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Employer Responses to Raising the Retirement Age: Spillovers on Coworkers and External Hiring

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Employer Responses to Raising the Retirement Age: Spillovers on Coworkers and External Hiring *

Sona Badalyan †

June 1, 2024
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Abstract

Low worker substitutability is a labor market friction that makes worker turnover costly to the firm. By bridging the literature on worker substitutability with the literature on labor supply effects of retirement reforms, I show that the older workers who posses specific skills and who are more difficult to substitutable internally by coworkers and externally by new hires or by automation are more likely to increase their employment in response to the reform that resulted in an increase of retirement age. To address endogeneity in labor supply decisions at an older age, I use regression discontinuity design by utilizing a reform in Germany that scrapped the opportunities to retire before the age 63. My results show that workers' labor supply response to the retirement reform is not solely made at the individual level, but is coordinated with firms dependent on their ease of substitutability, thus mitigating the costly turnover for firms. When analyzing the spillovers on coworkers and firms, I find that there are negative effects on hiring and promotions, especially of middle-aged women. The longer planning horizon specific to this reform does not eliminate the adjustment costs for establishments because of the frictions associated with worker turnover.

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1 Introduction

A large strand of literature shows that local labor markets (e.g. firms) matter in the wage losses of displaced workers (Jacobson et al., 1993), when replacing the workers due to their death (Jäger and Heining, 2022), as well as in parental leave decisions (Huebener et al., 2022; Ginja et al., 2023). While there is a wide range of research on the effect of retirement reforms on labor supply (Geyer and Welteke, 2021; Ye, 2020; Mastrobuoni, 2009), less is known about how internal labor market elements and worker substitutability shape the employment response to the retirement age increase. In this paper, I focus on the gap at the interactions of the literature of internal labor markets with the employment response to the retirement reform. The key element that I add to the literature is the firm perspective, i.e. since low worker substitutability makes the worker turnover costly to the firm, the workers may coordinate their retirement decisions with the firm in order to mitigate the costly turnover.

Studying the elements of internal markets in retirement literature is important. While the neoclassical theory relies on individualism, the internal labor markets incorporate more group-specific elements (Doeringer and Piore, 1971). The substitutability of workers can be understood as a group-specific element as well, i.e. if the labor supply decisions of older workers in response to the retirement reform are performed not solely at an individual level but also differ by availability of substitutes, then the decision has group-specific components, typical to the internal labor markets. Internal labor markets may have some procedures and rules which protect the internal workers and internal labor markets (e.g. firms) from external shocks. One element of internal labor markets that allows for such insurance is an implicit contract. On the one hand, workers receive wages below their productivity at the start of the tenure at a younger age and above productivity at the older age when their productivity is potentially lower (Lazear, 1979). On the other hand, the employers benefit from the incentive contracts as they help decrease the shrinking of employees (Doeringer and Piore, 1971). Hence, the senior workers are rewarded for their contributions and devotion to the firm. The implicit contracts result in attachments between workers and firms. Frederiksen and Manchester (2020) show passing an Age Discrimination act in the US led to weakening of implicit contracts. While I do not test for implicit contracts per se, I test whether employees coordinate their employment decisions at an older age with the availability of substitutes, which shows that they help the firms to mitigate the negative effect of costly turnover opposed to making retirement decisions individually.

The previous literature analyzed heterogeneity in retirement age increase as a reaction to a reform by the occupational demand, household characteristics, and health. The literature on occupational demand found mixed results- while Boockmann et al. (2023) shows that workers in occupations with high job strain exit employment earlier, in another paper Geyer et al. (2019) not find a different employment response by occupational demand. Other literature showed that household characteristics and income matter for employment at an older age- in particular, women with retired or low income partners have the largest employment effects (Geyer et al., 2020). Finally, workers with chronic diseases are more vulnerable to early exit (Hengel et al., 2021). Comparatively, there has been less attention on how the frictions in replacing workers influence the employment increase at an older age. However, the workforce composition and substitutability affects the careers of workers. For example, Linos et al. (2023) find that higher share of white coworkers in the initial teams of new hires is associated with a negative impact on promotions and retention of Black women. Jäger and Heining (2022) show that the frictions in replacing the workers are an important determinant for labor demand. Hence, I study heterogeneity in worker substitutability for an employment response to a retirement age increase reform, which is a new angle in the literate of labor supply mechanisms of retirement reforms.

To understand the mechanisms of the response to retirement age increase, I borrow some concepts discussed in the literature of internal labor markets and analyze in which situations worker turnover is costly for the employees and how such costly turnover affects the workers' employment decisions due to the reform. In particular, I create 3 main variable sets which show scenarios of costly turnover for firms- (1) human capital specificity of occupation; (2) internal and external substitutability; (3) substitutability by automation.

First, I analyze whether possessing specific skills leads to the higher employment response at an older age. If the skills are firm-specific, then the worker turnover is costly for employers. Firm-specific human capital is an important determinant of internal labor markets, as the likelihood of finding an external substitute for the skill-set of existing workers is difficult externally (Baker et al., 1994). To test if occupational specification matters, I create measures of tenure, human capital, and managerial status as proxies for specialized human capital.

Next, I focus on another dimension of worker substitutability- internal and external labor market thickness. External labor market thickness is defined as a local share of industry employment over the national share of industry employment, while the internal labor market thickness is constructed as a share of largest occupation in the workforce of the firm. In thin internal and external labor markets it is more difficult to find a substitute for the worker; making worker turnover costly for the firms. Moreover, the importance of one of the markets may drop once the other one is thick; hence, I also analyze the intersections of these markets.

Finally, older workers can be substituted not only by workers but also by automation. According to Acemoglu and Restrepo (2022), there is a positive correlation between aging population and automation. Nevertheless, some tasks may have comparative advantage of labor over automation (Acemoglu and Restrepo, 2019). For example, routine jobs are perceived as substitutes for automation, while non-routine tasks are complementing automation (Autor et al. (2003) as cited in Dengler et al. (2014)). Hence, if the retirement decisions depend on substitutability measures, the employment response to retirement age increase will be bigger in occupations that perform tasks complementing automation.

There are several problems I overcome to answer my research question. First, most of the seminal literature on internal labor markets, such as Baker et al. (1994), focused on just one enterprise, establishment or internal labor market, leaving scope for research on heterogeneity across the internal labor market attributes, which I contribute to. Germany has suitable data- a random sample of establishments with all of the workers and their employment histories observed. The presence of all the workforce within establishments provides me the possibility to study the role of internal markets, availability of substitutes within firms, and personnel practices overall.

Second, the employment decisions at an older age are affected by many factors, such as health, ability, and income; hence, such decisions are likely endogenous. I overcome the endogeneity issue by utilizing a reform in Germany that abolished the women's pathway to retirement and led to a sharp increase of retirement age by at least 3 years. The reform affected women discontinuously- starting from the cohort born in 1952; hence, it allows me to causally identify the effect of retirement age increase on employment using Regression Discontinuity design (RDD).

Last but not least, I wanted a country with the labor market with turnover frictions and difficult dismissal of workers. The previous literature for Germany showed that the frictions in replacing workers matter (Jäger and Heining, 2022; Huebener et al., 2022). In combination with relatively difficult dismissal of workers compared to the US, the German labor market gives a suitable setting for studying whether the workers coordinate their retirement decisions dependent on their substitutability.

I find that the reform increased the employment at the ages 60-62 by 17,5 percentage points (23% relative to the control mean). This result masks substantial heterogeneity. I find that the largest employment increase happens if the workers possess specific skills, and in the markets where the workers that are more difficult to substitute internally (by coworkers) and externally (by new hires from local labor markets). In addition, the workers holding occupations that complement automation (non-routine occupations) are more likely to increase the employment at an older age than the workers holding occupations that substitute the automation (routine occupations). In summary, such results show that raising the retirement age affects the employment decisions of workers differently by their substitutability levels; hence, such decisions are executed at the group-level, opposed to the individual one.

Finally, the spillover analyses show negative effect on hiring and promotion of middle-aged women.

The structure of this paper is as follows. The section 2 includes the institutional setting of German labor market, pension system, and details about the 1999 reform. Subsequently, in section 3, I describe the data I use and the sample construction. Next, section 4 specifies the identification strategy I employ to study the effect of the reform on labor supply and the mechanisms associated with employment, while the section 5 shows the corresponding estimation results and robustness checks, followed by conclusion.

2 Institutional setting

This section consists of 3 parts. First, I discuss the German labor market and its comparison to the US labor market. Next, I describe the main elements of the German public pension system- the three pillars, statutory retirement ages, and retirement pathways. Lastly, I discuss the 1999 reform that I study, and show the discontinuity in birth cohorts that it created, which I later utilize for my identification strategy.

2.1 German labor market

Germany is characterized as a labor market with relatively decentralized wage setting (Jäger and Heining, 2022; Dustmann et al., 2014). This feature of the market makes it easier to deviate the wages from the levels of bargaining agreements. The Equal Treatment Act (*Allgemeines Gleichbehandlungsgesetz – AGG*)[1] protects older workers from unjustified dismissal. Overall, the labor market structure in the years under my study make it unlikely for firms to fire older workers easily.

Historically, Germany was considered a country with relatively stable jobs and income within the internal labor markets, lower temporary layoffs, and higher severance pays compared to the US (Doeringer and Piore, 1971).

2.2 German public pension system

Germany has one of the oldest public pension systems in the world (Lorenz et al., 2018). There are three pillars of the German pension system- public, occupational pensions, and private provisions. However, the public pension insurance is the most popular choice among the working population, amounting to about 90% of German workforce (Zwick et al., 2022).

The public pension system consists of a pay-as-you-go scheme.[2]. There are 2 statutory retirement age levels: the early retirement age (ERA) and the normal retirement age (NRA). The ERA is the earliest age the person can retire, while the NRA is the earliest

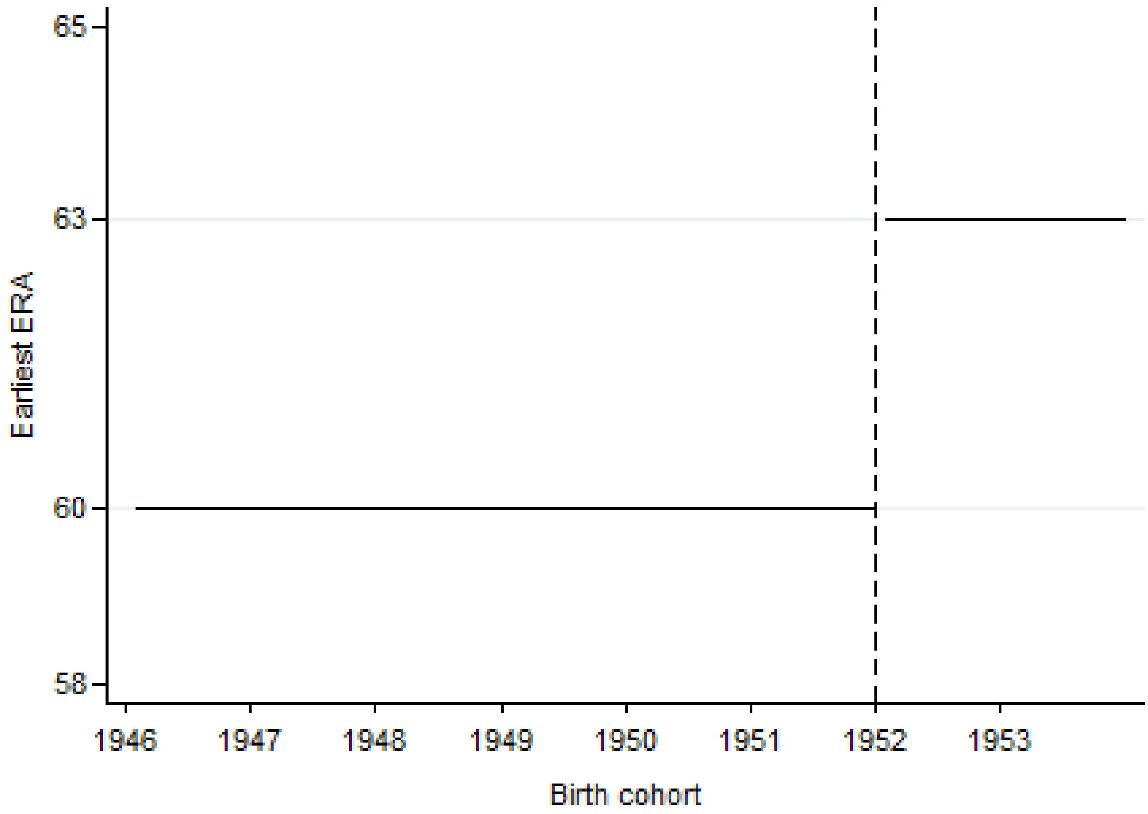
[1]General Act on Equal Treatment of 14 August 2006 (Federal Law Gazette I, p. 1897), as last amended by Article 4 of the Act of 19 December 2022 (Federal Law Gazette I, p. 2510).

[2]Pay-as-you-go scheme means that the current tax payers pay for the current pension claimants.

age the person can claim full pension benefits without actuarial deductions. Retirement between ERA and NRA leads to pension deductions, while the retirement after the NRA is associated with pension rewards. However, Seibold (2021) finds that the largest bunching appears at these 2 retirement ages, as people perceive them as reference points. Hence, given the weight people put on these 2 statutory retirement ages, the reforms which change the levels of ERA and NRA lead to large labor supply responses (see the findings of Riphahn and Schrader (2021) or Geyer and Welteke (2021) for a reference).

There are several pathways to retirement, such as regular, disability, long-insurance, women, and unemployment pathways. While the rules of some of these pathways changed or they were abolished all together, the workers eligible for regular pathway to retirement did not have 2 different statutory retirement ages, as ERA and NRA are equivalent for them. On contrary, ERA exists on pathways that are assumed to be for more vulnerable groups such as women, unemployed, or long-insured.

Figure 1: The treatment assignment by 1999 reform



Notes: this graph shows the assignment rule of early retirement age by birth cohorts. For cohorts 1946-1953, I plot the earliest possible ERA for women. The vertical dashed line at the January 1952 cohort indicates the birth cutoff starting from which the women's pathway was abolished, resulting in an increase of ERA by at least 3 years. ERA increased by 3 years for women who had contributions to qualify for long-term insurance pathway or even more if they qualified for regular pathways. For simplicity, I plot the minimum increase of ERA - i.e. just 3 years.

2.3 The 1999 reform

The 1999 reform abolished the women's pathway to retirement.[3] The reform affected women born from January 1, 1952; hence, the change was discontinuous in birth cohort. For those of them who accumulated enough contributions to be eligible for long insurance pathway, the ERA increased by 3 years, while for the workers on regular pathway to retirement ERA increased by at least 5,5 years. As a result, the reform led to an increase in ERA by at least 3 years (see Figure 1).[4]

[3] While the reform also abolished the pensions for unemployed and persons under a progressive retirement plan (Lorenz et al., 2018), I focus primarily on the abolishment of women's pathway because the other 2 categories are not recorded in my data.

[4] Before the 1999 reform, the NRA of women's pathway to retirement was fixed at 65 years old. After the abolishment of women's pathway to retirement, women also were affected by another reform which

An important feature of this reform is that not only did it increase the retirement age, but also abolished the women's pathway to retirement all together. In addition, the reform was announced when the first affected cohort was only 47 years old; hence, the long planning horizon specific to this reform makes it less likely to be associated with liquidity constraints, and rather more likely to be related to the changes in social and cultural norms.

3 Data

This section consists of two parts. First, I describe the original data I utilize, its sampling procedure, and suitability to my research question. Second, I describe how I construct my sample size, the reasoning behind each restriction, and the resulting sample size.

3.1 The Sample of Integrated Employer-Employee Data

I use the Sample of Integrated Employer-Employee Data (SIEED7518), which is a random 1.5% sample of all the establishments in Germany. [5] The employers are obliged to report data on all of their employees subject to Social Security. Hence, such data excludes self-employed and civil servants. By the June 30$^{\text{th}}$ in each year, the employers report the start and end date of employment, wages [6] and other occupational, educational, and demographic indicators. In addition to their June 30$^{\text{th}}$ reports, the employers are also obliged to report changes in employment contracts. One drawback of this data is that it lacks hours of work; hence, I am limited to the analyses of only the extensive margin of employment, but not the intensive one, such as number of hours worked, etc.

For each of these establishments, the entire employment biographies of all the employees are included over the observation period 1975-2018 for the West Germany and 1992-2018 for the East Germany. Hence, the data also includes the establishments which were not constituting the originally sampled random 1.5% of establishments, in case the workers from the originally sampled establishments were ever employed elsewhere. Observing the entire workforce of the sampled establishments is pivotal for my analyses because I study the establishment and substitutability mechanisms behind the employment reactions to the retirement age increase, which require observing all the coworkers.

affected the regular pathway to retirement. In particular, due to the 2007 reform, the workers on the regular pathway to retirement experienced retirement age increase starting from the cohort 1946 by 1 month per birth year due (see Figure A.1 in the Appendix), and it is expected to reach 67 years old for the 1963 birth year cohort by 2029. This 2007 reform affected the women under my study, because the NRA of those born on 1951 was 65 years old, wile that of those born in 1952 became 65.5 years old.

[5]The paper uses SIEED7518 data. The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently via remote data access.

[6]The wages are censored (top coded).

The establishment identifiers are fixed by industry, ownership, and location at the municipality level; hence, in selected cases, an establishment is not equivalent to a firm. Despite that, I use the words firms and establishments interchangeably. Schmidtlein et al. (2020) describe the data sampling in more details.

3.2 Sample construction for labor supply analyses

To construct the final sample for my analyses, I keep only women born in 1951- the control group, i.e, the women who were potentially eligible for women pathway if they would accumulate enough years of contributions in later life; and 1952- the treatment group, i.e. the women who encountered the abolishment of women's pathway to retirement. I delete minors and sailors as their retirement rules differ from those in other occupations.[7]

To address the issue of parallel spells in my data, I keep the spells in the randomly selected 1.5% establishments with the highest tenure. In cases where the employee works in 2 randomly selected establishments with equal amount of tenure, I keep the job with highest wage. Dropping the parallel spells allows me to construct panel data and study the firm mechanisms for only the establishments which the dual workers are more attached to.

The final data consists of person-age entries (in age-month), where I observe women from the age 42 (age-month 504) up until 66 (age-month 792). The choice of this time frame is driven by the fact that the first affected cohort was 47 years old at the time of the reform announcement in 1999, and in some of my analyses I want to observe the employment (1) before the reform announcement, (2) between the reform announcement and its inaction at 60 years old, (3) as well as beyond both the ERA (60 or at least 63) and NRA (65 or 65 years 6 months). First, studying employment before the reform announcement shows whether the treatment and control groups had different labor supply before the reform announcement. Second, studying employment between 47-60 years old is interesting because it shows whether increasing the ERA leads to different employment choices during middle age in expectation to the longer employment time span. Finally, studying the effects beyond the new ERA shows how the effect of increasing ERA also spills over to the post ERA employment- which could show indirect employment effects beyond the age targeted by the reform, further increasing its effectiveness.

I keep the workers who are continuously employed at the age 58 and 59. Since most of the main heterogeneity variables are constructed at the establishment level, I want the

[7] The seminal work by Geyer and Welteke (2021) on labor supply responses of the 1999 reform makes additional restrictions, such as keeping only women who are eligible for the women's pathway to retirement at the age of 60, i.e. I do not make the sample restrictions that keep the women eligible for women's pathway (e.g. 15 years of contributions in total and 10 years after 40 years old, etc), because I do not observe the unemployment spells that also contribute to the contribution years. Not making this restriction results in smaller treatment effects in my sample, compared to theirs.

workers to have sufficient attachment to them. The final data consists of 32,770 workers, observed over 9,036,582 worker-age months.

