. Table A.12 depicts the results for women and Table A.13 for men. For women the reform effects remain more pronounced for occupations with a relatively low stress physical as well as psycho-social stress exposure. For men (see Table A.13) there seems to be a small positive pension reform effect on employment for jobs with a below/equal average overall exposure and a below/equal average physical intensity, as the point estimate for this group increases to approximately 0.12 percentage points in Panel A and Panel B and is significant at the ten percent level.

Taken together, the results suggest that irrespective of sex, stress exposure plays a key role in determining to which extent individuals can react to a rise in their statutory retirement age. For women this seems to apply to both physical and psycho-social stress exposure. In contrast, mens' employment response to the reform are small and only positive and significant in jobs with a below/equal average physical stress exposure.

Table 8: Results by Exposure Indices

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment
Panel A: Overall Exposure Index (OJI)				
Interaction	0.00114	0.00040	0.00215***	-0.00142**
	(0.00084)	(0.00094)	(0.00066)	(0.00069)
N	850,829	850,829	711,316	711,316
Panel B: Physical Exposure Index (OPI)				
Interaction	0.00116	0.00046	0.00232***	-0.00158**
	(0.00081)	(0.00090)	(0.00068)	(0.00071)
N	897,608	897,608	664,749	664,749
Panel C: Heavy Work Index (HWI)				
Interaction	0.00127	0.00020	0.00184***	-0.00124*
	(0.00084)	(0.00095)	(0.00065)	(0.00068)
N	838,080	838,080	723,592	723,592
Panel D: Psycho-Social Exposure Index (OSI)				
Interaction	0.00146	0.00094	0.00163***	-0.00117*
	(0.00093)	(0.00098)	(0.00060)	(0.00070)
N	729,020	729,020	830,521	830,521
Bi-Cohort Year FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Group	Above Mean	Above Mean	Below/Equal Mean	Below/Equal Mean
Observed Years	2000-2014	2000-2014	2000-2014	2000-2014

Note: Source: SIAB data, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts regression results for the two main outcome variables employment and unemployment throughout a given year for individuals in occupations with an above or below/equal mean value of the respective stress exposure index denoted in each panel.

9 Discussion and Interpretation of the Results

All in all, the documented results of both pension reforms show that raising the normal and early retirement ages have positive forward-looking employment effects for individuals before they retire. They thus support the notion that increasing the retirement age provides a valid option to relieve pay-as-you go pension systems by reducing the number of unemployment benefit recipients and increasing the number of tax payers even before reaching the retirement age, therefore ultimately reducing the overall fiscal burden. However, the above heterogeneity analyses show that these results are not uniform but different across occupation areas, types and sex. Crucially, they are strongest for individuals in jobs that have a relatively low level of exposure to physical and psycho-social stress and in the service sector. These findings are in line with previous research highlighting differences in old-age unemployment and retirement behaviour. Blekesaune and Solem (2005), for example, show that workers in physically-demanding jobs are more likely to retire early and may also replace early retirement with unemployment or inactivity bridging the time until retirement (Chirikos and Nestel, 1991), which in turn may lead to financial losses. In addition, the findings of this paper suggest that there are differences along the same lines when analysing forward-looking responses to pension reforms, which may exacerbate inequalities amongst older individuals approaching retirement.

On a more general level, this paper has important implications for the policy debate on pension reforms and old-age employment. The findings show how pension policies, job search and active labour market policies are interrelated. A holistic view of these aspects is key to promoting employment amongst middle-aged and older workers, while simultaneously lifting pressure from pay-as-you go pension systems. In particular, the results documented in this paper highlight that pension reforms affect individuals and occupations before they reach official old and new retirement thresholds. Further, individuals' employment outcomes adjust differently to increases in the retirement age - a result policymakers may consider when promoting old age employment through pension reforms or labour market policies.

10 Conclusion

This paper provides novel causal evidence for the existence of forward-looking effects of pension reforms on employment outcomes, thereby confirming key predictions of human capital theory and job search models. Specifically, the paper, using two pension reforms from Germany, shows that a longer statutory working horizon increases the expected retirement age, the probability to be employed in a given year and decreases the probability to be unemployed in a given year. It shows that the documented higher employment probability is driven by an increase in employment inflows as well as a decrease in employment outflows. Further, it provides suggestive evidence that the effects are partially driven by a change in individuals' job search behaviour: unemployed individuals with a longer working horizon on average send out more job applications during the first months of unemployment. Last, as a key novel finding the paper shows that the documented employment effects are driven by individuals in non-physical jobs in the service sector. Further, it corroborates existing research that forward-looking employment responses to an exogenous increase in the retirement age are predominantly driven by women.

