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Retirement Age Reforms and Worker Substitutability: Implications for Employment of Older Workers

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

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Abstract

This paper studies how labor demand factors—specifically worker substitutability and job-specific skills—shape employment responses to a rise in the early retirement age. Using a regression discontinuity design, I exploit a 1999 German reform that eliminated the option for women to retire at age 60. Before the reform, older workers could exit voluntarily, thereby imposing turnover costs on firms. Afterward, firms were better able to retain less substitutable workers for whom turnover costs are higher. At the same time, the loss of early pension eligibility reduced workers' outside options, allowing firms to offer lower wages, often through partial retirement.

Zusammenfassung

Dieses Papier untersucht, wie arbeitsnachfrageseitige Faktoren – insbesondere die Ersetzbarkeit von Arbeitskräften und berufsspezifische Fähigkeiten – die Beschäftigungsreaktionen auf eine Anhebung des frühestmöglichen Rentenalters beeinflussen. Mithilfe eines Regression-Discontinuity-Designs analysiere ich eine Reform in Deutschland im Jahr 1999, die die Möglichkeit für Frauen abschaffte, bereits mit 60 Jahren in Rente zu gehen. Vor der Reform konnten ältere Beschäftigte freiwillig aus dem Erwerbsleben ausscheiden, was den Unternehmen Fluktuationskosten verursachte. Nach der Reform waren Betriebe besser in der Lage, schwer ersetzbare Arbeitskräfte mit höheren Austrittskosten zu halten. Gleichzeitig verschlechterte sich durch den Wegfall des vorgezogenen Rentenzugangs die Verhandlungsposition der Beschäftigten, was es den Unternehmen ermöglichte, niedrigere Löhne durchzusetzen – häufig in Form von Altersteilzeit.

JEL

H32, H55, J21, J24, J26

Keywords

aging, raise in the retirement age, internal labor markets, human capital, worker substitutability

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1 Introduction

The dynamics of labor markets are profoundly influenced by the interplay between worker substitutability and firm-specific human capital. The ease with which workers can be replaced affects various labor supply decisions, including absences due to temporary illness (Hensvik/Rosenqvist, 2019), the duration of actual parental leave in reaction to extension of parental leave duration (Ginja/Karimi/Xiao, 2023) and increase of paid parental leave eligibility coverage (Huebener et al., 2024), and labor supply following a coworker's death (Jäger/Heining, 2022). Worker substitutability has also been associated with wage losses after job displacement (Jacobson/LaLonde/Sullivan, 1993), as workers with more specific skills, such as those tied to a particular industry or occupation, face greater difficulty finding comparable jobs in the external labor market. However, the role of worker substitutability in the context of retirement, a significant driver of workforce turnover, remains underexplored.

While substantial literature examines how statutory retirement age reforms impact labor supply (Atalay/Barrett, 2015; Brinch/Vestad/Zweimüller, 2015; Geyer/Welteke, 2021; Hanel/Riphahn, 2012; Hernæs et al., 2016; Lalive/Staubli, 2015; Lalive/Magesan/Staubli, 2023; Manoli/Weber, 2016; Mastrobuoni, 2009; Staubli/Zweimüller, 2013; Vestad, 2013), there is limited understanding of how labor demand mechanisms, such as job-specific skills and worker substitutability, shape employment responses to such reforms because these papers often assume that labor demand is perfectly elastic at the relevant margins. In contrast, my paper argues that labor demand is not uniformly elastic and highlights the role of worker substitutability in shaping firms' retention decisions. This paper aims to bridge this gap in the retirement literature by integrating insights from studies on worker substitutability with research on employment reactions to retirement reforms. Understanding this mechanism is crucial, as it offers deeper insights into how worker substitutability influences labor supply adjustments to retirement reforms and the coping strategies adopted by workers and firms. This challenges the standard assumption of uniformly elastic labor demand and offers new insights into the incidence and efficiency of retirement reforms.

The seminal study by Becker (1962) posits that firm-specific human capital renders incumbent workers less substitutable by external hires. In the context of reforms that raise the retirement age, this theory suggests that employment responses by older workers may exhibit substantial heterogeneity based on their substitutability and the specificity of the human capital required for their roles. A pertinent question arises: When early retirement options are curtailed, do firms respond uniformly across worker types, or do employment gains disproportionately accrue to those with more specific skills and lower substitutability?