3.3 Establishment data construction for spillover effects

First, I construct employer × year and establishment × year data, following the procedure described in Dauth and Eppelsheimer (2020). Next, after defining main treatment and outcome variables, such as individual-level hiring, promotion, etc- I aggregate them at the establishment level. I keep all the establishments that had at least 1 worker born in 1950-1953 time frame (i.e. 2 years before and after the ERA increase cutoff) employed continuously in 2009. I chose to construct my treatment variables in 2009, because it is the last year when all of the 4 cohorts 1950-1953 are younger than 60 years old- the ERA before the reform. Next, I delete the establishments with less than 5 workers and the establishments that are at the top 5 percent of the establishment distribution, which results in the establishment size ranging from 5-249 workers [double check after PP mode]. Lastly, I keep the establishments that are observed in the years 2006-2016, but also keep the years 2004-2005 and 2017-2018, despite that some establishments are not observed in those 4 years. [8]. The final sample consists of 3750 establishments with the total amount of 8015 focal workers and 122205 coworkers. Establishments had from 5 up to 246 employees (median = 16 workers). [TBA insert sumstats in 2009, discuss]

4 Identification

4.1 Labor supply effects: RDD

In section 2, I showed that the 1999 reform led to discontinuity in birth cohort, leading to the jump in the ERA. Such feature of the reform allows me to analyze the causal effect of ERA increase on employment outcomes by RDD.

I follow Geyer and Welteke (2021) to locally identify the effect of the reform that increased ERA on employment, τ, in an RDD framework:[9]

$$\begin{aligned} y_{im} = \alpha + \tau \mathbb{1}\{b_i \geq b^*\} + \\ + \beta_0 \mathbb{1}\{b_i < b^*\}(b_i - b^*) + \beta_1 \mathbb{1}\{b_i \geq b^*\}(b_i - b^*) + X_i'\beta + \epsilon_{im} \end{aligned} \tag{1}$$

[8] I do not restrict the sample to the establishments observed in all the years under study: 2004-2018, so that I keep a bigger sample size

[9] There are several differences from the identification in Geyer and Welteke (2021). First, I do not control for children in my RDD regression as I do not observe such variables in my data. Second, since the last observed year in my data is 2018, my data allows me to pool together all the age-months corresponding to 60-62 years old in the baseline regression below, while they pooled only 60-61. Finally, I use mean square-based optimal bandwidth with uniform kernel, while they use 12-month bandwidth with triangular weights.

where y_{im}- is employment status, recorded for each individual i at every age in months m; b_i is the birth cohort of the individual i; $\mathbb{1}\{b_i \geq b^*\})$ is an indicator showing that i was born starting from the cutoff b^* (January 1952), i.e. experienced the increase of early retirement age (treatment group); while $\mathbb{1}\{b_i < b^*\})$ includes the individuals who are below the cutoff (control group). Hence, I use first order polynomials and allow for different slopes around the cutoff. The Figure 2 below shows that a linear trend in the running variable is a plausible assumption and there is a clear discontinuity which is unlikely to be attributed to a wrong functional form of polynomials. To compute the RDD estimates, I use uniform kernel function, and mean square error-based optimal bandwidth choice (Imbens and Kalyanaraman, 2012). As a result, I display the bias-corrected RD estimates with robust variance estimator.

I also control for calendar month, a dummy for Western residence, wages at the age 46, and 2 education categories (out of 3), as the previous literature confirms that education is an important determinant of employment at an older age Geyer et al. (2019). I cluster the standard errors at the birth month level to account for potential correlation of age-months across the people belonging to the same birth cohort.

This identification relies on 2 main assumptions: (1) continuity of the running variable (birth cohort) around the cutoff, which eliminates the possibility of strategic bunching (manipulation of the treatment status) at the cutoff, and (2) continuity of the distribution of the observed and unobserved variables around the threshold, showing that the assignment of the treatment around the cutoff is random. I test for these assumptions in section 5.

The baseline regressions pool the 60-62 ages (720-756 age months) together, as this is the age frame which was affected by the ERA increase reform. This identification results in a local average treatment affect (LATE) of ERA increase on employment outcomes at the ages 60-62 (coefficient τ in equation Equation 1).

In terms of outcome variables, at each age-month I create 3 main labor market categories - employment, nonemployment, and retirement. I further disentangle the employment into 3 groups- employees liable to social security, marginal part-time employment and partial retirement. Nonemployment stands for a gap in the employment age-month spells. I proxy retirement age-months with the the age-months after the last labor market activities. Figure A.2 in the Appendix displays the evolution of 3 main employment states over age by treatment status. It shows clearly the gap in employment and retirement statuses at the age 60-62.

To study the mechanisms behind these effects, I perform subsample analysis by several categories of variables, described in the next section.

4.2 Labor demand effects (spillovers): generalized DiD

To identify the spillover effects, I compare establishments with similar composition of workforce, but a random variation in the amount of workers who experienced ERA increase. Hence, I focus on establishments who had at least 1 worker born in a narrow window around the cutoff 1952 employed in 2009. I estimate a Generalized Difference-in-Differences (DiD)[10]:

$$\begin{aligned} y_{jt} = &\sum_{t=2006}^{t=2016} \beta_t \mathbb{1}\{year = t\} N\_treat_j + \\ &+ \sum_{t=2006}^{t=2016} \gamma_t \mathbb{1}\{year = t\} N\_OldWom_j + \sum_{t=2006}^{t=2016} \delta_t \mathbb{1}\{year = t\} N\_Old_j + \sum_{t=2006}^{t=2016} \zeta_t \mathbb{1}\{year = t\} N_j + \\ &+ \alpha_j + \lambda_t + \epsilon_{jt} \end{aligned} \tag{2}$$

where y_{jt}- outcomes of interest (number of hired workers, number of promotions, etc), N_treat_j- number of workers in 2009 that belong to 1952-1953 (treated) cohorts, N_OldWom_j- number of workers in 2009 that belong to 1950-1953 (treated and control) cohorts, N_Old_j- number of workers (both male and female) born in 1947-1955 cohorts- the cohorts which reach their ERA, NRA ages in the period of main reform effects, N_j- total number of workers in 2009. α_j are establishment fixed effects and control for firms' characteristics that are constant over time, λ_t are year fixed effects controlling for time-varying shocks common to all establishments, ϵ_{it} is the error term.

The coefficient of interest is β_t, which shows the effect of having an additional worker who experienced ERA increase (born to the right of the cutoff, i.e. 1952-1953) employed before reaching the pre-reform ERA (in 2009) on the outcomes of interest.

In my baseline results, I pool together the years when the workers born in 1950-1953 turn 60-62, i.e. in the simplified DiD model *Post* stands for the years from 2012, when the first treated cohort 1952 turns 60, till 2015, when the last treated cohort 1953 turns 62; hence, the variable *Post* stands for 2012-2015, while *Antic* stands for 2010-2011. The labor market in the periods of these 2 variables was not characterized by any large macroeconomic shocks.

[10] An identification close to mine was used by Hut (ming). The difference is that my 1999 reform affected primarily the women, while the Dutch reform that Hut (ming) studies affected both genders, hence; I control for the number of all the people who will reach their retirement ages in reform enaction years, but keep the main treatment variable as the number of women belonging to the affected cohorts after the cutoff (1952-1953).

$$
\begin{aligned}
y_{jt} =& \beta_P(Post \times N\_treat_j) + \beta_A(Antic \times N\_treat_j) + \\
&+ \gamma_P(Post \times N\_OldWom_j) + \gamma_A(Antic \times N\_OldWom_j) + \\
&+ \delta_P(Post \times N\_Old_j) + \delta_A(Antic \times N\_Old_j) + \\
&+ \zeta_P(Post \times N_j) + \zeta_A(Antic \times N_j) + \\
&+ \kappa_p Post + \kappa_A Antic + \theta_j + \eta_t + \epsilon_{jt}
\end{aligned} \tag{3}
$$

I cluster the standard errors in equations 2 and 3 at the establishment level to address potential correlation across workers in the same establishments. This identification results in average treatment on treated (ATT) effect of an additional treated worker around the cutoff on the establishment outcomes.

The main identification assumption is that absent the treatment the outcomes (e.g. hiring or promotions) in more and less exposed establishments would have followed parallel trends. I test for parallel trends assumption directly by looking at event-coefficients in pre-treatment construction years (2006-2009).

One threat of identification strategy may occur if the focal workers were strategically employed at establishments with different workforce composition in 2009. Since the reform was announced in 1999, there is a 10 year window between the reform announcement and my treatment construction. I redo the analyses, constructing the treatment in 1999 as a robustness check.

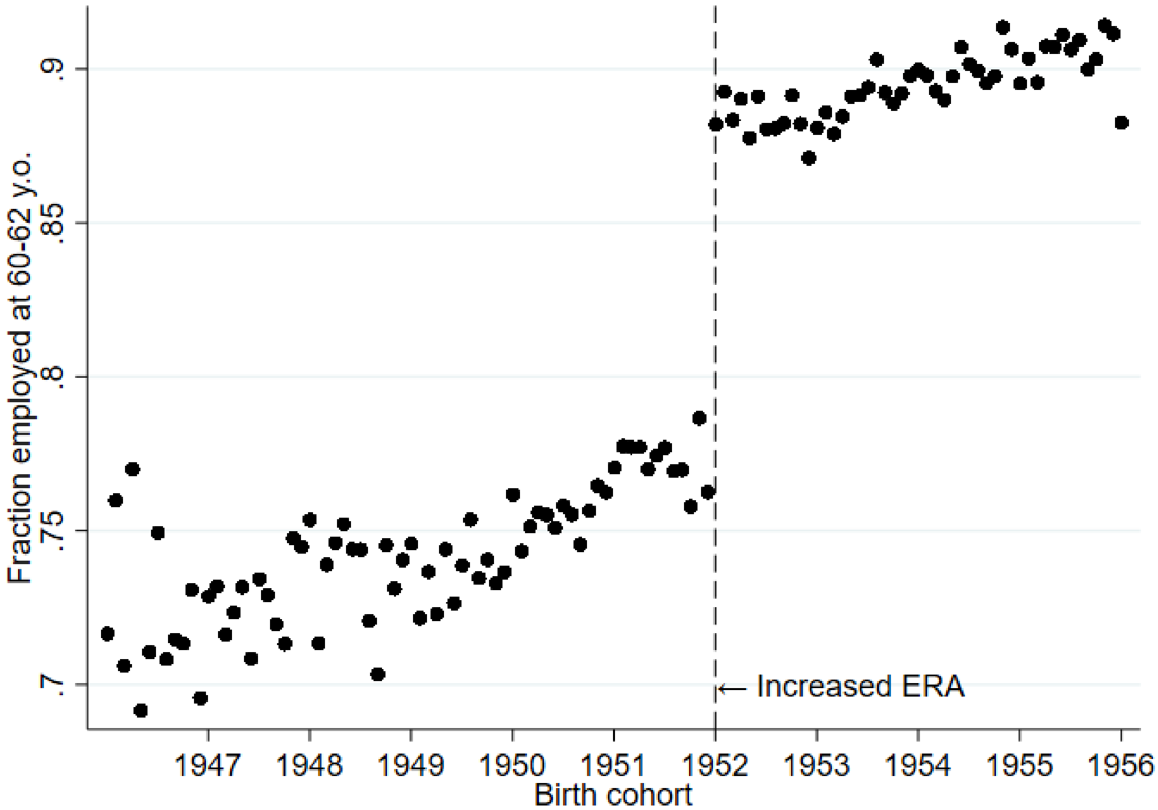
5 Labor Supply and Worker Substitutability

This section presents the effect of the ERA increase on workers' labor supply decisions at the ages 60-62. The abolishment of women's pathway to retirement led to a large increase in employment rates at the age of 60-62 years old, as shown in Figure 2. While overall there is an upward sloping employment at the 60-62 years old over the birth cohorts, there is also a clear discontinuity around the 1952 cohort. Only around 75 percent of women born in 1946-1951 were employed at 60-62 [11]; however, the employment rate jumped to approximately 90 percent starting with the 1952 cohort- the reform cutoff.

Figure A.3 in the Appendix confirms the presence of discontinuity in employment rates at the age 60-62 (due to the 1999 reform that I study) and to a smaller magnitude at the ages 65-65.5 (due to the 2007 reform). Estimating the treatment effects of the 2007 reform is beyond the scope of this paper; hence, in this section I causally quantify the biggest employment discontinuity that happens due to the 1999 reform - i.e. at the ages 60-62.

[11] This control mean is higher than that of existing literature studying the labor supply response of this reform (Geyer and Welteke, 2021), likely because the sampling of SIEED and my sample restriction (employment at the ages 58-59) results in a sample of workers more attached to the labor force.

Figure 2: Discontinuity in the running variable



Notes: this graph is the scatter plot of the fraction of women employed at the ages 60-62 over the birth cohorts. The dashed line presents the birth cohort cutoff, January 1952, starting with which the ERA increased by at least 3 years.

First, I show that women adjust their employment trajectories at the age 60-62 in a reaction to ERA increase. In particular, they are more likely to be employed, and less likely to be nonemployed or retired at 60-62 years old. Interestingly, the partial retirement claims, as well as marginal part-time employment increase at the age 60-62;l hence, the employees respond at the extensive margin, but not necessarily the intensive margin of employment.

Next, for studying how worker substitutability and skills affect such decisions, I focus only on *employment* as an outcome variable through the rest of the paper, and study 3 groups of mechanisms showing different levels of worker substitutability. The first group's variables are created based on the individual's occupation, rather than markets. Second group is based on market-level substitutability. The last includes the task-based substitutability. While neither of these groups is more preferred to another, they all show different dimensions of substitutability which complement each other for the fuller picture. I confirm that (1) women who work on occupations with high specificity of human capital (higher return to experience) or in managerial positions; (2) women who are not substitutable internally (by coworkers) and externally (by external hires); (3) and women who perform tasks requiring higher skills on occupation are more likely to increase their employment. Such results show that the labor supply decisions are not made on individual level, but are rather influenced by group-specific components and worker substitutability.

The higher employment effect for less substitutable workers could be driven by a combination of high supply and demand effects - i.e. workers that are harder to substitute may have higher utility from working (labor supply) and be more valued by the firms (labor demand). Disentangling between the labor supply and demand is challenging because I observe only the final outcome- employment.

5.1 The effect of increased ERA on employment at the age 60-62

I start by analyzing how the employment statuses change at the ages targeted by the retirement reform- at 60-62 years old. In this section, I analyze several employment statuses as outcome variables- (1) the *employment* (which is further disentangled into *employment subject to social security*, *marginal part time employment*, and *partial retirement*), (2) *nonemployment*, and (3)*retirement* (see section 4 for more details about these variables). Figure 3 shows the causal effect of the increase of ERA on employment statuses at the age 60-62. The 77,2% of women in the control group (born in 1952), are employed at the ages 60-62, while 5% are nonemployed, and 17.8 % are already retired.

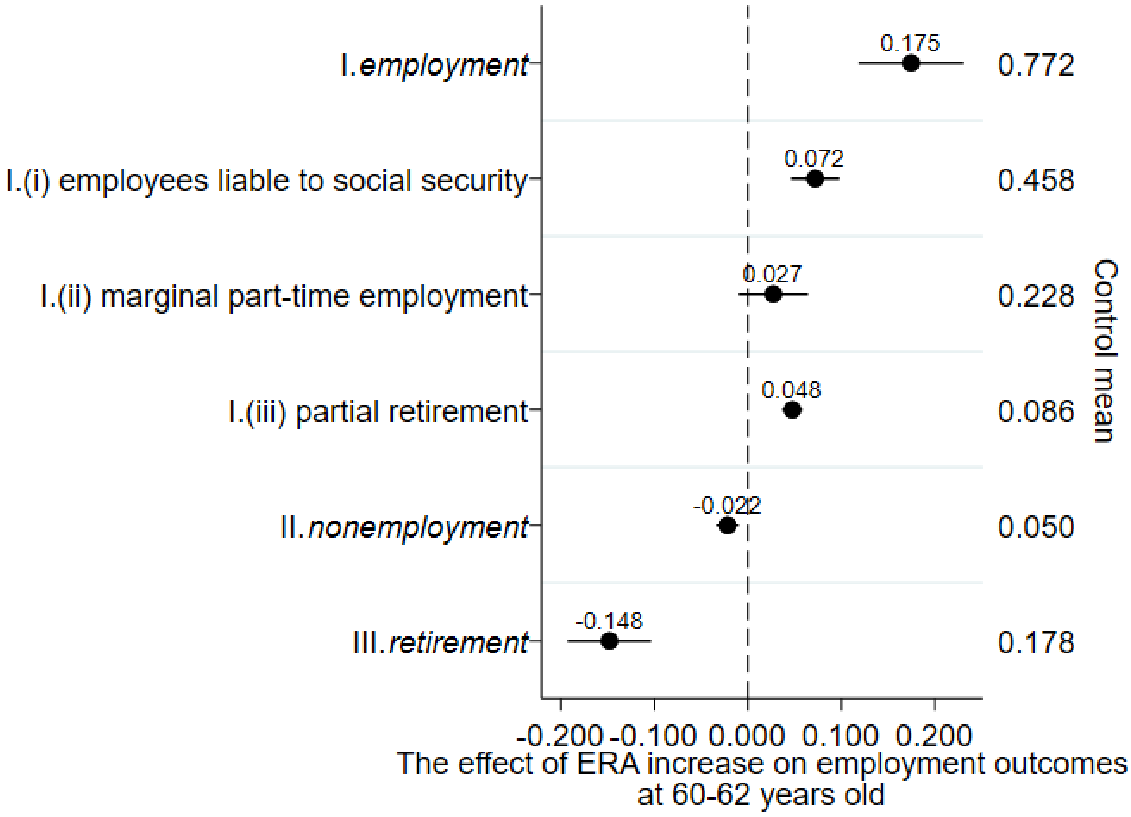
Increasing ERA leads to the increase of likelihood to be employed at the ages 60-62 by 17,5 percentage points (pp) ($p < 0.01$; a 22.7% increase relative to control mean). Figure A.4 in the Appendix zooms in *employment* outcome in an RD plot, and confirms once more the presence of a discontinuous jump.

Despite that most of such increase in employment is attributed to the employment subject to social security, i.e. 7.2 pp ($p < 0.01$; a 15.7% increase relative to control mean), there is also some evidence of the increase of partial retirement claims by 4.8 pp ($p < 0.01$; a 55.8% increase relative to control mean).

The likelihood to retire at 60-62 years old drops by 14.8 pp ($p < 0.01$; a 83.1% decrease relative to control mean), and there is a small negative effect on nonemployment: 2.2 pp ($p < 0.01$; 44% decrease relative to control mean). Overall, such results show that the workers are more likely to be present at the workplace due to the reform. [12]

[12] Additionally, I analyze the effect of the reform on wages at the age 60-63. I find that the wages increase by around 278 EUR/month (see the last column in Table B.1). However, studying the effect of the reform on wages may be biased by the mediation problem, i.e. the reform affects the employment decisions along with wages, and there is also the mediation channel where being employed at 60-62 years old affects the wages; hence, I do not focus on that outcome.

Figure 3: Effect of an increased ERA on the employment status (overall and from each category)



Notes: Coefficient plots. Each row corresponds to the RDD regression of the share of employment status of the corresponding category (left axis) around the 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. The points represent the estimated coefficients and the bars represent the 95% confidence intervals. The control means (right column) are the means of the share of employment status in the corresponding category over the control group (born in 1951). The corresponding table with more details can be found in Table B.1 in the Appendix.

The results described above pool the ages 60-62 together, but it is interesting to see the employment effects separately by each age-month. Figure A.5 in the Appendix shows the same RDD regressions for each age-month separately, opposed to pooling the age months corresponding to 60-62 years old above. There are 2 main periods of significant effects- at the ages of 60-62 and 65-65.5, the rest are either insignificant or very small. The widest gap in employment appears at the ages 60-62, corresponding to the effects of ERA increase per 1999 reform, while the increase at 65-65 years and 6 months corresponds to the 2007 reform's NRA increase response.[13] At all the other age months the employment

[13]Despite that the 2007 reform resulted in NRA increase for the same cohorts under study, the direction

effects are either insignificant or very small in size.

As I have analyzed the overall employment effects above, I undertake analyzing the mechanisms of the employment response through subsample analyses in the subsections below. Henceforth, I analyze only one outcome variable- *employment.* I focus on 3 groups of mechanisms- worker skills, internal and external substitutability, and the role of occupational tasks. All of these groups of variables create measures which show whether the worker is harder to substitute.