The findings have important implications for the academic debate on forward-looking and direct effects of pension reforms. First, they confirm recent empirical findings and theoretical considerations that pension reforms indeed imply forward-looking effects. The main pension reform studied in this paper further shows that even relatively small differences in the increase in the statutory retirement age are perceived by individuals and lead to different (un)employment behaviour. Second, the paper provides important insights on the mechanisms behind these forward-looking effects and shows that there are heterogeneities across occupational fields and the stress exposure of jobs. Crucially, the paper advances physical intensity of jobs as an explanation for the observed heterogeneous patterns, as physically demanding jobs may be harder to exercise at older ages.

A limitation of the paper is that the suggestive results documenting changes in job search intensity cannot be interpreted causally. Future research should focus on further investigating these behavioural adjustments among the unemployed in causal empirical settings. Moreover, due to the timing of the 2007 pension reform, this study can only examine forward-looking reform effects for individuals aged 44 and older. Analysing possible forward-looking effects of pension reforms for individuals under the age of forty could be an interesting starting point

for future research. In particular, different mechanisms and heterogeneities might be at play for younger individuals than documented in this study. For example, the effects of reforms might differ between future-oriented sectors and dying sectors, and sorting into these specific sectors might matter.

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Appendices

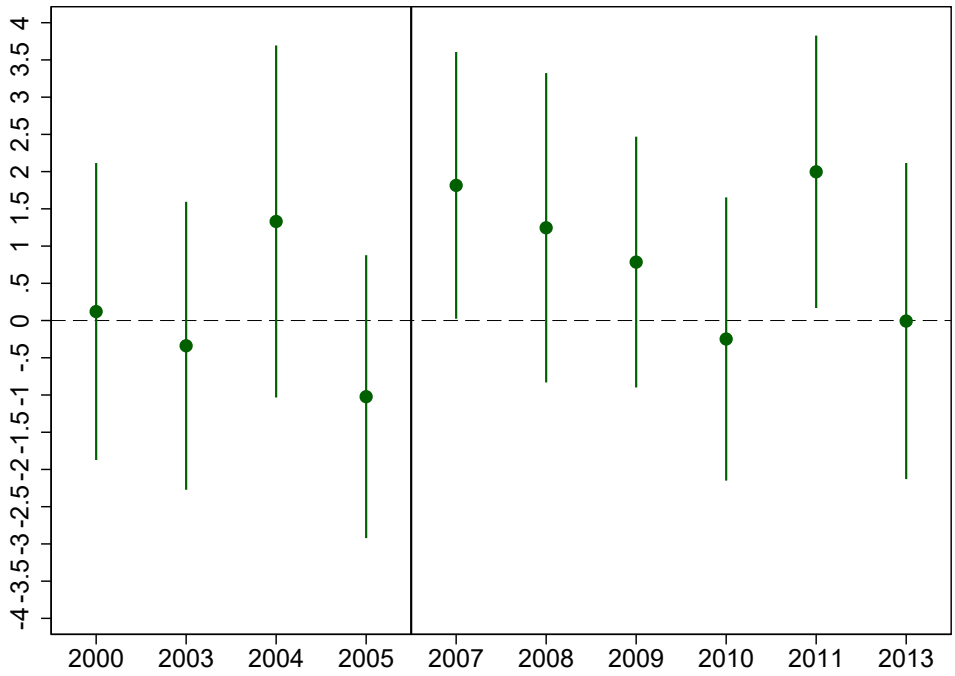
Appendix A: Additional Tables and Figures

Table A.1: Changes in Normal Retirement Age by Cohort denoted in Months

Cohort	$NRA_{<2007,i}$	$NRA_{>2007,i}$	$NRA_{>2007,i} - NRA_{<2007,i}$
1959	780	794	14
1960	780	796	16
1961	780	798	18
1962	780	800	20
1963	780	802	22
1964	780	804	24

Source: The table depicts the the normal retirement age for a given cohort before ($NRA_{<2007,i}$) and after ($NRA_{>2007,i}$) the reform as well as the change denoted in months.