Such differences may reflect how firms and workers coordinate—depending on their turnover costs—in response to extended employment horizons. The demand for workers rises due to firm- or job-specific human capital, or challenges in finding suitable replacements internally or externally. However, in the presence of outside options in the form of pensions, firms may have difficulties retaining such workers. Reforms raising the retirement age could help firms to retain such workers.[1]

Employment decisions at older ages are affected by many factors, including health, ability, income, and flexibility of contracts and firms; hence, in the absence of exogenous drivers, such decisions are likely endogenous at the individual level. Moreover, given an option to retire and receive a pension, workers may opt to exit the workplace and instead prioritize personal benefits (such as health, leisure time with family, etc) over firm factors (such as their substitutability and costs of replacement) in deciding to retire. A reform that raises the retirement age shifts the employment dynamics of those affected. I overcome this endogeneity challenge by studying the effects of a reform in Germany that abolished the women's pathway to early retirement by making the statutory retirement ages gender neutral. This reform resulted in a sharp rise of at least three years (from 60 to 63) in the Early Retirement Age (ERA), the earliest age women could begin to claim a pension. This discontinuous policy change, which impacted women born from 1952 onward, provides a natural experiment for causally identifying the effect of raising the retirement age on employment and wages using a Regression Discontinuity Design (RDD), and exploring the relationship of worker substitutability with a large labor supply increase.

The German labor market, characterized by substantial variation in worker substitutability[2] and strong dismissal protections, offers a suitable setting for investigating whether workers delay retirement based on their skills and substitutability. The availability of comprehensive German establishment data, which encompasses entire workforces and employment histories, together with job cell data (3-digit occupation groups within the establishments), enables analysis of internal markets, measurement of the availability of internal substitutes (workers sharing the same 3-digit occupation), and a study of personnel practices employed by the establishments.

To examine how employment responses to the rise in retirement age interact with worker substitutability, I start by sketching a simple model of the interplay between the reform that raises the age of the option to receive pensions, turnover costs, and employment decisions

[1] Stole/Zwiebel (1996a) and Stole/Zwiebel (1996b) provided theoretical discussions of intra-firm bargaining and its relation to firm-specific human capital, while Lazear (2009) and Cahuc/Marque/Wasmer (2008) extended the discussion by arguing that, similar to firm-specific human capital, the ease with which a firm can find a suitable replacement could affect the wages of workers. However, having lower bargaining power after removal of the option to receive pensions, firms may be in a stronger position than workers.

[2] Previous literature for Germany has shown that frictions in replacing workers are important (Jäger/Heining, 2022; Huebener et al., 2024).

at 60-62. I also outline a Nash bargaining model with implications for the effects of the reform and of substitutability on wages conditional on employment at 60-62.

To test these implications empirically, I first construct several proxies for worker substitutability (and therefore turnover costs). First, I examine whether workers with specific skills are more likely to be retained at older ages. Specific human capital and managerial roles are key determinants of worker substitutability, as external replacements for these skills are often scarce (Baker/Gibbs/Holmstrom, 1994)[3]. Consistent with theories of firm- and job-specific human capital (Becker, 1962), Bertheau (2021) shows that jobs requiring teamwork and training with senior workers are more often filled internally. In the context of retirement reform, this suggests that establishments where older workers' positions rely on job- or firm-specific human capital may benefit most from the extended retention of older workers. Next, I explore internal (coworkers in the same occupation) and external (potential hires in a commuting zone for a given occupation or industry) labor market thickness. According to Topel/Ward (1992), both internal and external labor markets affect workers' life-cycle labor market outcomes. In thin labor markets, finding suitable replacements is more challenging, making worker turnover costly for firms (Lazear, 1979). Automation can substitute for some types of labor, leading to reduced employment and wages, particularly in economies with aging populations like Germany (Acemoglu/Restrepo, 2022). Hence, I test whether the substitutability matters beyond the worker level, by dividing occupations by routineness, a proxy for substitution by automation. Finally, I consider the tradability of industries as another dimension of worker substitutability. Firms in tradable industries can replace workers not only locally but also by outsourcing tasks globally, increasing substitutability (Drenik et al., 2023). While characteristics such as managerial status or skill specificity may reflect both firm-side costs and worker-side preferences, I interpret heterogeneity in the reform's effects primarily through the lens of firms' retention incentives — that is, the labor demand channel.