5.2 The role of worker skills

The first group of variables showing substitutability of workers is the worker skills. I create two measures: return to occupation (which I interchangeably call human capital specificity of occupation) and managerial occupations. Both of these variables show the substitutability of workers. The choice of the first variable was driven by the idea that the more specific skills the worker has the less likely she is to be substituted. In a related study, while the choice of the second variable is motivated by Jäger and Heining (2022) who find that the death of a manager or a worker on specialized occupation leads to a more negative effects on the workers in other occupations. In my setting, if a worker is a manager, she likely has many coworkers under her hierarchy, likely communicates with them more and has more information, making her less substitutable. Absent worker leaving, such workers are usually moved up the hierarchy; hence, their retirement may lead to difficulty to substitute them.

Human capital specificity of occupation. To obtain a measure of human capital specificity of an occupation, I follow a similar strategy to Jäger and Heining (2022) and Bleakley and Lin (2012) to estimate Mincer equations for each of the 3-digit occupations. [14]. I use the occupation-specific returns to experience, and classify the specialization as high if this return is greater than the median value. I define this variable at the age 58.

Managerial status. I create a variable showing the managerial or supervisory status from the last 2 digits of the 5-digit occupations. I pool the supervisors and managers together into the dummy variable *managers.* I define this variable at the age 46, as the managerial position after the reform is influenced by the treatment status and is a potential outcome itself, rather than a candidate for subsample analyses.

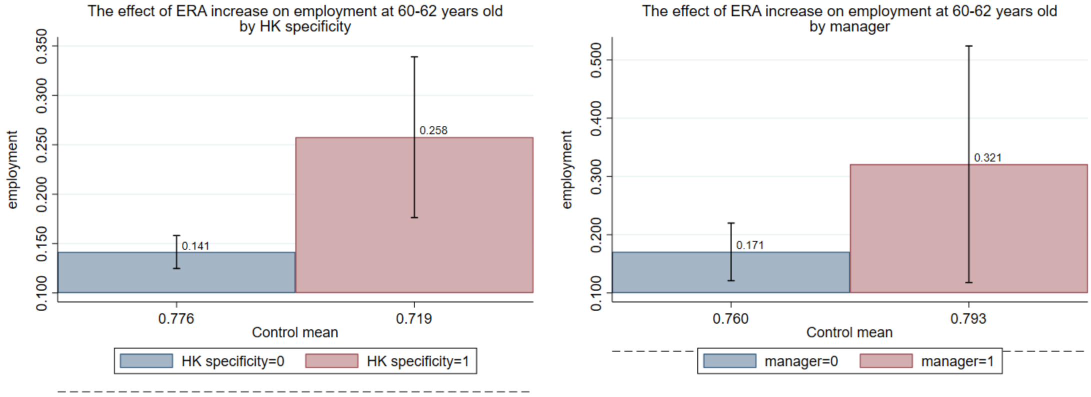
The Figure 4 below displays the results. It shows that the workers in occupations that require higher specificity have significantly higher employment effects (almost twice) at the age 60-62 due to the reform, than those employed at the occupations with lower specificity. Despite less significant due to sample size, the managerial status also leads to higher employment outcomes in response to the reform. Hence, I conclude that the

of effects is the same, and is unlikely to cause any threat on identification of the 1999 reform under study. Analyzing the effects of the 2007 reform is beyond the scope of this paper.

[14]Given the smaller sample size, I use only 3-digit occupations opposed to Jäger and Heining (2022)

treatment effect hides substantial heterogeneity by the demand of worker skills.

Figure 4: The effect of the ERA increase on employment at age 60-62 by return to occupation and managerial occupations



Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. The subsample analysis in the left panel is performed by "*HK specificity*"- which stands for human capital specificity of occupation. It is based on the return of experience in Mincer equations performed separately for each of the 3-digit occupations. The blue color stands for below median value, while the red color stands for above the median level of corresponding index. The right panel stands for managerial status, which is created as a dummy from the last 2 digits of the 5-digit occupational variables. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.2 in the Appendix.

5.3 The role of market-level worker substitutability in employment increase

The next group of variables showing the substitutability of workers is based on the markets- internal (establishment) and external (local labor market). The main motivation for studying internal labor market thickness is that the more diversified the set of occupations within the establishment, the less substitutable the workers of such establishments are. Similarly, the less workers are performing jobs in the industry of the establishment in local labor markets, the less substitutable the workers of such establishments are, motivating the analyses by external labor market thickness.

Internal labor market thickness (ILMT). I define the internal labor market thickness as a share (s_j) of the largest occupation employment ($N_j^{\text{largest occupation}}$) of establishment j in total employment of the establishment (N_j), following Ginja et al. (2023) and Cortes and Salvatori (2019):

$$s_j = \frac{N_j^{\text{largest occupation}}}{N_j} \tag{4}$$

I define the internal labor market as thick if this index is above its median value.

External labor market thickness (ELMT). I define the ELMT in 2 steps. First, I create 50 labor market regions based on high within and low across commuting for work, following Kropp and Schwengler (2011) (see Figure A.6 in the Appendix). Next, I create an index θ_{kc}, showing the local labor market share of 2-digit industry employment (N_{kc}/N_c) over the national share of industry employment (N_k/N). [15]

$$\theta_{kc} = \frac{N_{kc}/N_c}{N_k/N} \tag{5}$$

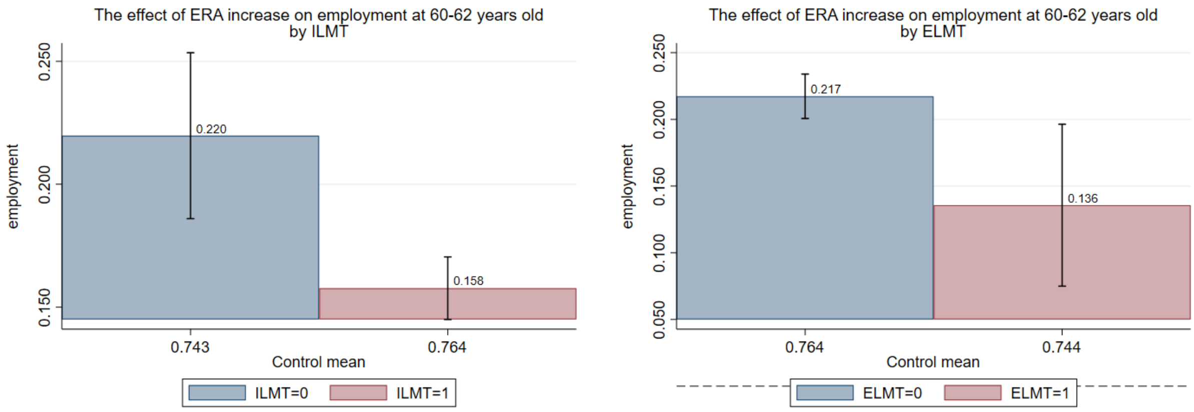
where k is industry, and c is commuting zone, N_{kc} shows the number of workers employed in industry k and in the commuting zone c, N_c is the number of workers employed in the commuting zone c and all the industries together, N_k is the number of workers employed in the industry k in all the commuting zones together, while N is the number of workers employed in all the industries and all the commuting zones together (i.e. country). All of these variables are defined based on my SIEED data, but since it is representative of all the German establishments in country, I expect these indices to proxy the country-level index well.[16] I call an external labor market thick if this index takes values above its median value. I define both the internal and external labor market thicknesses at the age 58.

By performing subsample analyses, I find that women in thinner internal labor markets are around 1.4 times more likely to increase their employment relative to those in thick markets. Such result means that the employment response to the retirement age increase reform is higher if there are not many internal coworkers in the same occupations available (see the left panel of Figure 5). Similarly, the workers in thin external labor markets are around 1.6 times more likely to increase their employment at the ages 60-62 in the thick external labor markets relative to the thin ones. Hence, there is higher employment response to the ERA increase due to the lack of external substitutes (see the right panel of Figure 5).

[15]The 2-digit mapping of establishments is based on the IAB establishment panel. To aggregate the industries into these groups, I follow Dauth and Eppelsheimer (2020).

[16]Since I do not observe the civil servants in my data, this measure is more representative of the workforce subject to social security.

Figure 5: Subsample analyses for the effect of the ERA increase on employment at age 60-62 by internal and external substitutability

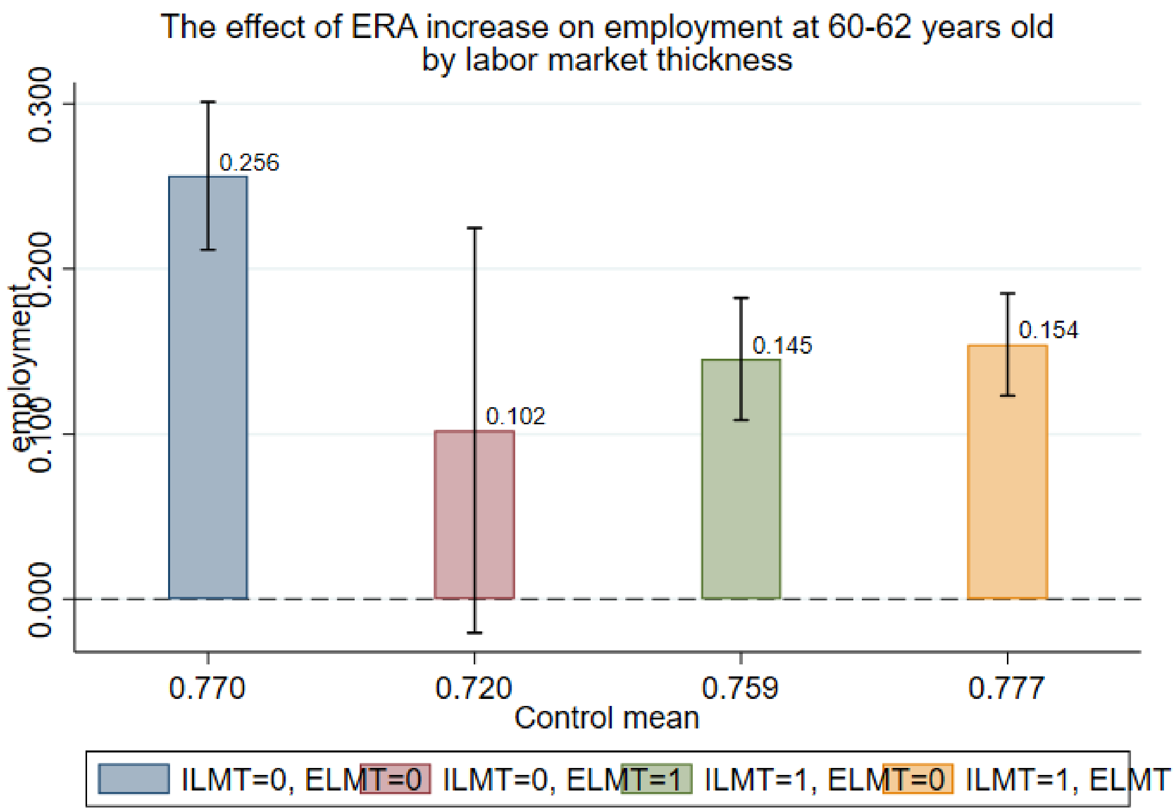


Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. The left panel shows subsample analyses by thin (blue) and thick (red) *internal labor market thickness (ILMT)*, based on median split of the biggest occupation employment over the total workforce employment in the establishment. The right panel shows subsample analyses by thin (blue) and thick (red) *external labor market thickness (ELMT)*, based on median split of the local labor market's share of industry employment over the national share. If these variables are equal 1 it means that their value is above their median values. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.3 in the Appendix.

An important follow-up question is whether the thickness of one of the markets (e.g. internal) diminishes once the other one (e.g. external) is thick. Figure 6 shows that the highest employment response to the retirement age increase happens in establishments that have thin markets both internally and externally: 25.6 p.p. ($p < 0.01$; a 33.2% increase relative to control mean). While all in all the other 3 categories of labor market interactions provide smaller employment increase, they are not significantly different from one another. Hence, the only conclusion is that if both markets are thin (i.e. workers are neither easily substitutable internally nor externally), that's when the worker increases employment the most.

To summarize, this section shows that both internal and external substitutabilities play a crucial role in employment response to the retirement reform; however, the internal and external markets also interact.

Figure 6: The effect of the ERA increase on employment at age 60-62 by interactions of two categories of labor market thickness



Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. I perform subsample analyses by interactions of ILMT and ELMT. The *internal labor market thickness (ILMT)* is based on median split of the biggest occupation employment over the total workforce employment in the establishment. The *external labor market thickness (ELMT)* is based on median split of the local labor market's share of industry employment over the national share. If these variables are equal 1 it means that their value is above their median values. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.4 in the Appendix.

5.4 The role of occupational task-level substitutability

While the results above mostly focus on substitutability of workers by other workers, the substitutability of workers by capital is another important dimension which can provide the full picture in the role of worker substitutability in employment response to the retirement age increase. To analyze this dimension of substitution, I focus on task types. The occupations in Germany are classified by the share of tasks implemented. Some of these tasks are more likely to be substituted by automation than the other; hence, I create 5 categories of tasks and implement subsample analyses by them. This variable is defined when the worker was 46 years old.

Task type. To obtain the 5 task types (analytical non-routine, interactive non-routine, cognitive routine, manual routine, manual non-routine), I merge my data to the assignments created by Dengler et al. (2014) by the 3-digit occupation identifier. [17] I define these variables at the age of 46.

Below I describe the meaning of the classification. The first dimension of classification refers to analytical, interactive, and manual tasks. *Analytical* category refers to the tasks that require to think or analyze, while *interactive* category refers to the tasks which require communication. *Manual* tasks can be performed by hands. The second dimension refers to the automation level- i.e. the routine tasks can be performed by machines, while the non-routine tasks cannot. Hence, the workers on routine tasks are more likely to be complemented by automation than those on non-routine tasks.

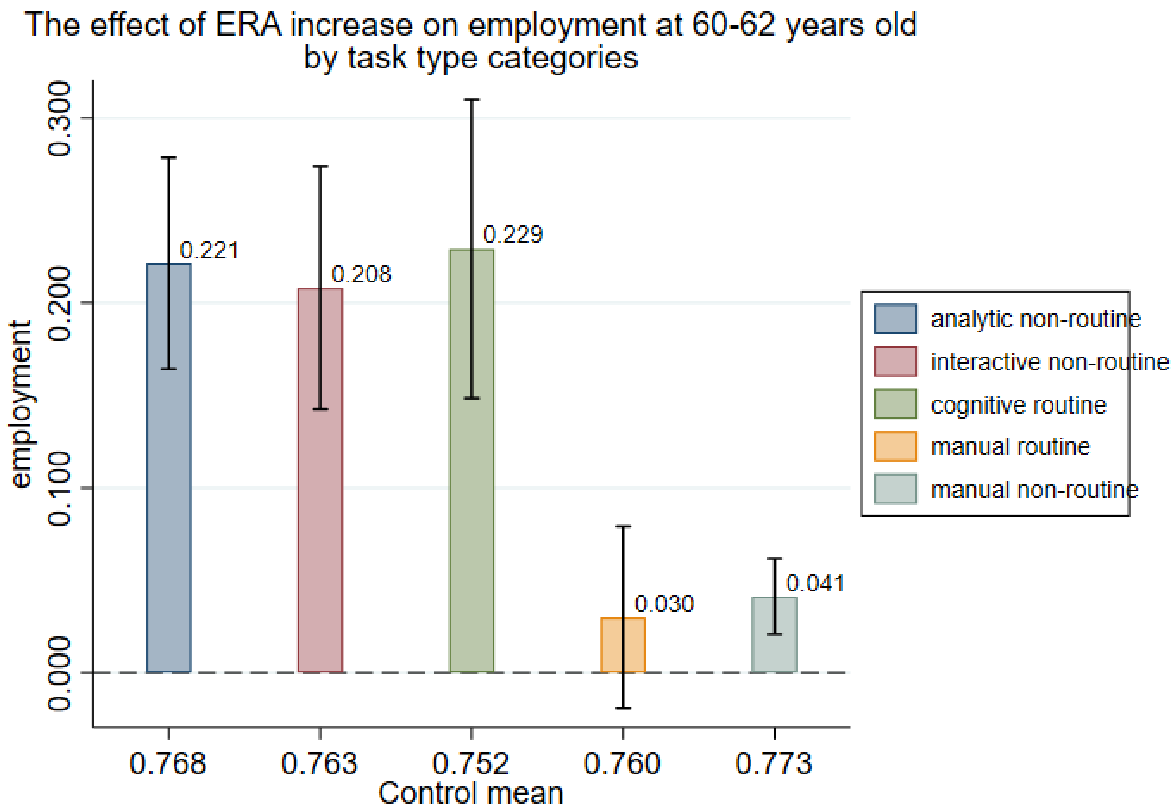
Figure 7 shows that the highest employment is recorded at the analytic non-routine, interactive non-routine and cognitive routine jobs. The manual routine jobs are insignificant, while the manual non-routine tasks have the smallest employment effect. While all the results go in line with the hypothesis on complementary or substitutability of automation, the result of cognitive routine tasks is somewhat puzzling, and goes against that hypothesis. There are several reasons why women on cognitive routine tasks respond to the retirement age increase reform so much. First, the cognitive routine tasks stand for correcting, calculating and accounting. For example, according to Dengler et al. (2014), the top 4 occupations within this category are chemical laboratory workers, radio operators, data entry operators, and telecommunications mechanics and craftsmen. The occupations in offices and secretariats are very common among the women; hence, the large employment increase. Second, according to Dengler et al. (2014), the high skilled individuals usually perform analytical non-routine tasks, medium skilled individuals mainly perform cognitive routine tasks, while the low-skilled individuals perform manual non-routine tasks. Hence, it seems that by skill level required for each occupation type, I receive logical results. Next, the cognitive tasks can be further broken down by analytic and interactive tasks, which could hide some heterogeneity. Finally, despite that automation is taking place rapidly, it could be that cognitive routine tasks are relatively slower affected than the manual routine tasks.

To conclude, it seems that there is substantial heterogeneity by occupational task categories. In particular, workers in the occupations which are described by tasks that require higher skill level increase their employment more than those in low-skilled occu-

[17] I keep the classification for 2013.

pations (i.e. in manual routine tasks). The results also support the classification of tasks into complements vs substitutes by automation, with one exception of cognitive routine tasks.

Figure 7: Subsample analyses for the effect of the ERA increase on employment at age 60-62 by task type



Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. I perform subsample analyses by 5 task type categories. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.5 in the Appendix.

5.5 Additional mechanisms behind labor supply increase.

There are several additional mechanisms which can contribute to the understanding of how workplace characteristics affect the employment at 60-62 years old in response to the reform. All of the variables below are defined when the worker was 58 years old.

Tenure. The literature studying worker substitutability, such as Jäger and Heining (2022), also focus on tenure as a measure of skills. In the setting of retirement reform, tenure may not be a good measure of worker substitutability as it counts towards eligibility for retirement pathways. Hence, workers that are less tenured may have larger incentive to be employed than those with high tenure. I define tenure as a dummy above vs below the median value. The result shows that less tenured workers are more likely to increase their employment (see Figure A.7 in the Appendix.)

Full-time vs part-time. I also define full-time and part-time dummies at the age 58. Table B.9 shows that the workers in part-time jobs increase their employment more than those in the full time jobs. In line with the result that the ERA increase led to the higher marginal part-time work and partial retirement claims relative to the pre-reform level in Figure 3, the higher employment at the part-time job could be a coping mechanism of workers to the increase of retirement age. While I do not observe the working hours in this setting, the result indicates that the intensive margin of labor supply effect of the reform may be smaller than the extensive margin.

Female/male dominated occupations and establishments. Since the 1999 reform primarily affected women, one important question is whether the effect of the ERA increase is different dependent on gender domination of occupations or establishments. I follow Tophoven et al. (2015) and define gender integrated occupations or establishments as those where the the proportion of men and women ranges from 21% to 79%. Gender dominated occupations or establishments are those where the share of one of the genders exceeds 80%. I define this variable at 46 years old.

I find that women in female-dominated occupations increase their employment at 60-62 years old more than those in gender integrated occupations (see Figure A.8). [18] Hence, if men and women are imperfect substitutes, then the female-dominated occupations should have had smaller employment response, but I do not find it. One candidates for explanation is that men and women are quite substitutable, but it might be more difficult for women to compete for employment at the older age with men. Hence, I do not classify this dimension of analyses as a "worker substitutability" in the main section above.