Figure A.1: Dynamic Specification, Outcome Variable: Expected Retirement Age.



Note: The graph depicts the point estimates and corresponding 95% confidence intervals of year variables interacted with the change in the retirement age variable of regressions including the full set of covariates controlling for education and gender as well as year and cohort fixed effects and an individuals' retirement age expectations as outcome variable. Note that the first post reform year in this specification is 2007, as the survey was conducted from April- August 2007, i.e. when the reform was already in place. The year 2006 thus serves as a baseline.

Table A.2: Placebo Results

	(1) Employment	(2) Unemployment
Interaction	-0.00002	0.00114
	(0.00113)	(0.00114)
Year FE	✓	✓
Individual FE	✓	✓
N	1,507,836	1,507,836

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts placebo regressions for the cohorts 1964-1969 for the main specification using bi-cohort times year fixed effects and individual year fixed effects.

Table A.3: Cohorts 1963-64 Results

(1)	(2) Employment	Unemployment
Interaction	0.0050***	-0.0008
	(0.0021)	(0.0022)
Year FE	✓	✓
Individual FE	✓	✓
N	622,357	622,357

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts regressions for the cohorts 1963-1964 using an interaction between a dichotomous treatment variable and a post-reform indicator, and including individual and year fixed effects.

Figure A.2: Outcome: Employment

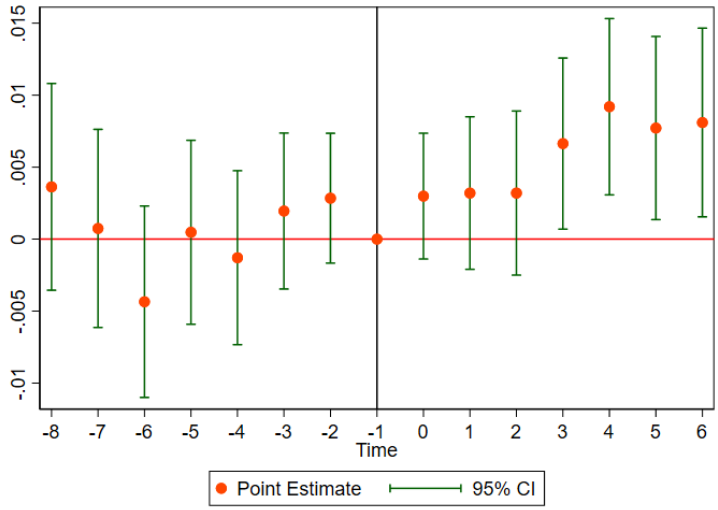
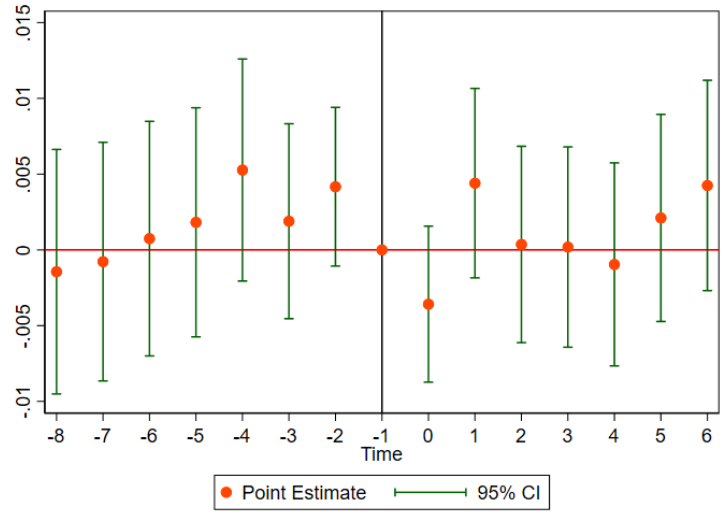


Figure A.3: Outcome: Unemployment



Note: Figures A.2 and A.3 depict point estimates of interactions of a binary treatment and year indicator as well as corresponding 95% confidence intervals obtained from an event study specification for cohorts 1963-64 for the outcome variables employement and unemployment. The x-axis denotes the distance to treatment. The year 2007 serves as a baseline, which is denoted with the value -1 in the graph, i.e. the period before treatment fully happened in 2008 here denoted with the value 0. The y-axis denotes the value of the point estimates.