My findings confirm the implications of the model and indicate that the reform increased employment among women aged 60–62 by 17.3 percentage points (a 22% increase relative to the control mean of workers who were eligible to retire at 60). These results are robust to variations in model specification. To gauge the potential scale of the reform's impact, I conduct a back-of-the-envelope calculation. This treatment effect would translate into roughly 540,000 additional women remaining employed at ages 60–62 due to the reform.[4] Conditional on employment, the workers whose retirement age rose by the reform are less likely to bargain for higher wages at ages 60–62, compared to those previously eligible for pension benefits. The reform removed access to early retirement, weakening outside

[3] See also Bartel et al. (2014), Friedrich/Hackmann (2021), Jäger/Heining (2022), Jaravel/Petkova/Bell (2018)

[4] This is a rough calculation based on local treatment effects for women born in 1951–1952, who were employed continuously at 58-59 years old. The estimate assumes that the sample is nationally representative and that the effect generalizes across cohorts affected by the reform. It does not adjust for compositional differences or cohort trends and should be interpreted as illustrative.

options and shifting bargaining power toward employers. This effect is likely amplified for older workers with specific skills and low substitutability, who already face limited mobility in the labor market. The observed decline in monthly wages partly reflects a compositional shift toward part-time or partial-retirement contracts, but also suggests a change in the wage-setting environment. These patterns are consistent with monopsony models, where firms exploit weak outside options to offer lower wages or fewer hours. Recent evidence for Germany supports this interpretation, showing that firms in more monopsonistic labor markets reduce wage costs when workers' alternatives are constrained (Plöger, 2024).

My findings reveal that raising the retirement age does not have a uniform effect across workers; instead, its impact depends on how easily firms can replace those approaching retirement. The largest employment gains are observed among women whose leave would be associated with high turnover costs for the employers, i.e., women with specific skills and those who are employed in occupations that are more difficult to replace both internally and externally. The findings suggest that reforms raising the retirement age are most effective in extending careers for workers who are less easily replaced, shedding light on the interplay between firm- and occupation-specific human capital, labor market frictions, and retirement decisions. It is noteworthy that substitutability by automation does not display post-reform differences. Moreover, external substitutability of industries does not show differences as widely as do the external substitutabilities of occupations, nor does the tradability of industries. These findings suggest that substitutability by humans is more likely to explain retirement decisions than substitutability by automation, and that skills and occupations are more linked to substitutability than industries. Importantly, heterogeneous RDD effects do not imply that firms actively dismiss substitutable workers; employment protection laws make such terminations unlikely. Rather, the reform removes the early retirement option, shifting the retention margin: firms now retain more older workers overall, but the largest increase occurs among non-substitutable workers—those who previously left voluntarily when early retirement was available. Other explanations could be that some workers who are not retained can, for example, be encouraged by employers to use up to two years of unemployment as a bridge to retirement (Gudgeon et al., 2023), or choose self-employment or inactivity. Due to data limitations, I do not test for these alternative explanations, and only focus on employment. Because Geyer/Welteke (2021) do not find strong evidence for active substitution to unemployment for this specific reform, I conclude that the main explanation of these results are pre-reform voluntary exits of substitutable workers.

Before the reform, i.e., when these women were eligible to retire at 60, although firms faced greater constraints when substitutes were scarce, they generally could not prevent women from retiring at ages 60 to 62. Therefore, before the reform, retirement decisions were primarily driven by workers rather than employers. However, after the reform, firms became more likely to retain women who were less substitutable—even as they were offering them lower wages compared to their peers who were eligible to retire at 60. These findings imply that raising the retirement age shifts the dynamics of retirement decisions from the individual level to the firm level, conditional on turnover costs measured by low substitutability. In this context, reforms that raise the retirement age may help firms operating in imperfect labor markets to better manage workforce turnover and skill retention. This is a significant relief for firms, especially as, according to Muehlemann/Pfeifer (2016), firms in Germany bear sizable hiring costs for high-skilled labor, amounting to almost two months' wages.