Environment and culture An interesting question is whether the women with Eastern German vs Western German origin respond to the retirement reform differently, as Eastern German women have traditionally higher attachments to the labor market (see Table B.7 which proves that Eastern German women increase their employment more).

Worker and establishment fixed effects. I merge my data with the variables showing the position in the distribution of worker (establishment) fixed effects in the period 2003-2010 described in Bellmann et al. (2020) by using worker (establishment) identifier. In particular, I create 5 categories of quantile ranges: quantiles 1-20, 21-40, 41-60, 61-80, and 81-100 for both worker fixed effects (WFE) and firm fixed effects (FFE). The subsample

[18] The same measure for establishment gender composition gives less significant differences.

analyses based on worker and establishment fixed effects (which proxy the more able workers and higher paying establishments) reveal a U-shaped pattern in employment (see Figure A.9). Such result could show that the lower paid workers are more constrained due to the ERA increase due to their liquidity constraints. Meanwhile the highest paid workers increase the retirement age due to their irreplaceable nature and high skills, which is confirmed by other subsample analyses in the section above. Hence, the most flexible in responding to retirement reform are those in the middle of the WFE and FFE distribution, as they neither are in need of work nor their employers depend on their skills.

Establishment size. I define 3 main establishment size categories- small (up to 19 workers), medium (20-249 workers), and large (250-999 workers). The biggest employment increase happens in the big firms (see Figure A.10).

Industry. I define industries by the mapping based IAB establishment panel, following the procedure described in Dauth and Eppelsheimer (2020). The subsample analyses by industries reveals that the highest employment increase is attributable to the food and beverage, agriculture, and construction industries, while the smallest effects are driven by the manufacturing, education, and transportation industries (see Table B.11).

Type of work contract. Next, I specify the types of work contracts. I find that the employment increase is largely attributed to the workers on regular jobs (i.e. not on temporary jobs via an employment agency contracts), and on part-time work (see Table B.9). The employment relationship (fixed term vs permanent jobs) do not yield differential employment effects due to the ERA increase.

5.6 Robustness and falsification checks for RDD.

Balance checks: continuity of pre-determined covariates at the cutoffs.

The Table B.13 shows that there is no significant discontinuity for pre-determined variables. In particular, I choose Western origin and nationality variables, as these variables are fixed over time.

Balance checks: continuity of heterogeneity variables around the cutoff.

Since I perform many subsample analyses throughout this study, I also check whether the main subsample variables are continuous at the cutoff. The Table B.14 shows that all the subsample variables except for human capital specificity and tasks jump at the cutoff. Hence, I redefine the ELMT variable at the age 46 and do the subsample analyses by that same variable. I receive the same result- the workers in thinner external labor markets are more likely to increase employment then those in the thick ones, but my results are less significant, as the market thickness could change over the 12 years. Since the task type is already defined at 46 years old, I cannot make a robustness check further. And the specificity of human capital is not continuous when both defined at 46 and 58 years old; hence, I do not perform robustness check either.

Sensitivity to inclusion of covariates.

Table B.15 reports an RDD regression controlling for firm fixed effects in 1985-1992 and 19993-1999. The inclusion of covariates does not alter the treatment affects; however, leads to better precision of estimates.

Placebo cutoffs.

Finally, I perform falsification tests by using placebo cutoffs- I check whether employment at 60-62 years old jumps for women at the other birth cohort cutoffs, which were not affected by the reform. [19] I use the cutoffs corresponding to January 1947, January 1948, January 1949, January 1950, and January 1951, as women were eligible for women's pathway at these cutoffs. The Table B.17 shows that all the placebo cutoffs yield insignificant effects (p¿ 0.05)

Sampling.

Since in my baseline results I pooled together the establishments in originally sampled firms together with all the establishments observed (due to worker mobility), I also perform subsample analyses by those 2 establishment types, and find that when focusing on the originally sampled establishments, I receive just slightly larger employment effects than in the baseline results discussed in sections above (see the last panel in Table B.9).

6 Employers' adjustment of retention of focal workers, coworker promotions and external hiring

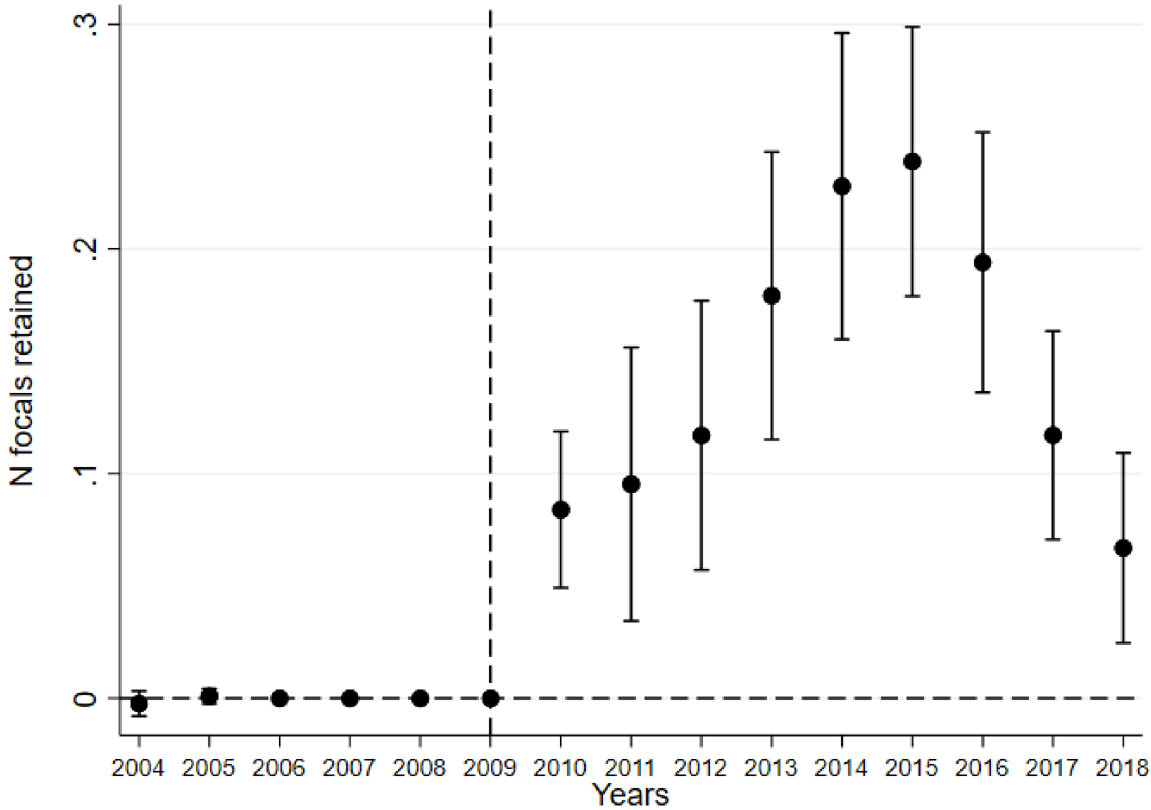
This section consists of 3 parts. First, it documents positive effect on retention of focal workers. Second, it shows negative spillover effects on promotions and external hiring associated with one additional treated worker employed in 2009. In particular, I document the effect on retention, promotions, and external hiring (i) by years (Equation 2), (ii) then I proceed to show the targeted hiring and promotion in a DiD framework (Equation 3). Last but not least, after revealing that the middle aged women bear most of the negative spillovers, I perform heterogeneity analyses by substitutability measures only on them.

[19] Table B.16 shows the RDD around 1952 cutoff for male workers. Despite that the male workers were affected by the 1999 reform to a lesser extend then women (due to abolishment of other pathways), they do not constitute an ideal setting placebo group, because if they were on the regular pathway to retirement, their NRA could increase by 1 month around the cutoff, so at the ages 60-62 they could increase their employment as a forward-looking approach towards the retirement after 65 and 5 months vs 65 and 6 months. Still, I report the results, and as expected there is a discontinuity in the employment at 60-62 years old, but very small in magnitude.

6.1 Positive effects on retention of focal workers

Figure 8 shows that having an additional treated (born in 1952-1953) focal worker in 2009 leads to 0.08 till 0.25 additional focal worker (born in 1950-1953) retentions. This result is important, as it shows that the treated focal workers stayed at the establishment after reaching 60 years old- the pre-reform ERA level.

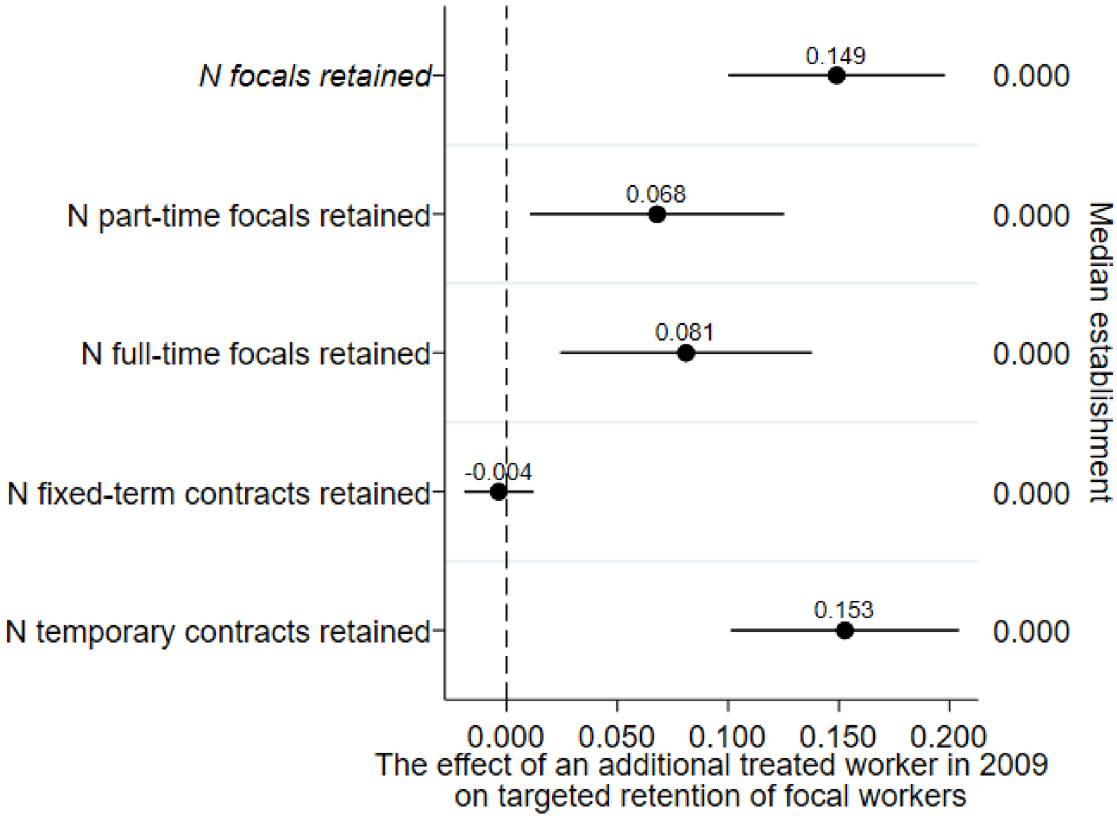
Figure 8: The effect of an additional treated worker on retention of focal workers



Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of focal worker (born in 1950-1953) retention in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

Next, I study who gets retained. The Figure 9 shows the targeted retention of focal workers in the years 2012-2015. The first row shows that an additional treated worker leads to 0.149 more retention. While there is not much difference between the part-time or full-time retention (46% and 54%, accordingly); disentangling the total retention by contract type shows that it is mainly driven by focal workers on temporary contracts (1 treated worker leads to 9.153 additional retention of focal workers on temporary contracts). Such result goes in line with general expectations as temporary contracts do not have an ending date, making such workers more binding to the establishments. (for more generalized-DiD plots see the top panel of Figure A.12.)

Figure 9: The effect of an additional treated worker on total and targeted retention of focal workers

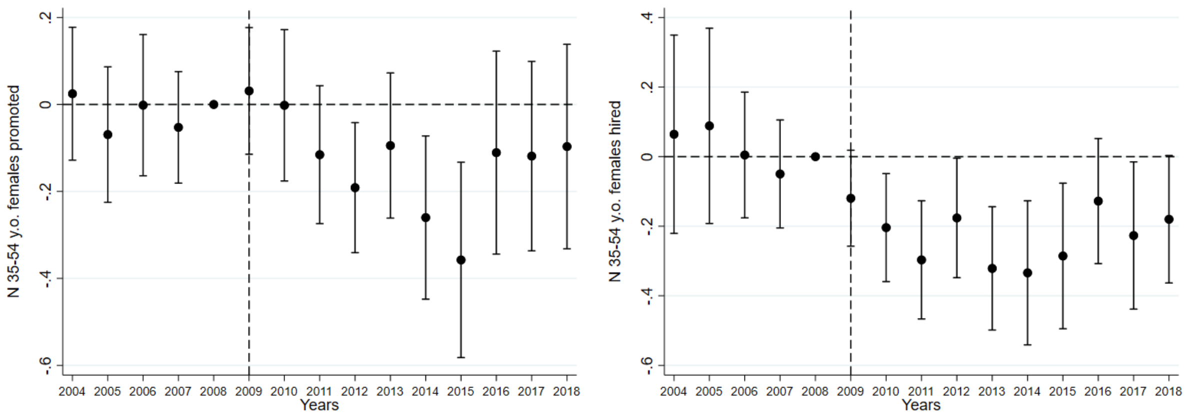


Notes: Coefficient plots. Each row corresponds to the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of the total and targeted retention outcomes (left axis) of focal workers (born in 1950-1953) in the years 2012-2015. The points represent the estimated coefficients β_P in Equation 3 and the bars represent the 95% confidence intervals. The "median establishment" (right column) corresponds to the medians of the outcome variables in the corresponding category of outcomes. The standard errors are clustered at the establishment level.

6.2 Negative effects on promotions and hiring

Figure 10 shows that having an additional treated worker in 2009 leads to negative effects on the number of promotions (left panel) and external hires (right panel) of middle-aged women. This gender-age category bears most of the negative effects of retirement age increase spillovers (the more detailed Figure A.14 shows that there are bigger negative spillovers on the middle aged workers and women than their young and male counterparts). Having one additional treated worker in 2009 leads to 0.205 less middle-aged women's promotions, and 0.348 less middle-aged women's external hires in years 2012-2015 (see the 6th row in Figure 11).

Figure 10: The effect of an additional treated worker on promotions and external hiring of middle-aged women

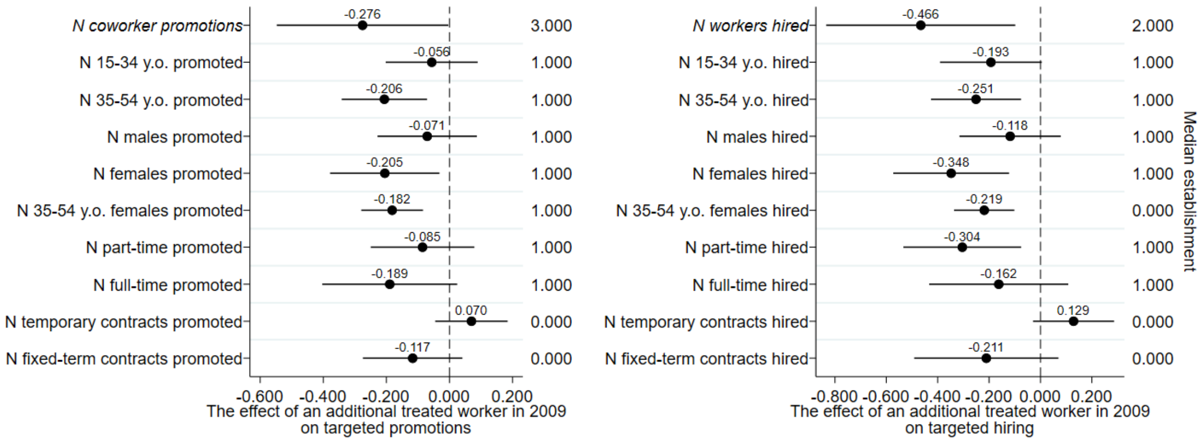


Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of middle aged workers promoted (left panel) and hired (right panel) in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

The overall negative effects amount to -0.206 middle-aged workers promoted and -0.251 hired (see Figure 11). In particular, the middle-aged workers are bearing most of the negative effects on promotions (amounting to 75%, see the 3rd row of the left panel in Figure 11). Such result indicates that the 1999 retirement age increase leads to blocking the career ladders of the next substitutes by age. Given the career blocks, one follow-up question is whether the coworkers leave such employers for other employers. The Figure A.11 shows that while the focal workers do not switch their employers after reaching the age of 60, there is a small positive likelihood of separations of coworkers associated with and additional treated worker. Such result further illustrates that some coworkers react to the negative effects on promotions by changing the employer.

When looking at the targeting by contract types (see last two rows of Figure 11), I find that the workers on fixed term contracts are bearing the negative effects. There are 2 possible reasons behind this result. First, the employers understand that the focal workers will leave one day; hence, they do not decrease the promotions and hires of temporary contracts (with no end date), but do so for the fixed ones as they are more costly. Second, it is useful for the employer to have the focal workers share the firm-specific skills with temporary workers (with no end date on contract), as they stay longer relative to the fixed-term contractors.

Figure 11: The effect of an additional treated worker on total and targeted promotions and external hiring



Notes: Coefficient plots. Each row corresponds to the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of the total and targeted promotion (left panel) and hiring (right panel) outcomes (left axis) of focal workers (born in 1950-1953) in the years 2012-2015. The points represent the estimated coefficients β_P in Equation 3 and the bars represent the 95% confidence intervals. The "median establishment" (right column) corresponds to the medians of the outcome variables in the corresponding category of outcomes. The standard errors are clustered at the establishment level.

Additional results on targeted hiring in Table B.23 show that the workers that the largest proportion of lost hires holds a vocational degree (column 2), is not a first time worker (column 4), and is a foreigner (column 6, significant at 10%).

6.3 The role of worker substitutability in negative effects on coworkers and external hiring

TBA: disclose and paste the subsample analyses by:
ILMT,
ELMT,
routinness of the firm,
gender domination of the firm

6.4 Additional mechanisms behind negative effects on coworkers and external hiring

TBA: disclose and paste the subsample analyses by:
east,

growing,
Firm FE,
establishment size categories,
industry

6.5 Robustness and falsification checks for generalized DiD

Anticipation

One threat to the identification strategy could be that the employers start to adjust their labor force before 2009, as the reform was announced in 1999 and there was a large period after the reform announcement and enaction. In such case, constructing the treatment variables as I do in my baseline regressions- in 2009 (a year before all the focal workers reach the retirement age under the pre-reform rules) might not be a "shock" to the establishments, opposed to constructing the variables in the pre-reform announcement year 1998. In Table B.27 I show that the baseline results (Panel A) are similar in sign to the alternative definition of treatments in 1998 (Panel B); however the effect on hired workers is significant only at the 10% level, while the effect on retention of focal workers is smaller, likely due to a weaker connection between the treatments and outcomes due to the bigger time frame). Interestingly, the effect on promotions is bigger in magnitude

Cyclical trends in outcome variables based on treatment

If the outcome variables display cyclical trends, then my outcomes will be sensitive to the way I define the *Post* and *Anticipation* variables described in section 4. Hence, I check if changing the main effect time frame (in baseline regressions above, *Post* stands for the years 2012-2015) changes the results. In particular, I redefine the time frame of variable *Post* to include 2013-2016 (Panel A in Table B.28), 2011-2014 (Panel B in Table B.28). As expected, the magnitude of the coefficients changes (as these years are less likely to cover all the years when the focal workers are 60-62 years old), but the signs of effects stay the same.

7 Conclusion

In this paper I quantify the employment effects of the 1999 reform that abolished the women's pathway to retirement and study the mechanisms of the corresponding employment increase of affected workers, as well as spillovers on coworkers and external hires by worker substitutability and skills. Overall, I contribute to the seminal paper by Geyer and Welteke (2021) by studying a new mechanism in the literature that studies the labor supply response due to the retirement age increase - the worker substitutability and frictions in replacing workers. In line with Jäger and Heining (2022), I construct measures for labor market thicknesses and specialized skills, and show that they matter for the employment decisions at the older age. In particular, I find that being employed at the firms in thin internal and external markets, where the worker turnover is particularly costly, and in occupations with specific skills leads to a larger increase of employment at 60-62 years old. The spillover results show a nengative effect on coowrkers and external hires, in particular the middle-waged women.