Table A.4: Results First Post-Reform Year 2007

	(1) Employment	(2) Unemployment
Interaction	0.00150***	-0.00119*
	(0.00068)	(0.00071)
Year FE	✓	✓
Individual FE	✓	✓
N	1,763,754	1,763,754

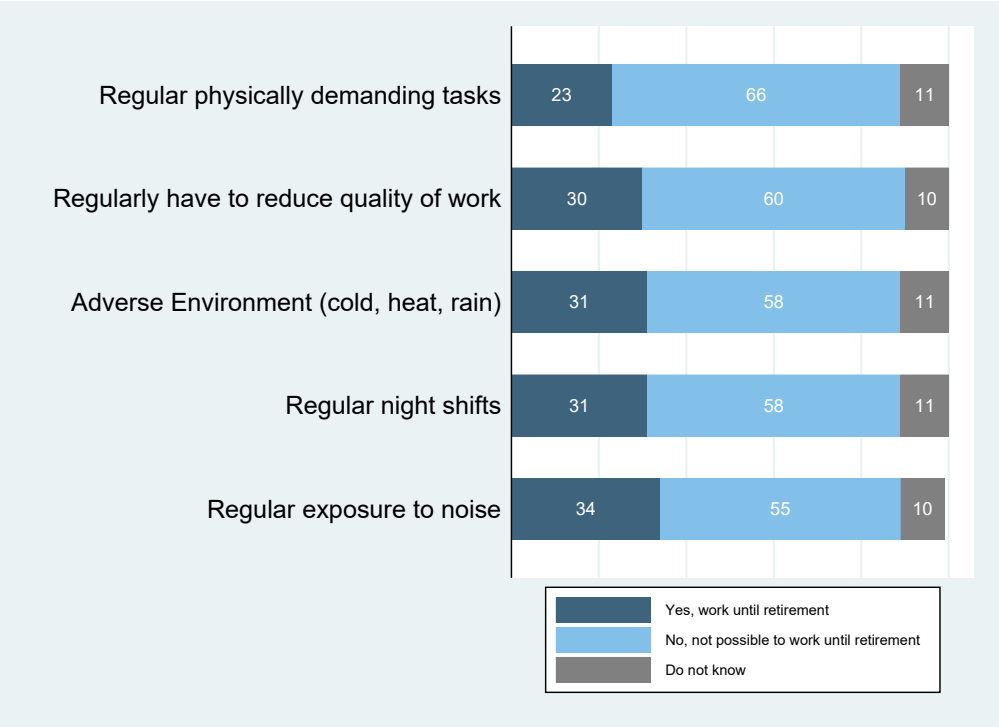
Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Note that the first post reform year in this specification is 2007. The table depicts results for the main specification using cohorts 1959-1964, bi-cohort times year fixed effects and individual year fixed effects.

Table A.5: 2000-2017 Results

	(1) Employment	(2) Unemployment
Interaction	0.00268***	-0.00123*
	(0.00065)	(0.00067)
Year FE	✓	✓
Individual FE	✓	✓
N	2,094,079	2,094,079

Note: Source: SIAB data 2000-2017, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts results for the main specification using cohorts 1959-1964, bi-cohort times year fixed effects and individual year fixed effects.

Figure A.4: Working until Retirement by Physical and Mental Stress Indicators



Note: Figure A.4 depicts average values of answers to the question *Do you think you will be able to work until statutory retirement without any restrictions given the requirements of their current job are maintained?* by individuals' job exposure to physical and psycho-social stress indicators, contained in a survey commissioned by the DGB. The DGB is one of Germany's largest trade union umbrella organisation and survey responses were collected from members and non-members across all sectors and regions in Germany between 2012-2016 of 31,164 participants. Figure is based on Gewerkschaftsbund (2016). Access to the raw data is not possible.