The effects of the retirement age rise on wages vary across worker types. While overall wage bargaining power declines due to the reform, wage increases are observed among managers and workers in occupations with thin external labor markets, consistent with firms raising wages to retain more difficult-to-replace employees. However, this pattern does not hold for all subsamples that proxy for high turnover costs. With the early retirement path closed, older workers are effectively locked into the labor market, and for some, into their current firm, especially those in thin labor markets or with high job-specific skills. This weakens their outside options and enhances employers' monopsony power, allowing firms to suppress wages even for valuable workers. The reform effectively increases firms' monopsony power over older workers by removing early retirement as a credible outside option. This shift particularly affects less substitutable workers, who, before the reform, could leverage their high retention costs and scarce replacements to negotiate better conditions or to exit. With retirement no longer available until age 63, these workers become more reliant on their current employer, which limits their bargaining position despite their value to the firm. This explains why employment rises without proportional wage gains and highlights how policy-induced changes to outside options can amplify monopsony effects in segmented labor markets.

This study contributes to several strands of the literature. First, it adds to the empirical research on worker substitutability and workplace characteristics affecting labor market decisions (Jäger/Heining, 2022; Ginja/Karimi/Xiao, 2023; Huebener et al., 2024). My results align with recent evidence on firm responses to retention shocks. Jäger/Heining (2022) find that firms facing the removal of their least substitutable workers experienced substantial wage growth and hiring strain. Similarly, Huebener et al. (2024) document how extended paid parental leave weakened the link between internal substitutability and return-to-work behavior, showing how such policy-induced frictions can distort employer–employee coordination. Opposed to a reform that increases worker absence due to parental leave, I study a reform that decreases worker absences due to a rise in retirement age. My findings suggest that the reform delaying retirement strengthens the employer-employee coordination: firm responses depend on skill-specific turnover costs, with large effects concentrated among workers with few internal or external substitutes. My paper introduces novel evidence on how substitutability mediates firm responses to a retirement age increase. A key distinction is the nature of the shock: whereas parental leave is temporary and expected (as the mothers usually return to their prior employers), raising the early retirement age (ERA) from 60 to 63 binds older workers more tightly to their jobs up until their new pensionable age. Anticipated parental leave absences allow firms to plan. By contrast, the removal of early retirement compresses exit options, increasing reliance on specific workers while weakening their leverage in wage negotiations. I find that firms are more likely to retain workers who are more difficult to replace, as these workers have weaker bargaining positions due to reduced outside options. The reform thus reshapes employment, especially for less substitutable workers, and average wage dynamics.

Second, this paper contributes to the literature on employment decisions at older ages by examining the novel labor demand mechanisms that shape them. Most existing studies focus on mechanisms related to individual and household characteristics, while less attention has been given to the role of firms and labor demand. For example, previous research shows that retirement decisions are often coordinated within households, particularly among couples (Atalay/Barrett/Siminski, 2019; Bloemen/Hochguertel/Zweerink, 2019; García-Miralles/Leganza, 2024; Johnsen/Vaage/Willén, 2022; Lalive/Parrotta, 2017; Selin, 2017; Zweimüller/Winter-Ebmer/Falkinger, 1996). I extend this literature by demonstrating that older women also effectively coordinate their retirement timing with their employers, depending on potential turnover costs. Additionally, I extend the seminal paper by Geyer/Welteke (2021) that analyzed the 1999 reform[5] by (1) studying workplace labor demand mechanisms, in particular those highlighting turnover costs, worker job-specific skills and substitutability, which have not been analyzed for retirement reforms; (2) analyzing employment responses beyond 62 years of age, including bunching at the Normal Retirement Age; (3) analyzing whether the option to receive a pension before the reform helps workers to bargain for higher wages, i.e., the link between wages and employment, which has not been analyzed previously for this reform.

Third, while several studies have examined the role of firms in shaping retirement behavior, they primarily focus on institutional constraints. Deshpande/Fadlon/Gray (2024) show that firms contribute to the rigidity of retirement decisions, with many workers continuing to retire at the pre-reform statutory age despite policy changes in the U.S. Similarly, Rabaté/Jongen/Atav (2024) find that automatic job termination policies in the Netherlands drive much of the observed bunching at the statutory retirement age. The only paper that provides evidence on replacement costs using a different reform in Germany is by Geyer et al. (2022). They show that employers with a high share of older worker inflow compared with their younger worker inflow, employers in sectors with few investments in research and development, and employers in sectors with a high share of collective bargaining