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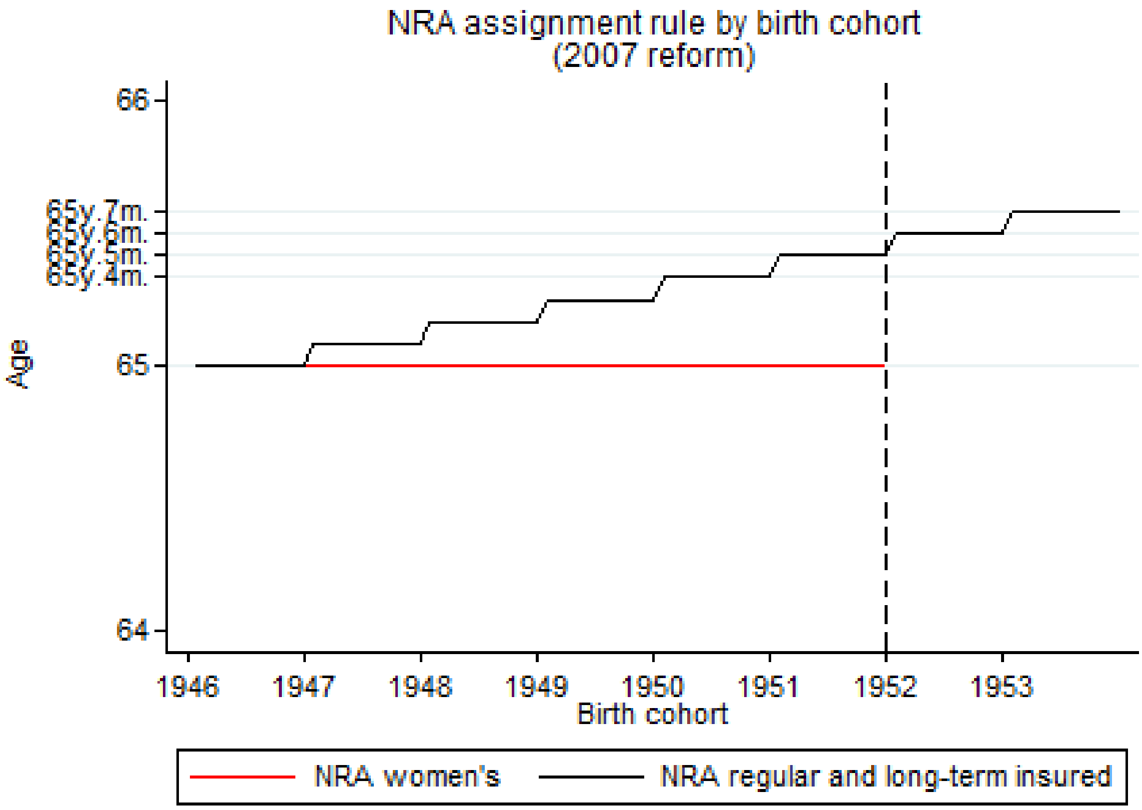
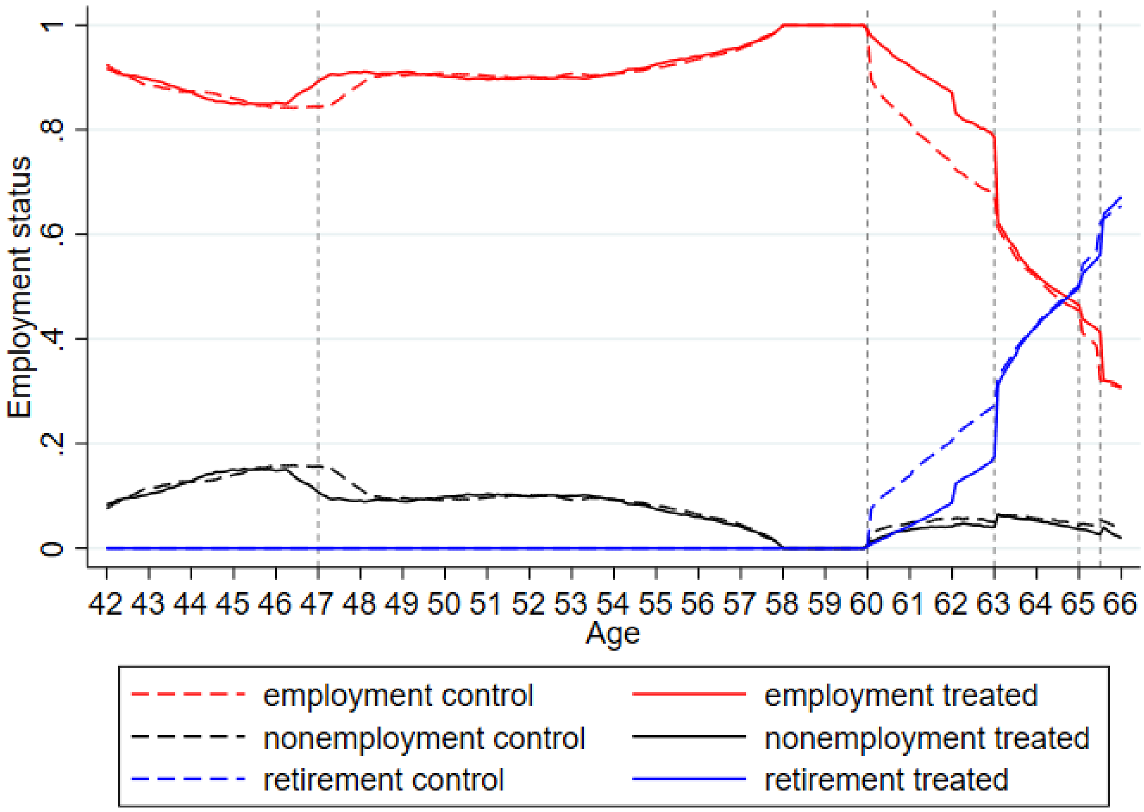
A Appendix figures

Figure A.1: The treatment assignment of 2007 reform



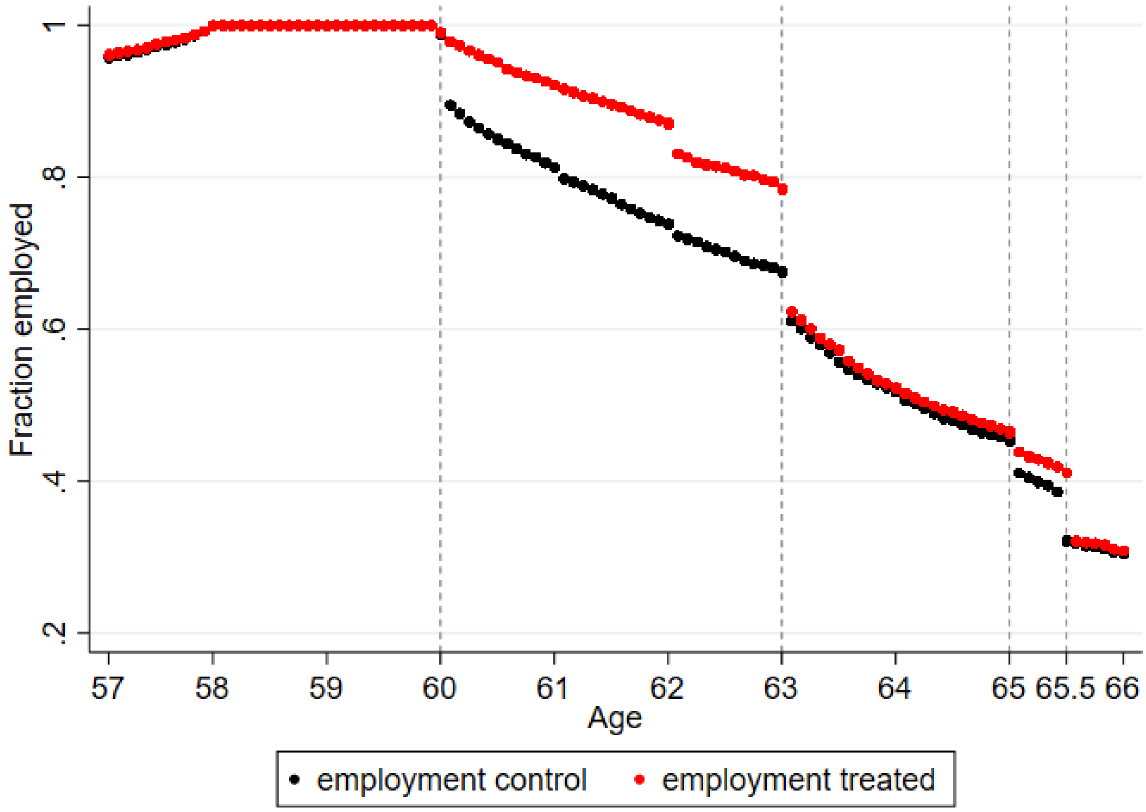
Notes: this figure represents the assignment rule of normal retirement age by birth cohorts. Before the 1952 cohort, there was the women's pathway to retirement (red line). The vertical dashed line at the January 1952 cohort indicates the birth cutoff starting from which the women's pathway was abolished. Starting from 1952 cohort, the NRA for people eligible for the regular pathway to retirement is equal to the NRA for long-term insured, which used to be 65 years old, but increased by monthly increments per birth year starting from the cohort 1947 (black line).

Figure A.2: Fraction of women employed, nonemployed, and retired at each age-month by treatment and control group



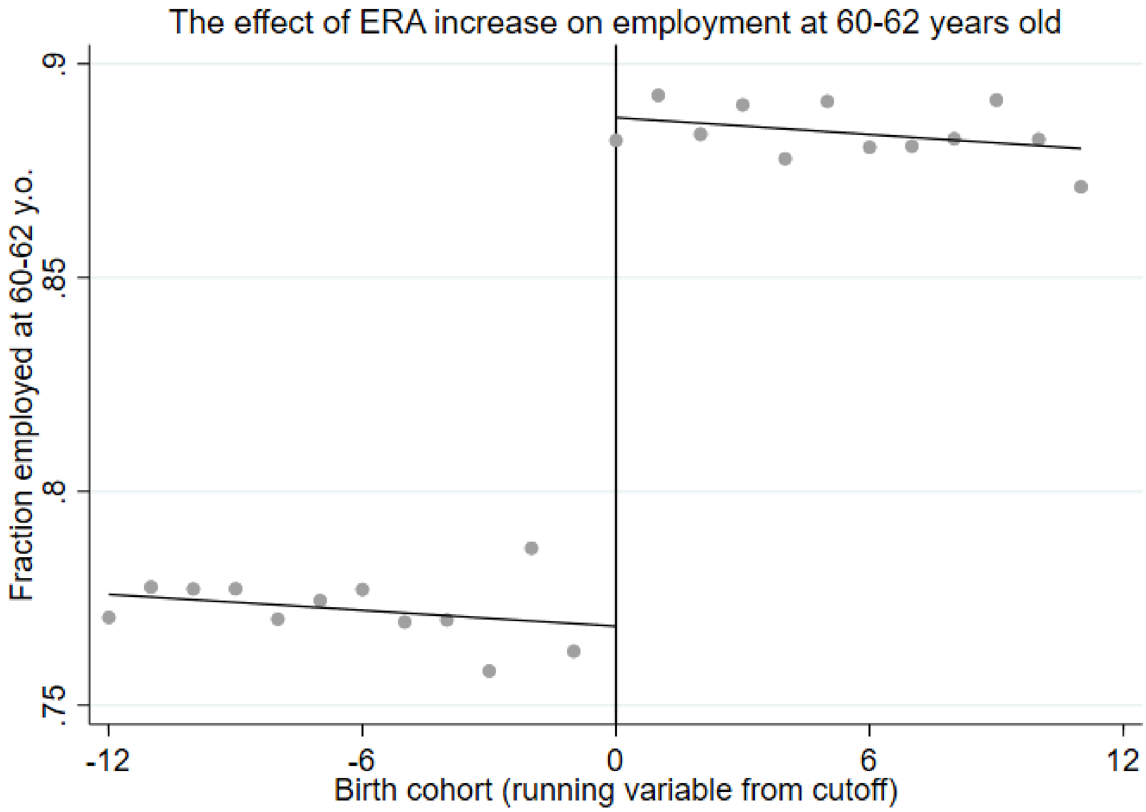
Notes: this figure displays the evolution of 3 main employment states (*employment* in red, *nonemployment* in black, and *retirement* in blue- see section 4 for more details) over age by treatment status: (i) *treated* - women born in 1952 (solid lines), and (ii) *control*- women born in 1951 (dashed lines). The first short-dashed vertical line (at age 47) corresponds to the age of the 1st treated cohort when the reform was announced in 1999. The next two short dashed vertical lines stand for the time frame between the old ERA scheme (at age 60) and the new one (at least age 63) per 1999 reform, while the last 2 short-dashed vertical lines stand for the old NRA scheme (at age 65) and the new one (at age 65 years and 6 months) per 2007 reform.

Figure A.3: Fraction of women employed at each age-month by treatment and control group



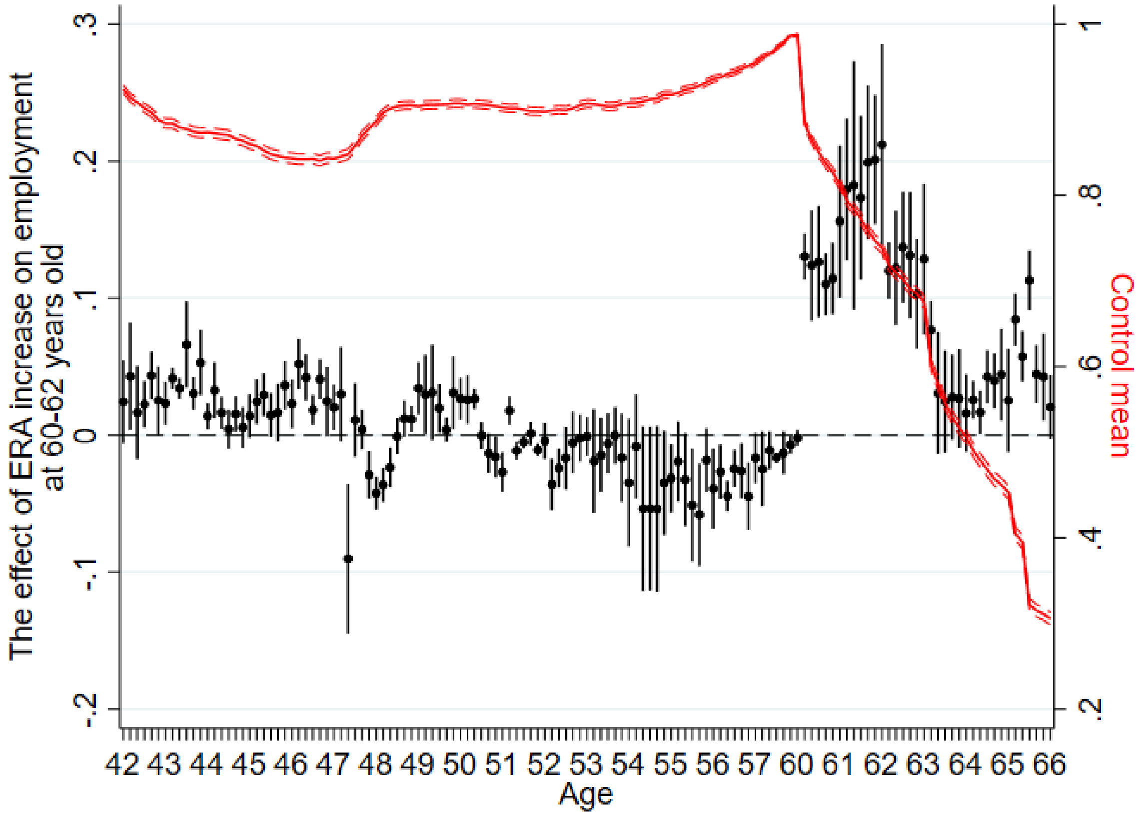
Notes: This figure displays the fraction of women employed at each age month by 2 treatment statuses: treated (the 1952 birth cohort) and control (the 1951 birth cohort). The period between the 2 dashed lines at 60 and 63 years old is the one when the gaps between the 2 groups exist.

Figure A.4: The effect of ERA increase: RDD plot



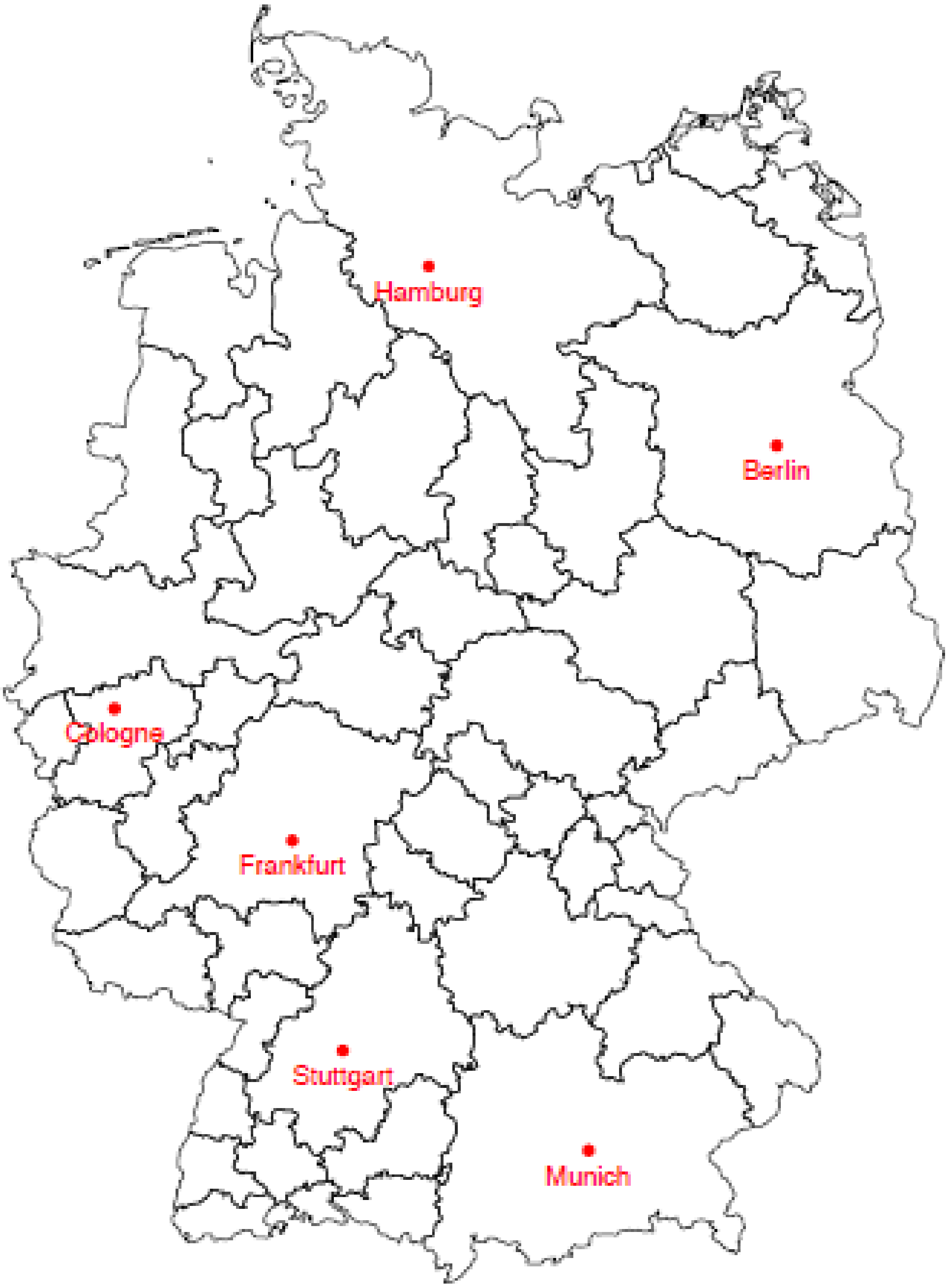
Notes: RDD regression of the share of employed at age 60-62 around the 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square-based optimal bandwidth selection procedure. Vertical line marks the birth cohort threshold 1952 (e.g. 0 corresponds to January 1952, -6 corresponds to people born 6 months before- on June 1951).