Table A.6: Occupation Areas

Category	Occupation Area
1	Agriculture, forestry, animal husbandry and horticulture
2	Raw material extraction, production and manufacturing
3	Construction, architecture, surveying and building technology
4	Science, geography and computer science
5	Traffic, logistics, protection and security
6	Commercial services, goods trading, sales, hotel and tourism
7	Business organization, accounting, law and administration
8	Health, social issues, teaching and education
9	Linguistics, literature, humanities, social and economic sciences, media, art, culture and design

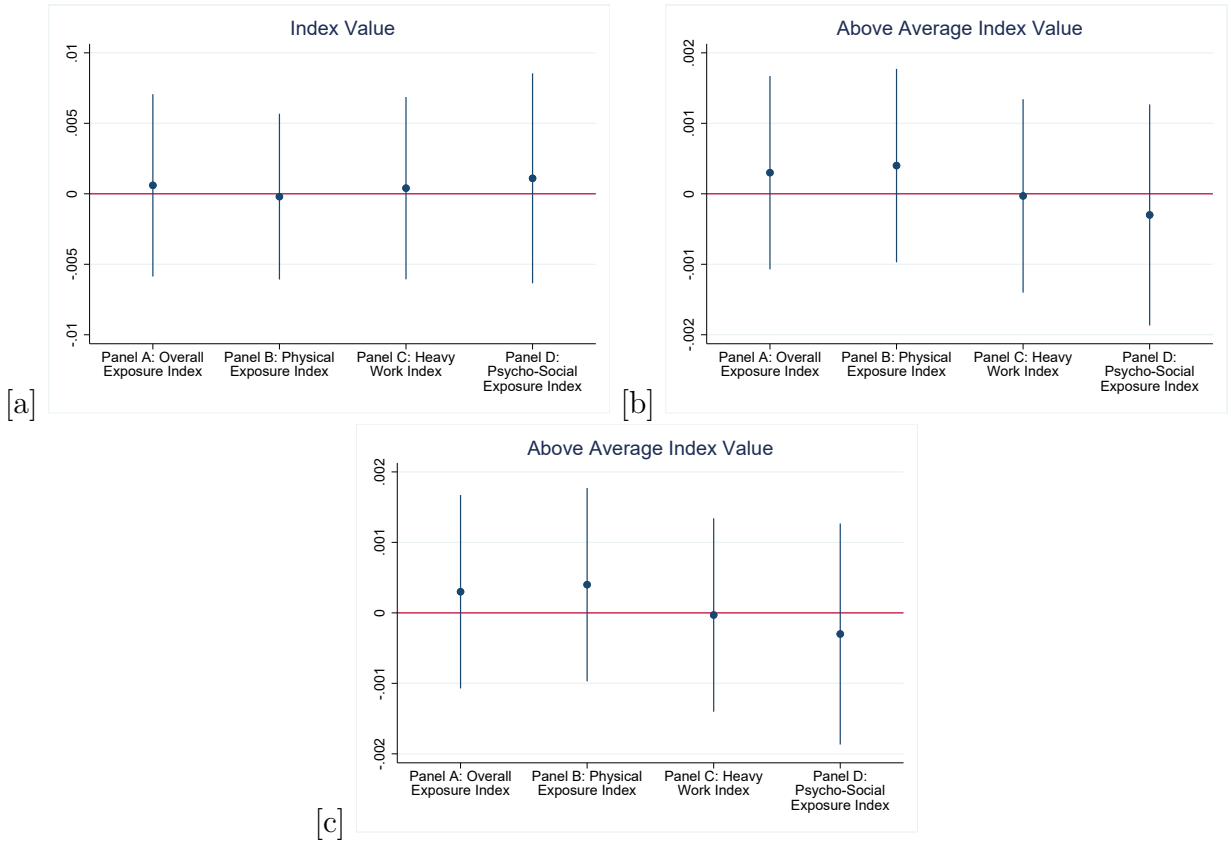
Note: Table A.6 depicts the different occupational areas in Germany's 2010 occupation classification system.

Table A.7: New Entry into Occupational Area

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Inter	-0.00012*	0.0002	0.000002	0.00008	0.00008	0.0002	-0.0002	-0.00007	0.00006
	(0.00006)	(0.0002)	(0.0001)	(0.00008)	(0.00019)	(0.00015)	(0.00018)	(0.00014)	(0.00007)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	162,708	24,349	367,616	93,099	52,529	245,959	160,160	350,445	227,210

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts results for regressions using the main DiD specification for dichotomous variables equal to one for a recorded new entry into the occupation area of interest. Column numbers correspond to occupational area: 1 *Agriculture, forestry, animal husbandry and horticulture*, 2 *Raw material extraction, production and manufacturing*, 3 *Construction, architecture, surveying and building technology*, 4 *Science, geography and computer science*, 5 *Traffic, logistics, protection and security*, 6 *Commercial services, goods trading, sales, hotel and tourism*, 7 *Business organization, accounting, law and administration* 8 *Health, social issues, teaching and education* , 9 *Linguistics, literature, humanities, social and economic sciences, media, art, culture and design*

Figure A.5: Sorting and Job Exposure



Note: Source: SIAB data 2000-2014, own calculations. Figure A.5 depicts point estimates and 95 % confidence intervals for regressions using the main DiD specification and different outcomes measuring job exposure. Outcome variables are the respective [a] exposures' index value, [b] an indicator variable that is equal to one if the index lies above the average index value of all observed occupations and [c] an indicator variable that is equal to one if the index decreased in comparison to the previous year. .

Table A.8: Heterogeneity: East vs. West

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment
Interaction	0.0018**	-0.0010	0.00015	-0.0017
	(0.0007)	(0.0007)	(0.0014)	(0.0017)
Bi-Cohort Year FE	✓	✓	✓	✓
Individ. FE	✓	✓	✓	✓
Group	West	West	East	East
N	1,357,361	1,357,361	398,479	398,479

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) show the point estimates obtained from estimations including bi-cohort year fixed effects and individual fixed effects for West Germany. Columns (3)-(4) show the results for east Germany.

Table A.9: Heterogeneity: By Education

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment	Employment	Unemployment
Interaction	0.00346	-0.00092	0.00143*	-0.00062	0.00288**	-0.00216
	(0.00290)	(0.00288)	(0.00074)	(0.00078)	(0.00129)	(0.00147)
Bi-Cohort Year FE	✓	✓	✓	✓	✓	✓
Individ. FE	✓	✓	✓	✓	✓	✓
Group	No Vocational Training	No Vocational Training	Vocational Training	Vocational Training	College	College
N	129,955	129,955	1,321,796	1,321,796	235,893	235,893

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) show the point estimates obtained from estimations including bi-cohort year fixed effects and individual fixed effects for individuals with no vocational training. Columns (3)-(4) show the results for individuals with vocational training and columns (5)-(6) for individuals with a university degree.

Table A.10: Employment Outcome by Occupational Area

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interaction	-0.00655	0.00027	-0.00021	0.00081	0.00109	0.00338	0.00281***	0.00252*	0.0002
	(0.00418)	(0.00066)	(0.00171)	(0.00129)	(0.00168)	(0.00206)	(0.00099)	(0.00130)	(0.041)
Bi-cohort Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	24,349	367,616	93,099	52,529	245,959	160,160	350,445	227,210	23,893

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column numbers correspond to occupational area: 1 *Agriculture, forestry, animal husbandry and horticulture*, 2 *Raw material extraction, production and manufacturing*, 3 *Construction, architecture, surveying and building technology*, 4 *Science, geography and computer science*, 5 *Traffic, logistics, protection and security*, 6 *Commercial services, goods trading, sales, hotel and tourism*, 7 *Business organization, accounting, law and administration* 8 *Health, social issues, teaching and education* , 9 *Linguistics, literature, humanities, social and economic sciences, media, art, culture and design*

Table A.11: Unemployment Outcome by Occupational Area

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interaction	0.00237	0.00037	0.00237	0.00030	0.00197	-0.00284	-0.00194**	-0.00280**	-0.0016
	(0.00630)	(0.00103)	(0.00324)	(0.00209)	(0.00187)	(0.00212)	(0.00096)	(0.00125)	(0.0044)
Bi-cohort Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	24,349	367,616	93,099	52,529	245,959	160,160	350,445	227,210	23,893

Note: Source: SIAB data 2000-2014, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column numbers correspond to occupational area: 1 *Agriculture, forestry, animal husbandry and horticulture*, 2 *Raw material extraction, production and manufacturing*, 3 *Construction, architecture, surveying and building technology*, 4 *Science, geography and computer science*, 5 *Traffic, logistics, protection and security*, 6 *Commercial services, goods trading, sales, hotel and tourism*, 7 *Business organization, accounting, law and administration* 8 *Health, social issues, teaching and education* , 9 *Linguistics, literature, humanities, social and economic sciences, media, art, culture and design*