Figure A.5: RDD by age in months



Notes: Coefficient plots. Each vertical line corresponds to the RDD regression of the share of employed at age 60-62 around the 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. The points represent the estimated robust coefficients and the bars represent the 95% confidence intervals. The standard errors are clustered at the birth month level. The red solid line represents the control mean, while the red dashed lines represents the confidence intervals for the control means.

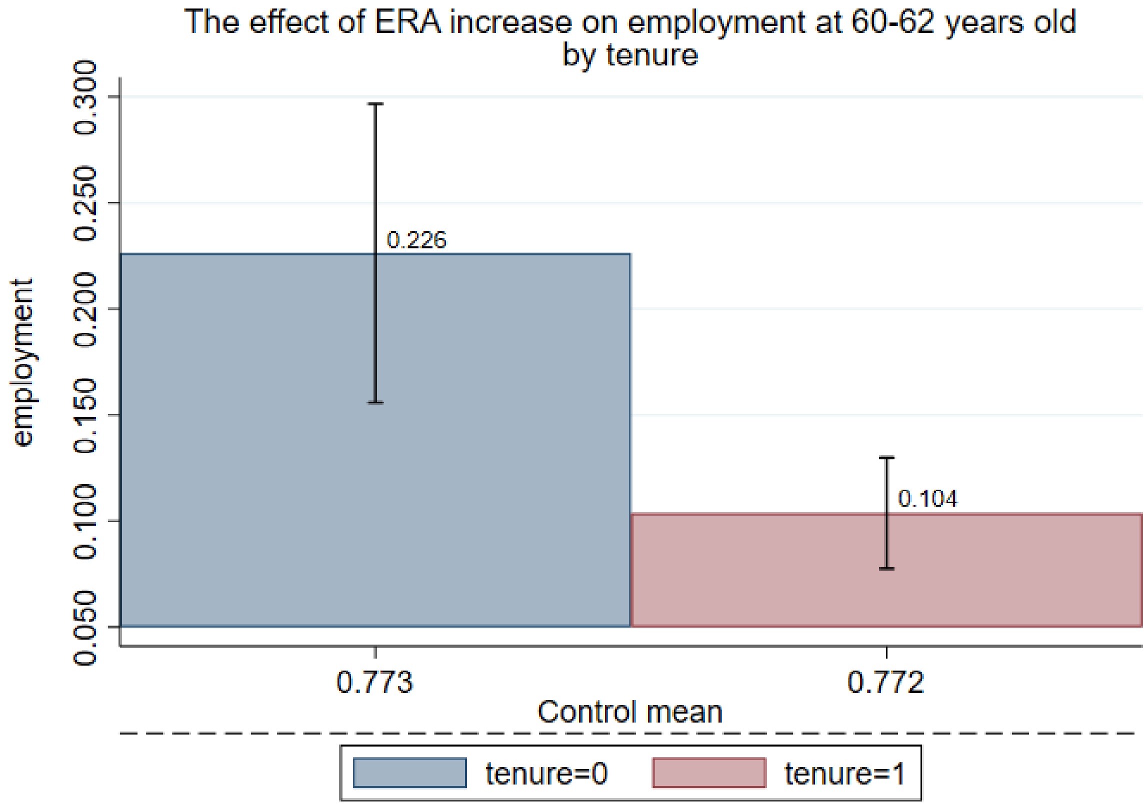
Figure A.6: Labor market regions in Germany



Notes: This map shows German labor market regions based on Kropp and Schwengler (2011) classification. In particular, the 50 labor market regions are constructed by high within region commuting, and low between region commuting.

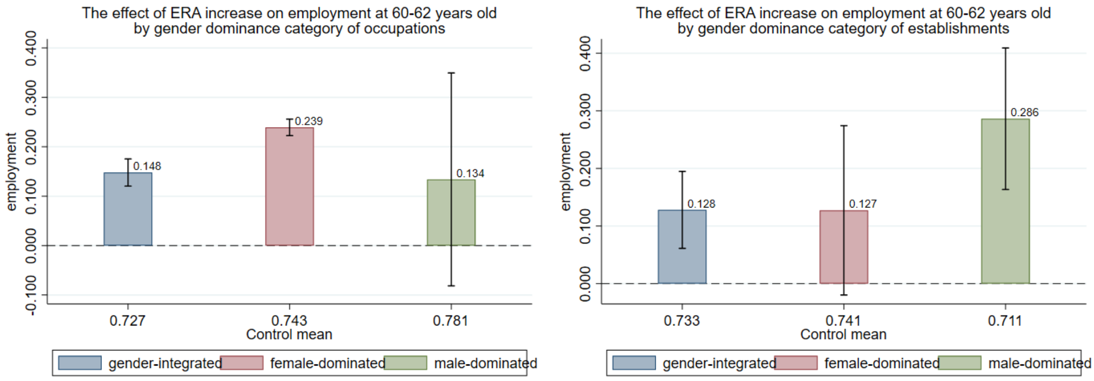
Source: Jäger (2016)

Figure A.7: Subsample analyses for the effect of the ERA increase on employment at age 60-62 by tenure



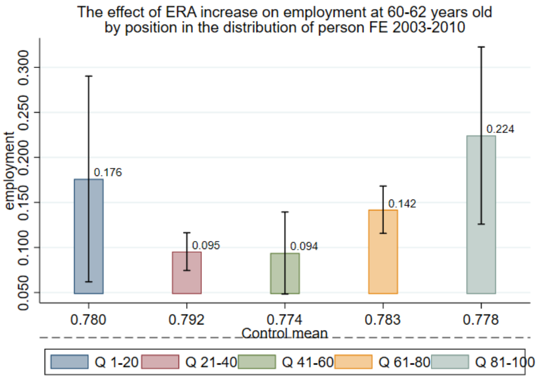
Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.2 in the Appendix.

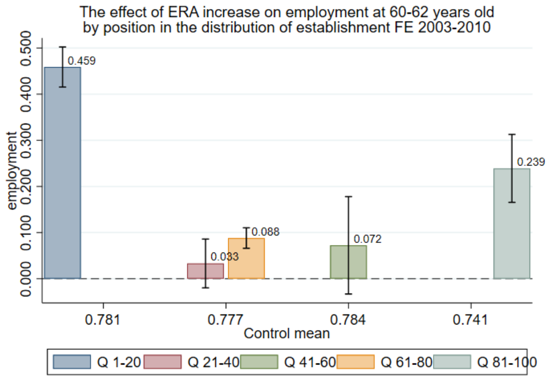
Figure A.8: The effect of the ERA increase on employment at age 60-62 by gender-composition of occupations



Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age 46, and education. The subsample analyses is performed by gender dominance of occupations. Integrated occupations are defined as those where the proportion of men and women ranges from 21% to 79%. Gender dominated occupations are those where the share of one of the genders exceeds 80%. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.6 in the Appendix.

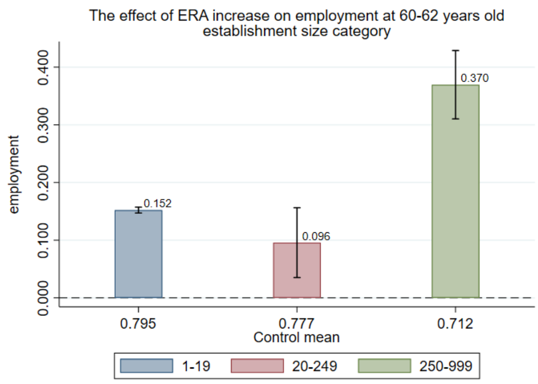
Figure A.9: The effect of the ERA increase on employment at age 60-62 by worker ability and firm premium





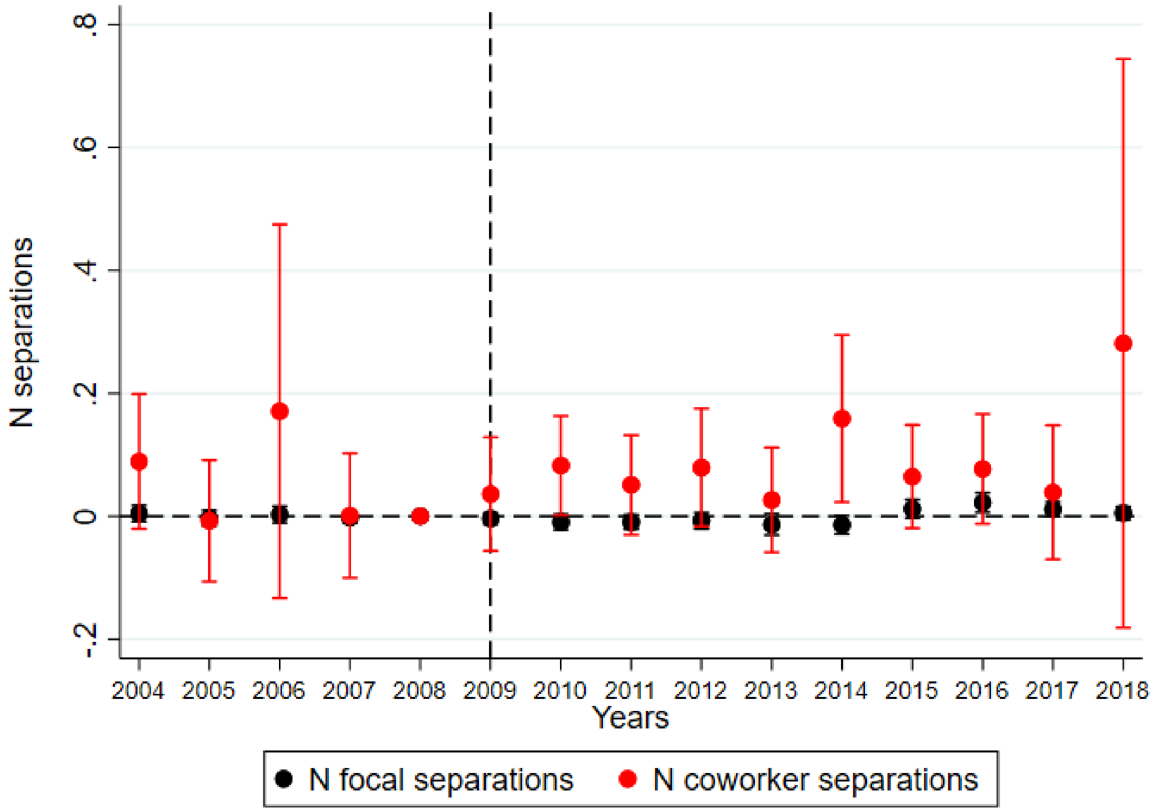
Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. I perform subsample analyses by 5 categories of the fixed effects based on quantile ranges: quantiles 1-20, 21-40, 41-60, 61-80, and 81-100 for both worker fixed effects (WFE) and firm fixed effects (FFE). The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.8 in the Appendix.

Figure A.10: The effect of the ERA increase on employment at age 60-62 by firm size



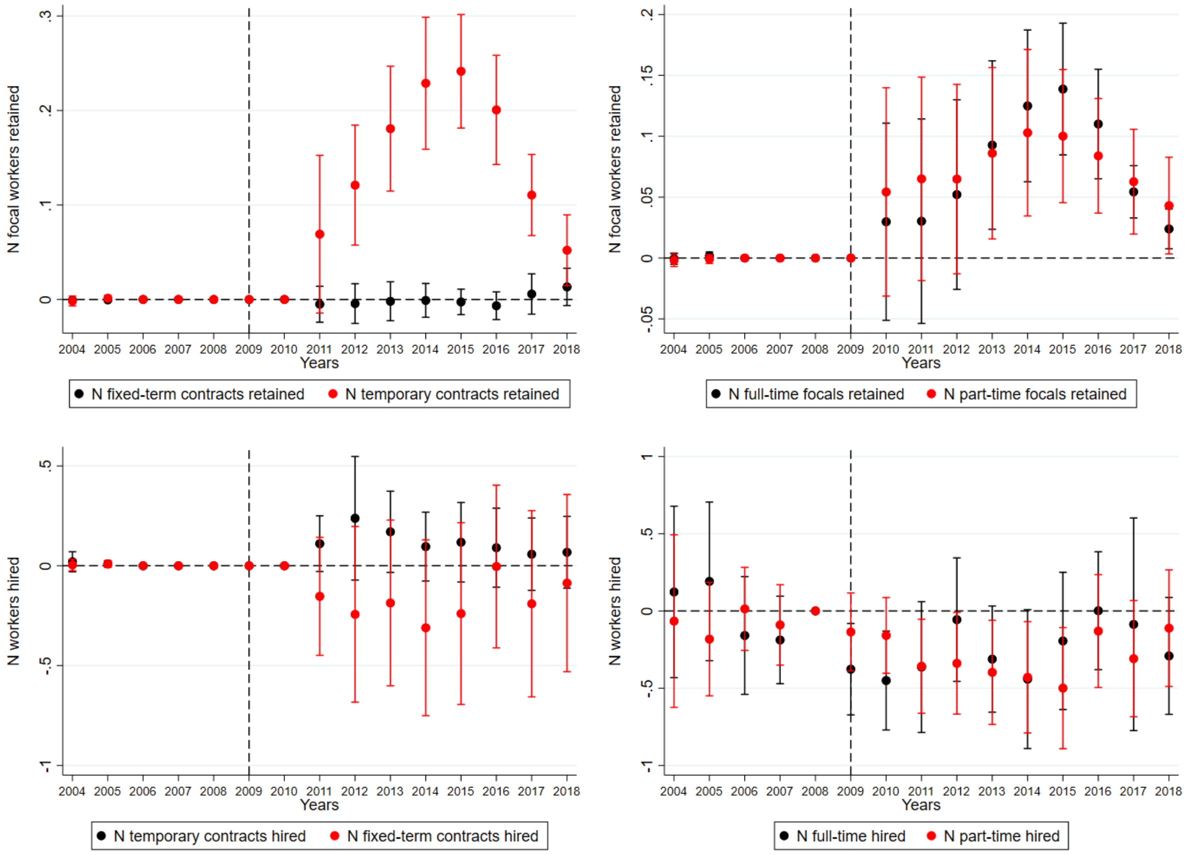
Notes: Coefficient plots for RDD regressions around 1952 cutoff. For computing the RDD estimates, I use first order polynomials, uniform kernel function, and mean square error-based optimal bandwidth choice. The 3 levels of firm size are (1) up to 19 workers, (2) 20-249 workers, and (3) above 25 workers employed at the firm. The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. The control means (on the x-axis) stand for the employment share at the age 60-62 in the corresponding subsample over the control group (born in 1951). The corresponding table with more details can be found in Table B.10 in the Appendix.

Figure A.11: The effect of an additional treated worker on separations of focal workers and coworkers



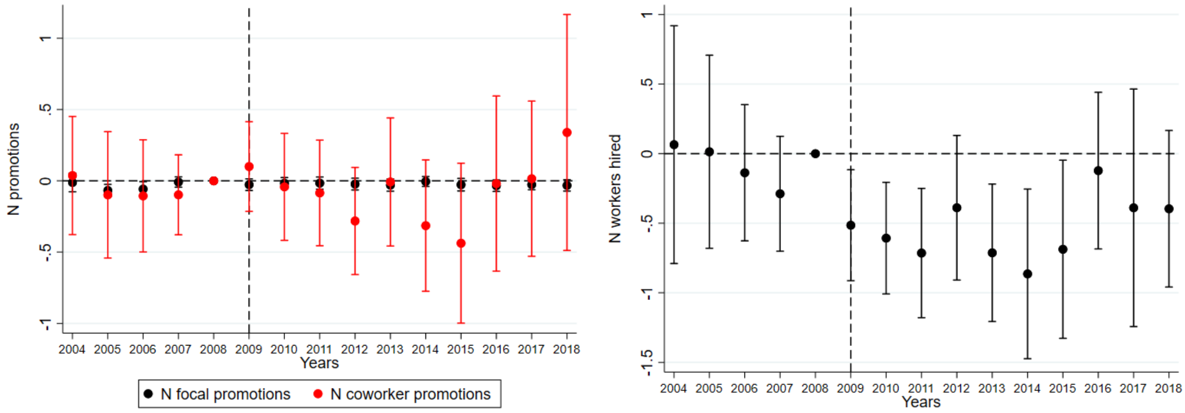
Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of focal worker (born in 1950-1953) separations in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

Figure A.12: The effect of an additional treated worker on retention of focal workers and external hiring by contract types and working hours



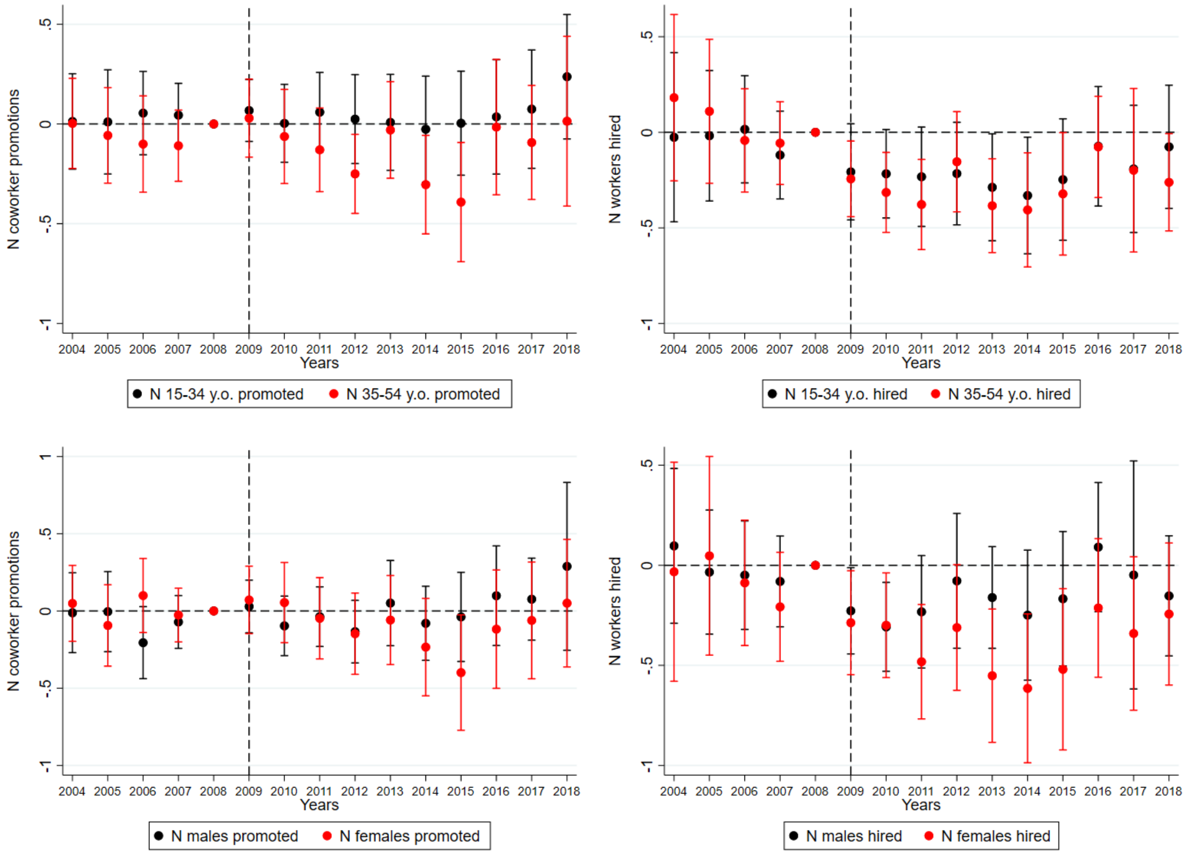
Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on (1) the number of focal workers (born in 1950-1953) retained (top panel) and number of hired workers (bottom panel) by contract type (left panel) and working hours (right panel) in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

Figure A.13: The effect of an additional treated worker on focal workers and coworkers' promotions and external hiring