Table A.12: Results by Exposure Indices for Women

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment
Panel A: Overall Exposure Index (OJI)				
Interaction	0.00293*	0.00185	0.00291***	-0.00239**
	(0.00166)	(0.00149)	(0.00105)	(0.00098)
N	351,628	351,628	393,894	393,894
Panel B: Physical Exposure Index (OPI)				
Interaction	0.00268*	0.00196	0.00321***	-0.00260***
	(0.00161)	(0.00145)	(0.00107)	(0.00099)
N	368,970	368,970	376,586	376,586
Panel C: Heavy Work Index (HWI)				
Interaction	0.00288*	0.00071	0.00274**	-0.00227**
	(0.00153)	(0.00142)	(0.00113)	(0.00101)
N	378,806	378,806	366,834	366,834
Panel D: Psycho-Social Exposure Index (OSI)				
Interaction	0.00246	0.00200	0.00310***	-0.00218**
	(0.00162)	(0.00146)	(0.00107)	(0.00100)
N	355,245	355,245	389,602	389,602
Bi-Cohort Year FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Group	Above Mean	Above Mean	Below/Equal Mean	Below/Equal Mean
Observed Years	2000-2014	2000-2014	2000-2014	2000-2014

Note: Source: SIAB data, own calculations. Standard errors in parentheses and clustered at individual
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts regression results for the two main outcome variables employment and unemployment throughout a given year for women in occupations with an above or below/equal mean value of the respective stress exposure index denoted in each panel. The mean value is derived from the whole sample and thus the same value as in Table 8.

Table A.13: Results by Exposure Indices for Men

	(1) Employment	(2) Unemployment	(3) Employment	(4) Unemployment
Panel A: Overall Exposure Index (OJI)				
Interaction	-0.00000	-0.00053	0.00124*	-0.00031
	(0.00083)	(0.00120)	(0.00066)	(0.00095)
N	499,201	499,201	317,422	317,422
Panel B: Physical Exposure Index (OPI)				
Interaction	0.00020	-0.00052	0.00120*	-0.00034
	(0.00079)	(0.00115)	(0.00070)	(0.00100)
N	528,638	528,638	288,163	288,163
Panel C: Heavy Work Index (HWI)				
Interaction	0.00001	-0.00019	0.00096	-0.00021
	(0.00088)	(0.00128)	(0.00062)	(0.00090)
N	459,274	459,274	356,758	356,758
Panel D: Psycho-Social Exposure Index (OSI)				
Interaction	0.00066	-0.00004	0.00032	-0.00027
	(0.00094)	(0.00131)	(0.00062)	(0.00099)
N	373,775	373,775	440,919	440,919
Bi-Cohort Year FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Group	Above Mean	Above Mean	Below/Equal Mean	Below/Equal Mean
Observed Years	2000-2014	2000-2014	2000-2014	2000-2014

Note: Source: SIAB data, own calculations. Standard errors in parentheses and clustered at individual level, significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table depicts regression results for the two main outcome variables employment and unemployment throughout a given year for women in occupations with an above or below/equal mean value of the respective stress exposure index denoted in each panel. The mean value is derived from the whole sample and thus the same value as in Table 8.

Appendix B: RDD Pension Reform 1999

This section of the appendix is dedicated to the additional provision of RDD results. FiguresB.1 and B.2 show the typical RDD graphs for each outcome variable. Table B.1 shows the results of a placebo test for cohorts 1952-53 between 2000-2006 when they were not differently affected by any reform. The results clearly do not show any significant effect on any of the analyzed outcome variables except for the quadratic specification and the unemployment outcome, which shows a significant point estimate at the ten percent level, albeit positive and thus in the opposite direction of the documented reform effects.

Table B.1: RDD Placebo Results

	(1) Employment	(2) Unemployment
	Panel A: Linear Running Variable	
Treatment	0.0091	0.0031
	(0.0159)	(0.0047)
	Panel B: Quadratic Running Variable	
Treatment	-0.0056	0.0107*
	(0.0210)	(0.0073)
Covariates	✓	✓
Year FE	✓	✓
Individual FE	✓	✓
N	108,830	108,830

Note: Source: SIAB data, own calculations. Standard errors in parentheses and clustered at birth month level. Results obtained from local regressions for cohorts 1952-1953. Panel A shows the results for a local linear and Panel B for local quadratic specification; significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A key assumption underlying the RD design is that covariates vary smoothly across the cut-off. I provide support for this assumption by using individual control variables as outcomes. More precisely, I regress an indicator variable whether individuals have a German high school degree (Abitur) and a variable measuring work experience at the beginning of the sample period (measured in days) on the linear and quadratic RDD specification. Table

B.2 shows that there is no significant effect on these covariates, supporting the notion that in the absence of the policy individuals close to the cut-off are similar and would have behaved similarly.