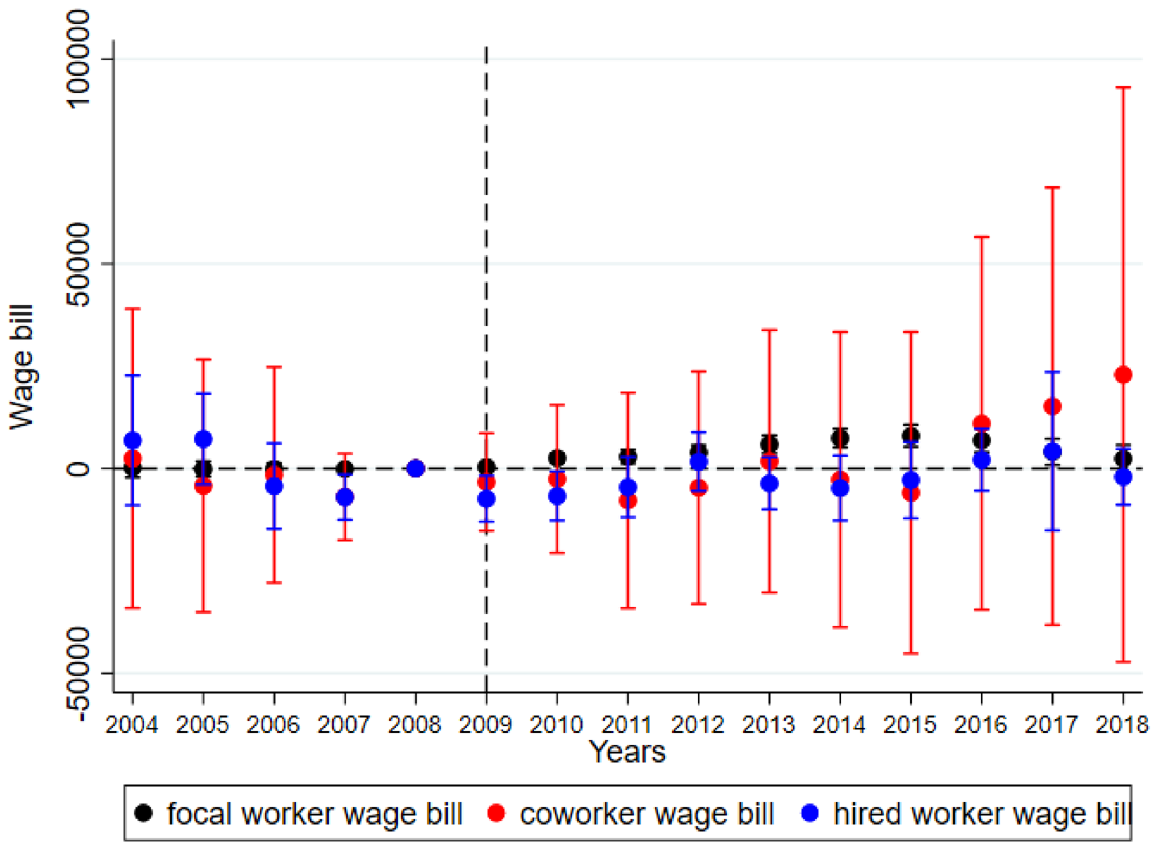
Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of focal workers (born in 1950-1953, in black) and promoted workers (in red) (left panel) and number of hired workers (right panel) in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

Figure A.14: The effect of an additional treated worker on coworkers' promotions and external hiring by age group and gender



Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the number of promoted workers (left panel) and number of hired workers (right panel) in each year by age (top panel) and gender (bottom panel). The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

Figure A.15: The effect of an additional treated worker on wage bills



Notes: this figure represents the effect of having 1 additional treated worker (born 1952-1953) in 2009 on the wage bill of focal workers (born in 1950-1953, in black), coworkers (in red), and hired workers (in blue) in each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent the 95% confidence intervals. The dashed vertical line represents the year before the policy enaction, i.e. when all the focal workers (born in 1950-1953) are below 60 years old. The standard errors are clustered at the establishment level.

B Appendix tables

Table B.1: The effect of ERA increase on employment outcomes at 60-62 years old

	(1) employ-ment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employ-ment	(6) retire-ment
ERA increase	0.175***	0.072***	0.027	0.048***	-0.022***	-0.148***
	(0.029)	(0.013)	(0.019)	(0.005)	(0.006)	(0.023)
Control mean	0.772	0.458	0.228	0.086	0.050	0.178
Observations	1179720	1179720	1179720	1179720	1179720	1179720
N workers	32770	32770	32770	32770	32770	32770

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which by at least 3 years (Equation 1). I pool all observations from the month after the 60th birthday to the 63rd birthd corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemploym* and *retirement* (column 6). *Employment* can further be decomposed into columns 2-4. I use a uniform kernel functi square error-based optimal bandwidth choice. I control for a calendar month, a dummy for Western residence, wages a and education. The control means are the average values of the outcomes when I limit the sample to women born in 19 group). Robust standard errors in parenthesis are clustered at the birth-month level. The corresponding coefficient plot Figure 3.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.2: The effect of increased ERA on employment at 60-62 years old by measures of worker skills

	(1)	(2)
Panel A: human capital specificity of occupation		
	low specificity	high specificity
ERA increase	0.141***	0.258***
	(0.009)	(0.041)
Control mean	0.776	0.719
Observations	154620	140364
N workers	4295	3899
Panel B: managerial occupation		
	not a manager	manager
ERA increase	0.171***	0.321***
	(0.025)	(0.104)
Control mean	0.760	0.793
Observations	999684	14832
N workers	27769	412

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. **Panel A** is performed by "*HK specificity*"- which stands for human capital specificity of occupation. It is based on the return of experience in Mincer equations performed separately for each of the 3-digit occupations. Then I create a dummy variable based on a median split across all the occupations. **Panel B** stands for managerial status, which is created as a dummy from the last 2 digits of the 5-digit occupational variables. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level. The corresponding coefficient plot can be found in **??**.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.3: The effect of increased ERA on employment at 60-62 years old by substitutability measures

	employment	
Panel A: internal labor market thickness		
	thin	thick
ERA increase	0.220***	0.158***
	(0.017)	(0.006)
Control mean	0.743	0.764
Observations	148428	143064
N workers	4123	3974
Panel B: external labor market thickness		
	thin	thick
ERA increase	0.217***	0.136***
	(0.008)	(0.031)
Control mean	0.764	0.744
Observations	146736	142848
N workers	4076	3968

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60$^{\text{th}}$ birthday to the 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. **Panel A** shows subsample analyses by *internal labor market thickness (ILMT)*, based on the median split of the biggest occupation employment over the total workforce employment in the establishment. **Panel B** shows subsample analyses by *external labor market thickness (ELMT)*, based on the median split of the local labor market's share of industry employment over the national share across all of the industries. If these variables are equal to 1, it means that their value is above their median value. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level. The corresponding coefficient plot can be found in Figure 5.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.4: The effect of ERA increase on employment outcomes at 60-62 years old by labor market interactions

	(1) thick ILMT , thick ELMT	(2) thick ILMT , thin ELMT	(3) thin ILMT , thick ELMT	(4) thin ILMT, thin ELMT
ERA increase	0.256***	0.102	0.145***	0.154***
	(0.023)	(0.063)	(0.019)	(0.016)
Control mean	0.770	0.720	0.759	0.777
Observations	67788	80352	78948	62496
N workers	1883	2232	2193	1736

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses based on interactions between ILMT and ELMT. The *internal labor market thickness (ILMT)* is based on the median split of the biggest occupation employment over the total workforce employment in the establishment. The *external labor market thickness (ELMT)* is based on the median split of the local labor market's share of industry employment over the national share. If these variables are equal to 1, it means that their value is above their median value. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level. The corresponding coefficient plot can be found in **??**.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B.5: The effect of increased ERA on employment by task type

	(1) analytic non-routine	(2) interactive non-routine	(3) cognitive routine	(4) manual routine	(5) manual non-routine
ERA increase	0.221***	0.208***	0.229***	0.030	0.041***
	(0.029)	(0.033)	(0.041)	(0.025)	(0.010)
Control mean	0.768	0.763	0.752	0.760	0.773
Observations	91800	189000	423072	85464	212148
N workers	2550	5250	11752	2374	5893

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses in five task-type categories. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure 7.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B.6: The effect of increased ERA on employment at 60-62 years old by gender domination

	employment		
Panel A: gender domination in occupation			
	gender-integrated	female-dominated	male-dominated
ERA increase	0.148***	0.239***	0.134
	(0.014)	(0.009)	(0.110)
Control mean	0.727	0.743	0.781
Observations	132912	59004	11556
N workers	3692	1639	321
Panel B: gender domination in establishment			
	gender-integrated	female-dominated	male-dominated
ERA increase	0.128***	0.127*	0.286***
	(0.034)	(0.075)	(0.063)
Control mean	0.733	0.741	0.711
Observations	119808	64440	15012
N workers	3328	1790	417

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60th birthday to the 63rd birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. The subsample analyses are performed by *gender dominance* of occupations (**Panel A**) and establishments (**Panel B**). *Gender-integrated* occupations and establishments are defined as those where the proportion of men and women ranges from 21% to 79%. *Gender-dominated* occupations/establishments are those where the share of one of the genders exceeds 80%. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure A.8 in the Appendix.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.7: The effect of increased ERA on employment at 60-62 years old by demographic characteristics of employees

	employment		
Panel A: residence of living			
	East	West	
ERA increase	0.153***	0.105***	
	(0.013)	(0.013)	
Control mean	0.726	0.784	
Observations	228168	949392	
N workers	6338	26372	
Panel B: residence of origin			
	East	West	
ERA increase	0.154***	0.104***	
	(0.014)	(0.012)	
Control mean	0.728	0.783	
Observations	232776	945756	
N workers	6466	26271	
Panel C: education			
	high school	vocational	university
ERA increase	0.126***	0.110***	0.138***
	(0.009)	(0.011)	(0.016)
Control mean	0.782	0.772	0.789
Observations	163620	971568	155340
N workers	4545	26988	4315

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. **Panel A** performs subsample analyses by the residence of the workers (dummy variable); **Panel B** splits the workers by Eastern and Western origin, proxied by the place of residence of the first worker as observed in the employment biography; and **Panel C** splits the sample by educational categories. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.8: The effect of increased ERA on employment at 60-62 years old by worker and establishment fixed effects

	employment				
Panel A: worker fixed effect quantile range categories					
	Q 1-20	Q 21-40	Q 41-60	Q 61-80	Q 81-100
ERA increase	0.176***	0.095***	0.094***	0.142***	0.224***
	(0.058)	(0.011)	(0.023)	(0.013)	(0.050)
Control mean	0.780	0.792	0.774	0.783	0.778
Observations	180504	116748	100548	144036	140220
N workers	5014	3243	2793	4001	3895
Panel B: establishment fixed effect quantile range categories					
	Q 1-20	Q 21-40	Q 41-60	Q 61-80	Q 81-100
ERA increase	0.459***	0.033	0.072	0.088***	0.239***
	(0.022)	(0.027)	(0.054)	(0.011)	(0.038)
Control mean	0.781	0.777	0.784	0.777	0.741
Observations	57660	131670	172956	276066	330005
N workers	1655	3807	4905	7827	9410

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by 5 categories of the fixed effects based on quantile ranges: quantiles 1-20, 21-40, 41-60, 61-80, and 81-100 for both *worker fixed effects* (WFE, **Panel A**) and *firm fixed effects* (FFE, **Panel B**). The vertical lines indicate 95% confidence intervals based on robust standard errors clustered at the birth month level. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level. The corresponding coefficient plot can be found in Figure A.9 in the Appendix.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.9: The effect of increased ERA on employment at 60-62 years old by additional subsamples

	employment	
Panel A: temporary job via an employment agency		
	no temporary agency work	temporary agency work
ERA increase	0.203***	0.134***
	(0.030)	(0.051)
Control mean	0.865	0.810
Observations	1064515	10433
N workers	26754	260
Panel B: employment relationship (fixed-term vs permanent)		
	permanent	fixed-term
ERA increase	0.197***	0.197***
	(0.030)	(0.028)
Control mean	0.869	0.799
Observations	1006787	68161
N workers	25673	1341
Panel C: type of contract (working hours)		
	full-time	part-time
ERA increase	0.068***	0.248***
	(0.017)	(0.038)
Control mean	0.819	0.740
Observations	482544	696492
N workers	13404	19347

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by dimensions of employment contracts based on temporary agency work (**Panel A**), employment relationship (**Panel B**), and working hours (**Panel C**). I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.10: The effect of increased ERA on employment at 60-62 years old by establishment size category

	(1) small $N \in [5;19]$	(2) medium $N \in [20;249]$	(3) large $N \in [250;999]$
ERA increase	0.152***	0.096***	0.370***
	(0.003)	(0.031)	(0.030)
Control mean	0.795	0.777	0.712
Observations	59040	115956	48204
N workers	1640	3221	1339

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60th birthday to the 63rd birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by 3 categories of establishment size: small (5–19 workers), medium (20–249 workers), and large (at least 250 workers). I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level. The corresponding coefficient plot can be found in Figure A.10 in the Appendix.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.11: The effect of increased ERA on employment at 60-62 years old by industry categories

	(1) Agriculture, hunting and forestry, fishing	(2) Food and beverage	(3) Manu-facture of consumer products	(4) Manu-facture of industrial goods	(5) Manufacture of capital and consu mer goods	(6) Cons-truc-tion
ERA increase	0.496***	1.239***	0.342***	0.090	0.163**	0.547*
	(0.164)	(0.192)	(0.097)	(0.132)	(0.075)	(0.085
Control mean	0.602	0.744	0.755	0.800	0.670	0.782
Observations	4968	9252	9360	9540	13860	5976
N workers	138	257	260	265	385	166

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff increased by at least 3 years. I pool all observations from the month after the 60th birthday to the 63rd birthday (age a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average value women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.12: The effect of increased ERA on employment at 60-62 years old by number of affected peers

	(1) $N = 0$	(2) $N \in [1; 2]$	(3) $N \in [3; 9]$	(4) $N \in [10; ...]$
ERA increase	0.326***	0.216***	0.249***	0.260***
	(0.042)	(0.035)	(0.071)	(0.057)
Control mean	0.758	0.734	0.745	0.707
Observations	488988	129312	96516	76104
N workers	13583	3592	2681	2114

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60$^{\text{th}}$ birthday to the 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by TBA. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.13: Balance check. The effect of ERA increase on covariates

	(1) West origin	(2) non-German
ERA increase	-0.006	0.004
	(0.009)	(0.007)
Control mean	0.803	0.046
Observations	1179720	1179720
N workers	32770	32770

Notes: This table shows the effect of the ERA increase on *Western origin* (column 1) and *non-German* nationality (column 2) (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60$^{\text{th}}$ birthday to the 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.14: Balance check. The effect of ERA increase on covariates

	(1) ILMT	(2) ELMT	(3) human capital specifi-city	(4) mana-ger	(5) tenure	(6) task	(7) gender do-mination in occupation	(8 gende mina in es lishr
ERA increase	0.005	0.025**	0.118***	0.010	-0.022	0.117***	-0.100	-0.1
	(0.013)	(0.011)	(0.017)	(0.008)	(0.023)	(0.038)	(0.045)	(0.0
Observations	284652	282816	288036	1008864	1164888	1164888	201888	197

Notes: This table shows the effect of the ERA increase on the main variables used for subsample analyses described regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool a after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function optimal bandwidth choice. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in p birth-month level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.15: The effect of ERA increase on employment outcomes at 60-62 years old

	(1) employment
Panel A: baseline	
ERA increase	0.175***
	(0.029)
Panel B: controlling for firm FE in 1985-1992 and 1993-1999	
ERA increase	0.175***
	(0.044)
Control mean	0.772
Observations	1179720
N workers	32770

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. In **Panel A**, I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. In **Panel B**, I additionally control for the establishment fixed effects in 1985–1992 and 1993–1999. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parenthesis are clustered at the birth-month level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.16: Falsification test: RDD on employment at 60-62 years old around the reform cutoff for males

	(1)
	employment
ERA increase	0.052***
	(0.016)
Control mean	0.852
Observations	1230624
N workers	34184

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1) for males. The cutoff is January 1952, starting from which ERA increased by at least 3 years. I pool all observations from the month after the 60$^{\text{th}}$ birthday to the 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to men born in 1951. Robust standard errors in parenthesis are clustered at the birth month level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.17: Falsification test: RDD on employment at 60-62 years old around placebo cutoffs

	(1) employment
Panel A: 1948 cohort females	
ERA increase	-0.021
	(0.013)
Control mean	0.727
Observations	728892
N workers	20247
Panel B: 1949 cohort females	
ERA increase	0.007
	(0.017)
Control mean	0.736
Observations	853812
N workers	23717
Panel C: 1950 cohort females	
ERA increase	-0.003
	(0.007)
Control mean	0.736
Observations	985104
N workers	27364
Panel D: 1951 cohort females	
ERA increase	0.017 *
	(0.012)
Control mean	0.755
Observations	1083420
N workers	30095

Notes: This table shows the effect of the ERA increase on *employment* (RDD regression in Equation 1). **Panel A** performs RDD for the women born in 1947–1948, around the January 1948 cutoff; **Panel B** - born in 1948–1949, around the January 1949 cutoff; **Panel C** - born in 1949–1950, around the January 1950 cutoff; and **Panel D** - born in 1950–1951, around the January 1951 cutoff. I pool all observations from the month after the 60^{th} birthday to the 63^{rd} birthday (age months corresponding to ages 60–62). I use a uniform kernel function and a mean square error-based optimal bandwidth choice. I control for a calendar month, a dummy for Western residence, wages at the age of 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in the corresponding years below the cutoff. Robust standard errors in parenthesis are clustered at the birth month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.18: Summary statistics of all the treatment and control variables in 2009

Variable	Mean	sd	median	min	max	N
N_treat	1.144	1.463	1	0	17	3750
N_OldWom	2.137	2.441	1	1	29	3750
N_old	4.486	5.566	2	1	60	3750
N_f	34.725	43.752	16	5	246	3750
N_male	1.146	2.409	0	0	24	3750
N_treatpt	1.224	1.899	1	0	21	3750
N_treatptTreated	0.638	1.124	0	0	14	3750
N_treatft	0.913	1.413	1	0	15	3750
N_treatftTreated	0.505	0.888	0	0	9	3750
N_1534_ft	10.237	16.019	4	0	174	3750
N_3554_ft	18.103	24.686	8	0	190	3750
N_55_ft	6.375	7.841	3	1	73	3750
N_frau_ft	19.311	26.278	9	1	198	3750
mean_wage_ft	20018.687	11577.021	17975.390	246.297	66993.844	3750

Notes: this table shows the summary statistics (mean, standard deviation, median, min, and max) of the treatment variables in the year 2009.

Table B.19: Summary statistics for establishments in 2009 (pre-reform enaction year)

Variable	Mean	(Std. Dev.)	Min.	Max.	N
N focal separations	0.011	(0.126)	0	5	3750
N coworker separations	0.284	(1.547)	0	69	3750
N workers hired	5.708	(10.609)	0	191	3750
N 15-34 y.o. hired	3.09	(6.294)	0	95	3750
N 35-54 y.o. hired	2.147	(4.687)	0	102	3750
N males hired	2.397	(5.976)	0	180	3750
N females hired	3.311	(6.392)	0	88	3750
N 35-54 y.o. females hired	1.317	(2.957)	0	50	3750
N part-time hired	2.551	(6.206)	0	107	3750
N full-time hired	3.115	(7.425)	0	189	3750
N focal promotions	0.211	(0.569)	0	10	3750
N coworker promotions	6.115	(9.313)	0	82	3750
N males promoted	2.584	(5.023)	0	70	3750
N females promoted	3.531	(5.946)	0	65	3750
N 15-34 y.o. promoted	3.178	(5.713)	0	67	3750
N 35-54 y.o. promoted	2.533	(4.204)	0	43	3750
N 35-54 y.o. females promoted	1.692	(3.16)	0	35	3750
N part-time promoted	2.189	(4.574)	0	48	3750
N full-time promoted	3.889	(7.079)	0	81	3750
hired worker wage bill	51045.22	(154623.265)	0	6279447	3750
coworker wage bill	788458.209	(1356296.642)	3919.198	14934470	3750
focal worker wage bill	43832.197	(68731.008)	0	979077.625	3750
N high school hired	1.354	(3.562)	0	59	3750
N vocational degrees hired	3.38	(6.755)	0	145	3750
N university degree hired	0.726	(2.184)	0	33	3750
N hired from other establishments	4.756	(9.059)	0	186	3750
N hired for the 1st job	0.665	(2.935)	0	80	3750
N non-Germans hired	0.511	(2.132)	0	42	3750
N focal promotions	0.01	(0.117)	0	4	3750
N coworker promotions	0.193	(0.842)	0	21	3750
N focal promotions (level)	0.007	(0.103)	0	4	3750
N coworker promotions (level)	0.113	(0.636)	0	21	3750
N focal promotions	0.01	(0.117)	0	4	3750
N coworker promotions	0.193	(0.842)	0	21	3750
N focal demotions	0.083	(0.307)	0	5	3750
N coworker demotions	0.804	(1.952)	0	38	3750
N focal promotions (level)	0.007	(0.103)	0	4	3750
N coworker promotions (level)	0.113	(0.636)	0	21	3750
focal wage growth (lagged)	0.316	(2.27)	-1.915	65.409	3750
coworker wage growth (lagged)	12.31	(48.151)	-6.183	2164.932	3750
establishment closure in years	0.317	(1.545)	0	8	3750
focal wage growth	0.372	(2.563)	-1.591	85.369	3750
coworker wage growth	11.648	(25.347)	-14.029	447.379	3750
number of occupations	6.409	(5.502)	1	42	3750

Notes: this table shows the summary statistics (mean, standard deviation, median, min, and max) of the outcome variables in the year 2009.

Table B.20: The effect of an additional treated worker on worker retention

	(1) N focals retained	(2) N focals retained at 60-62 y.o.	(3) N part-time focals retained	(4) N full-time focals retained	(5) N fixed-term contracts retained	(6) N temporary contracts retained
Anticipation	0.024	0.216***	0.048	-0.024	0.026***	0.835***
	(0.017)	(0.013)	(0.032)	(0.029)	(0.005)	(0.023)
Anticipation × N_treat	0.048**	-0.539***	0.039	0.009	-0.004	-0.006
	(0.020)	(0.013)	(0.035)	(0.034)	(0.005)	(0.020)
Post	-0.135***	-0.447***	0.008	-0.143***	-0.005	-0.131***
	(0.022)	(0.020)	(0.027)	(0.026)	(0.010)	(0.026)
Post × N_treat	0.149***	0.274***	0.068**	0.081***	-0.004	0.153***
	(0.025)	(0.019)	(0.029)	(0.029)	(0.008)	(0.026)
Median establishment	0.000	0.000	0.000	0.000	0.000	0.000
Observations	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752

Notes: the coefficient on $Post \times N\_treat$ corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on the focal workers (born in 1950-1953) retention (Columns 1-2) and the targeted retention by contract types (Columns 3-6) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.21: The effect of an additional treated worker on separations and promotions of focal workers and coworkers

	(1) N focal separations	(2) N coworker separations	(3) N focal promotions	(4) N coworker promotions	(5) N focal demotions	(6) N coworker demotions
Anticipation	0.006	0.093**	-0.120***	0.991***	0.001	-0.194***
	(0.004)	(0.042)	(0.015)	(0.157)	(0.010)	(0.030)
Anticipation × N_treat	-0.014***	-0.008	0.013	-0.079	-0.011	0.033
	(0.003)	(0.033)	(0.013)	(0.138)	(0.008)	(0.028)
Post	0.002	0.015	-0.077***	1.913***	0.025***	-0.237***
	(0.005)	(0.067)	(0.014)	(0.189)	(0.009)	(0.031)
Post × N_treat	-0.010**	0.008	0.008	-0.276**	-0.011*	-0.008
	(0.004)	(0.038)	(0.011)	(0.139)	(0.006)	(0.024)
Median establishment	0.000	0.000	0.000	3.000	0.000	0.000
Observations	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on the focal workers (born in 1950-1953) and coworkers' separation (Column 1-2), promotion (Column 3-4), and demotion (Column 5-6) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.22: The effect of an additional treated worker on targeted promotions by age groups and gender

	(1)	(2)	(3)	(4)	(5)	
	N workers	N 15-34 y.o.	N 35-54 y.o.	N males	N females	N 35-54 y.o. females
Panel A: number of promotions						
Anticipation	0.991***	0.448***	0.456***	0.602***	0.390***	0.255***
	(0.157)	(0.078)	(0.091)	(0.089)	(0.103)	(0.065)
Anticipation × N_treat	-0.079	-0.027	-0.058	-0.087	0.008	-0.015
	(0.138)	(0.078)	(0.075)	(0.077)	(0.090)	(0.055)
Post	1.913***	0.673***	0.893***	0.904***	1.009***	0.591***
	(0.189)	(0.093)	(0.095)	(0.123)	(0.107)	(0.064)
Post × N_treat	-0.276**	-0.056	-0.206***	-0.071	-0.205**	-0.182***
	(0.139)	(0.074)	(0.069)	(0.080)	(0.088)	(0.050)
Median establishment	3.000	1.000	1.000	1.000	1.000	1.000
Observations	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752
Panel B: number of externally hired workers						
Anticipation	0.353	0.293**	-0.029	0.207	0.146	-0.049
	(0.233)	(0.116)	(0.118)	(0.140)	(0.124)	(0.067)
Anticipation × N_treat	-0.464***	-0.147	-0.281***	-0.224**	-0.239**	-0.190***
	(0.174)	(0.095)	(0.086)	(0.096)	(0.107)	(0.058)
Post	0.422*	0.300**	-0.021	0.224	0.198	-0.005
	(0.249)	(0.127)	(0.124)	(0.140)	(0.139)	(0.069)
Post × N_treat	-0.466**	-0.193*	-0.251***	-0.118	-0.348***	-0.219***
	(0.188)	(0.101)	(0.089)	(0.101)	(0.115)	(0.060)
Median establishment	2.000	1.000	1.000	1.000	1.000	0.000
Observations	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752

Notes: the coefficient on $Post \times N\_treat$ corresponds to the effect of having 1 additional treated worker (born 1952-1953, who experienced ERA increase) in 2009 on the targeted number of promotions (**Panel A**) and external hires (**Panel B**) by age (Columns 2-3) and gender (Columns 4-5) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$,

Table B.23: The effect of an additional treated worker on targeted hiring

	(1) N high school hired	(2) N vocational degrees hired	(3) N university degree hired	(4) N hired from other establishments	(5) N hired for the 1st job	(6) N foreigners hired
Anticipation	0.063	-0.223	0.201***	0.113	0.049	0.274***
	(0.058)	(0.160)	(0.047)	(0.210)	(0.037)	(0.046)
Anticipation × N_treat	-0.057	-0.363***	-0.007	-0.423***	-0.021	-0.090*
	(0.058)	(0.113)	(0.036)	(0.149)	(0.036)	(0.050)
Post	0.061	-0.165	0.287***	0.197	0.019	0.375***
	(0.066)	(0.168)	(0.055)	(0.227)	(0.045)	(0.079)
Post × N_treat	-0.131*	-0.266**	0.023	-0.358**	-0.058	-0.127*
	(0.068)	(0.131)	(0.039)	(0.171)	(0.052)	(0.066)
Median establishment	0.000	1.000	0.000	2.000	0.000	0.000
Observations	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, who experienced ERA increase) in 2009 on number of externally hired workers by education categories (Columns 1-3) and source of hires (Columns 4-5), as well as foreigners (Column 6) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.24: The effect of an additional treated worker on targeted promotions by contract types

Panel A: number of promotions				
	(1)	(2)	(3)	(4)
	N part-time	N full-time	N temporary contracts	N fixed-term contracts
Anticipation	0.455***	0.551***	0.097***	1.233***
	(0.093)	(0.124)	(0.021)	(0.062)
Anticipation × N_treat	0.078	-0.155	0.013*	0.002
	(0.091)	(0.108)	(0.007)	(0.047)
Post	0.915***	1.007***	0.051***	0.732***
	(0.105)	(0.150)	(0.018)	(0.099)
Post × N_treat	-0.085	-0.189*	0.070	-0.117
	(0.084)	(0.109)	(0.059)	(0.080)
Median establishment	1.000	1.000	0.000	0.000
Observations	44661	44661	44661	44661
N establishments	3752	3752	3752	3752
Panel B: number of externally hired workers				
Anticipation	0.456***	-0.084	0.187***	1.875***
	(0.115)	(0.199)	(0.051)	(0.096)
Anticipation × N_treat	-0.146	-0.318**	0.029	-0.042
	(0.120)	(0.134)	(0.022)	(0.068)
Post	0.622***	-0.187	0.115***	0.810***
	(0.133)	(0.193)	(0.041)	(0.139)
Post × N_treat	-0.304***	-0.162	0.129	-0.211
	(0.117)	(0.138)	(0.080)	(0.143)
Median establishment	1.000	1.000	0.000	0.000
Observations	44661	44661	44661	44661
N establishments	3752	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on number of promotions **(Panel A)** and externally hired workers (**Panel B**) by working-hours (Columns 1-2) and contract types (Columns 3-4) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.25: The effect of an additional treated worker on wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	focal worker wage bill	coworker wage bill	hired worker wage bill	focal worker wage growth	focal worker wage growth (lagged)	coworker wage growth	coworker wage growth (lagged)
Anticipation	-15560.528***	94967.576***	5478.842	-0.131	0.021	4.109***	-0.670
	(867.386)	(16129.220)	(4168.642)	(0.103)	(0.154)	(1.571)	(2.236)
Anticipation × N_treat	1161.603*	-8757.066	-5520.630*	-0.070	-0.132	-1.227	0.514
	(689.424)	(10874.539)	(2831.071)	(0.059)	(0.097)	(1.479)	(1.021)
Post	-18857.487***	142402.160***	2892.217	-0.097	-0.077	5.346***	-0.015
	(858.010)	(16520.595)	(4200.332)	(0.093)	(0.113)	(1.488)	(2.254)
Post × N_treat	4743.420***	-6461.148	-2279.591	-0.089*	-0.095	-1.679	-0.134
	(825.426)	(13337.706)	(2586.251)	(0.046)	(0.067)	(1.377)	(0.959)
Median establishment	1.9e+04	2.8e+05	1.5e+04	0.000	0.000	2.912	2.483
Observations	44661	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on the wage bills of focal workers (born in 1950-1953), coworkers (Column 1-2), and hired workers (Column 1-3), as well as the growth rates of focal workers and coworkers (Columns 4-7) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 who experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.26: The effect of an additional treated worker on workforce composition

Panel A: main outcomes

	N focals retained	N 35-54 y.o. females hired	N N 35-54 y.o. females promoted
Anticipation	0.026	-0.044	0.259***
	(0.017)	(0.065)	(0.065)
Anticipation × N_treat	-0.102	-8.588	7.064
	(0.438)	(6.015)	(5.283)
Post	-0.136***	-0.003	0.600***
	(0.021)	(0.070)	(0.063)
Post × N_treat	0.888***	-1.483***	-1.225***
	(0.103)	(0.351)	(0.302)
Anticipation×N_treatmale	0.033*	-0.016	-0.015
	(0.020)	(0.077)	(0.061)
Anticipation×N_treatpt	0.178	8.468	-7.005
	(0.439)	(6.017)	(5.285)
Anticipation×N_treatft	0.116	8.343	-7.163
	(0.439)	(6.016)	(5.284)
Post×N_treatmale	0.033	0.142	-0.003
	(0.023)	(0.094)	(0.062)
Post×N_treatpt	-0.725***	1.273***	1.109***
	(0.111)	(0.366)	(0.315)
Post×N_treatft	-0.752***	1.259***	0.993***
	(0.107)	(0.384)	(0.316)
Median establishment	0.000	0.000	1.000
Observations	44661	44661	44661
N establishments	3752	3752	3752

Panel B: promotions and hiring

	N males promoted	N part-time promoted	N part-time promoted	N full-time promoted	N males hired	N females hired	N part-time hired	N full-time hired
Anticipation	0.600***	0.394***	0.464***	0.544***	0.188	0.152	0.474***	-0.118
	(0.089)	(0.101)	(0.090)	(0.123)	(0.134)	(0.121)	(0.111)	(0.190)
Anticipation × N_treat	-12.805	-4.163	-0.652*	-6.295	-45.061**	-19.241*	-4.253***	-61.549**
	(8.618)	(6.097)	(0.389)	(14.483)	(18.168)	(11.133)	(1.606)	(29.596)
Post	0.896***	1.022***	0.926***	0.997***	0.235*	0.206	0.616***	-0.169
	(0.123)	(0.106)	(0.103)	(0.151)	(0.141)	(0.140)	(0.134)	(0.194)
Post × N_treat	-15.429***	-4.022	-0.998	-17.430**	-11.114***	-2.031	0.237	-19.758**
	(3.921)	(2.620)	(1.654)	(8.663)	(2.410)	(3.019)	(1.514)	(8.983)
Anticipation×N_treatmale	-0.075	-0.034	-0.014	-0.093	-0.191	0.090	-0.022	-0.073
	(0.171)	(0.100)	(0.094)	(0.208)	(0.229)	(0.151)	(0.120)	(0.300)
Anticipation×N_treatpt	12.744	4.235	0.791	6.165	44.927**	19.159*	4.196**	61.390**
	(8.619)	(6.101)	(0.481)	(14.484)	(18.169)	(11.137)	(1.654)	(29.596)
Anticipation×N_treatft	12.729	4.117	0.685*	6.138	44.805**	18.854*	4.077**	61.081**
	(8.619)	(6.098)	(0.404)	(14.485)	(18.169)	(11.135)	(1.601)	(29.597)
Post×N_treatmale	-0.143	0.043	-0.099	-0.014	-0.057	0.272	0.207	-0.010
	(0.195)	(0.118)	(0.101)	(0.228)	(0.232)	(0.196)	(0.161)	(0.287)
Post×N_treatpt	15.299***	3.904	0.922	17.259**	10.998***	1.773	-0.555	19.702**
	(3.922)	(2.626)	(1.669)	(8.664)	(2.415)	(3.027)	(1.535)	(8.985)
Post×N_treatft	15.482***	3.752	0.964	17.246**	11.077***	1.576	-0.521	19.550**
	(3.923)	(2.625)	(1.654)	(8.665)	(2.412)	(3.036)	(1.527)	(8.985)
Median establishment	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Observations	44661	44661	44661	44661	44661	44661	44661	44661
N establishments	3752	3752	3752	3752	3752	3752	3752	3752

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.27: The effect of an additional treated worker on workforce composition

	(1)	(2)	(3)
	N focals retained	N 35-54 y.o. females hired	N 35-54 y.o. females promoted
Panel A (baseline): defining treatments in 2009 (pre-reform enaction year)			
Anticipation (2010-2011)	0.024	-0.049	0.255***
	(0.017)	(0.067)	(0.065)
Anticipation × N_treat20091	0.048**	-0.190***	-0.015
	(0.020)	(0.058)	(0.055)
Post (2012-2015)	-0.135***	-0.005	0.591***
	(0.022)	(0.069)	(0.064)
Post × N_treat20091	0.149***	-0.219***	-0.182***
	(0.025)	(0.060)	(0.050)
Median establishment	0.000	0.000	1.000
Observations	44661	44661	44661
N establishments	3752	3752	3752
Panel B: defining treatments in 1998 (pre-reform announcement year)			
Anticipation (1999-2011)	-0.346***	0.172	0.524***
	(0.044)	(0.222)	(0.153)
Anticipation × N_treat19981	-0.063	-0.089	-0.054
	(0.056)	(0.204)	(0.127)
Post (2012-2015)	-0.081**	0.356**	0.745***
	(0.036)	(0.141)	(0.118)
Post × N_treat19981	0.113***	-0.319*	-0.492***
	(0.043)	(0.181)	(0.157)
Median establishment	1.000	0.000	1.000
Observations	46332	46332	46332
N establishments	2220	2220	2220

Notes:

Notes: the coefficient on $Post \times N\_treat$ corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 (**Panel A**) or 1998 (**Panel B**) on the focal workers (born in 1950-1953) retention (Column 1) and the number external hires and promotions of middle-aged females (Columns 2-3) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011 (**Panel A**) or 1999-2011 (**Panel B**), *N_treat*- number of treated workers (focal workers born in 1952-1953 that experienced the ERA increase.) Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.28: The effect of an additional treated worker on workforce composition

	(1) N focals retained	(2) N 35-54 y.o. females hired	(3) N 35-54 y.o. females promoted
Panel A: Anticipation (2010-2012), Post (2013-2016)			
Anticipation (2010-2012)	-0.078***	-0.120*	0.221***
	(0.019)	(0.063)	(0.061)
AnticipationB×N_treat20091	0.077***	-0.174***	-0.067
	(0.022)	(0.053)	(0.047)
Post (2013-2016)	-0.169***	-0.051	0.306***
	(0.022)	(0.074)	(0.066)
PostB×N_treat20091	0.188***	-0.215***	-0.170***
	(0.026)	(0.067)	(0.063)
Median establishment	0.000	0.000	1.000
Observations	48411	48411	48411
N establishments	3752	3752	3752
Panel B: Anticipation (2010-2010), Post (2011-2014)			
Anticipation (2010-2010)	0.178***	-0.110	0.222***
	(0.017)	(0.069)	(0.075)
AnticipationC×N_treat20091	0.022	-0.121	0.074
	(0.018)	(0.076)	(0.074)
Post (2011-2014)	-0.023	-0.062	0.433***
	(0.022)	(0.071)	(0.062)
PostC×N_treat20091	0.093***	-0.199***	-0.089**
	(0.024)	(0.047)	(0.040)
Median establishment	0.000	0.000	1.000
Observations	40911	40911	40911
N establishments	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on the focal workers (born in 1950-1953) retention (Column 1) and the number external hires and promotions of middle-aged females (Columns 2-3) (coefficients β_P in Equation 3). *Post* stands for the years 2013-2016 in **Panel A** (2011-2014 in **Panel B**), *Anticipation*- years 2010-2012 in **Panel A** (2010 in **Panel B**), *N_treat*- number of treated workers (focal workers born in 1952-1953 that experienced the ERA increase.) The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.29: The effect of an additional treated worker on workforce composition

	(1) N focals retained	(2) N 35-54 y.o. females hired	(3) N 35-54 y.o. females promoted
Panel A: sample that restricts to Bavaria (no education reform)			
Anticipation	0.119***	0.077	-0.109
	(0.046)	(0.135)	(0.158)
Anticipation × N_treat	-0.001	-0.064	-0.015
	(0.045)	(0.118)	(0.139)
Post	0.007	0.317**	0.423***
	(0.092)	(0.128)	(0.133)
Post × N_treat	0.045	-0.065	-0.126
	(0.066)	(0.092)	(0.107)
Median establishment	0.000	0.000	1.000
Observations	7557	7557	7557
N establishments	618	618	618
Panel B: include industry × year fixed effects			
Anticipation	0.047	-0.071	0.345***
	(0.043)	(0.139)	(0.130)
Anticipation × N_treat	0.047**	-0.185***	-0.019
	(0.020)	(0.058)	(0.055)
Post	-0.003	-0.008	0.650***
	(0.052)	(0.190)	(0.174)
Post × N_treat	0.148***	-0.214***	-0.183***
	(0.025)	(0.059)	(0.050)
Median establishment	0.000	0.000	1.000
Observations	44661	44661	44661
N establishments	3752	3752	3752

Notes: the coefficient on *Post* × *N_treat* corresponds to the effect of having 1 additional treated worker (born 1952-1953, experienced ERA increase) in 2009 on the focal workers (born in 1950-1953) retention (Column 1) and the number external hires and promotions of middle-aged females (Columns 2-3) in the years 2012-2015 (coefficients β_P in Equation 3). *Post* stands for the years 2012-2015, *Anticipation*- years 2010-2011, *N_treat*- number of treated workers (focal workers born in 1952-1953 that experienced the ERA increase.) **Panel A** is the subsample regression on Bavaria only. **Panel B** controls for industry-year fixed effects. The *Median establishment* stands for the medians of the outcome variables in the corresponding category of outcomes. The standard errors in parentheses are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.