Table B.2: RDD Balancing Test

	(1) Abitur	(2) Work Experience
	Panel A: Linear Running Variable	
Treatment	0.0013	-91.71
	(0.0063)	(80.12)
	Panel B: Quadratic Running Variable	
Treatment	0.0013	-90.9
	(0.0075)	(102.73)
Year FE	✓	✓
Individual FE	✓	✓
N	136,199	136,199

Note: Source: SIAB data, own calculations. Standard errors in parentheses and clustered at birth month level. Results obtained from local regressions. Panel A shows the results for a local linear and Panel B for local quadratic specification; significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.1: Outcome: Employment

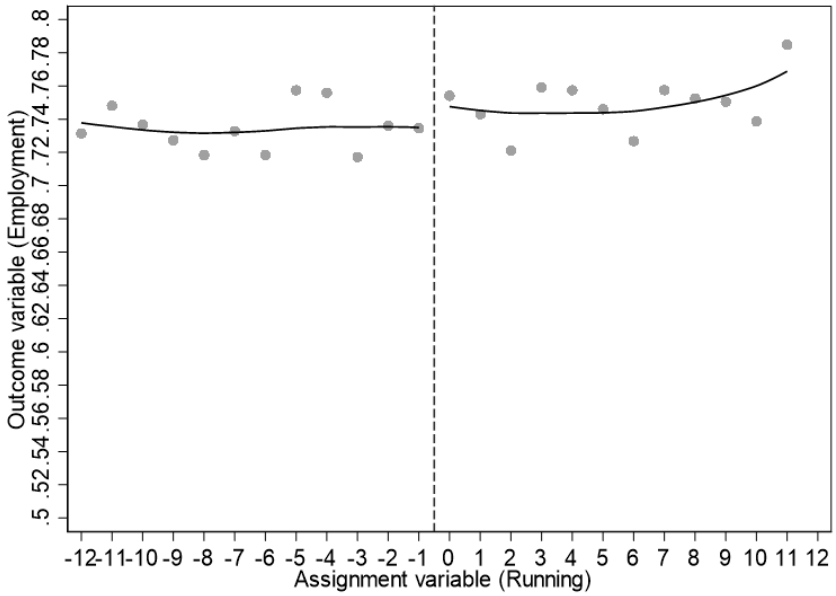
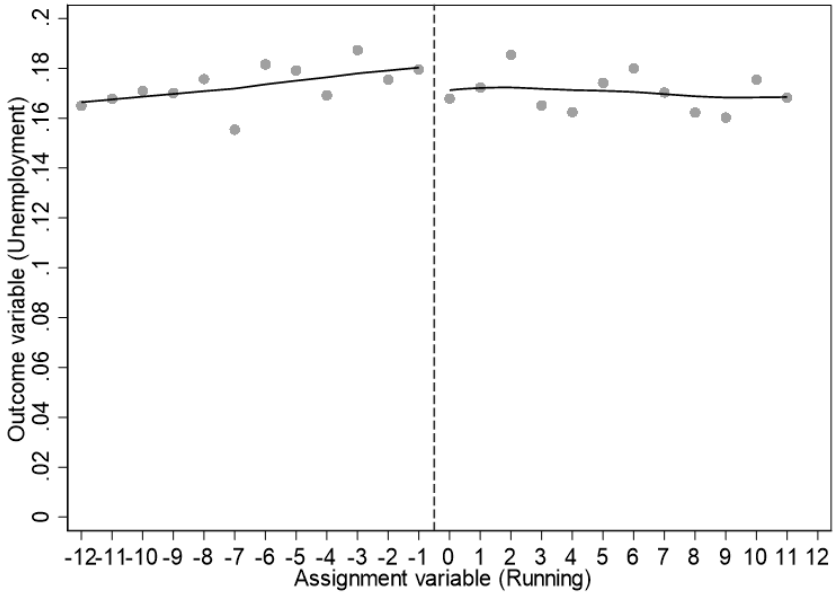


Figure B.2: Outcome: Unemployment



Note: FiguresB.1 to B.2 show RDD graphs for the outcomes (a) Employment and (b) Unemployment. The x-axis depicts the running variable and the y-axis average values of the respective outcome variables with local quadratic polynomials with a bandwidth of 12 months on each side and a triangular kernel.