[5] Geyer/Welteke (2021) use data on pension insurance, which is not linked to workplaces. Moreover, at the time when the paper was written, their data were right censored, preventing analyses beyond the ERA.

agreements allow their employees to remain employed longer after a reform that raised the normal retirement age. My research builds on these insights by introducing worker substitutability as a key labor demand factor influencing retirement responses that has been overlooked by the literature due to the scarcity of workplace data linked to retirement decisions. Detailed job-cell data from German social security records, combined with employment data of monthly frequency, allow for such analyses. Several papers analyze spillovers of raising retirement age on hiring using Italian (Bianchi et al., 2023; Boeri/Garibaldi/Moen, 2022; Carta/D'Amuri/von Wachter, 2021) and Dutch data (Hut, 2024; Ferrari/Kabátek/Morris, 2023); however, due to limited data on occupations and job cells in these administrative records, these studies do not analyze the direct effects of a reform on older workers' employment by availability of internal and external substitutes, or by human capital specificity of occupations, which I focus on in this paper. This study also bolsters understanding of the findings of papers that argue that institutional constraints and firms explain retirement behavior (Deshpande/Fadlon/Gray, 2024; Rabaté/Jongen/Atav, 2024). I focus on workers aged 60–62, who fall between the pre- and post-reform early retirement ages. At these ages, employment is not determined by formal contract changes or layoffs by employers, but by more implicit coordination between firms and workers. Building on this insight, I examine how these dynamics interact with voluntary early retirement decisions. I show that the reform, by restricting early retirement eligibility, enables firms to selectively retain workers who are more difficult to replace. My study extends theirs by focusing on retirement choices, policy-induced separation risk, and voluntary exits.

The structure of this paper is as follows. In chapter 2, I describe the institutional setting, including details about the 1999 reform that raised women's retirement age and the conceptual framework with implications of employment and wage dynamics. Chapter 3 presents the data and sample construction. Chapter 4 specifies the identification strategy I employ to study the effect of the reform on labor supply and the mechanisms associated with employment, while chapter 5 shows the corresponding estimation results for employment at 60-62 and wages, and robustness checks. Chapter 6 studies the mechanisms associated with labor demand, skills, and worker substitutability, and is followed by the conclusion.

2 Institutional setting and conceptual framework

This section presents the German labor market institutions and the 1999 reform, followed by the conceptual framework and implications I aim to test empirically.

2.1 Institutional setting

The German labor market is characterized as a labor market with relatively decentralized wage setting (Jäger/Heining, 2022; Dustmann et al., 2014). This labor market feature makes it easier for wages to deviate from the levels set by bargaining agreements. Overall, the labor market structure during the years under study makes it unlikely for firms to easily fire older workers.[6] Such regulation implies that the older workers are more likely to either leave voluntarily or in a subtle agreement with their employers through offering differentiated contracts, working hours, or wages. The downward rigidity of wages implies that wages usually decrease through offering different contracts, for example, through lower working hour agreements.

There are three pillars of the German pension system: public pensions, occupational pensions, and private provisions. Public pension insurance is the most popular choice among the working population, covering about 90% of the German workforce, according to Zwick et al. (2022). Given that in this paper I analyze a reform that changed some attributes in the public pension system, it had an impact on many people in the country because participation in the public pension is mandatory for all workers except for civil servants and the self-employed.[7].

The early retirement age (ERA) serves as a key behavioral anchor and coordination point in the German retirement system. It marks the first age at which workers can begin claiming a pension, albeit with actuarial deductions, while the normal retirement age (NRA) determines when a full, undeducted pension can be drawn. Workers respond strongly to both thresholds: Seibold (2021) documents bunching at these statutory ages, and Riphahn/Schrader (2021) and Geyer/Welteke (2021) find large labor supply shifts when either is reformed. In my setting, a reform raised the ERA from 60 to 63 for certain cohorts, effectively eliminating a prominent and widely used exit option between ages 60 and 62. This creates a new period where workers must either continue working or negotiate alternative exit paths. Importantly, this window still lies below the NRA, so continued employment is legally possible and common, but less predictable. Understanding the institutional role of ERA helps motivate the analysis: it clarifies why changes at 60 to 62 years generate observable effects and why firm decisions about retention and wages matter in the absence of this early retirement channel.

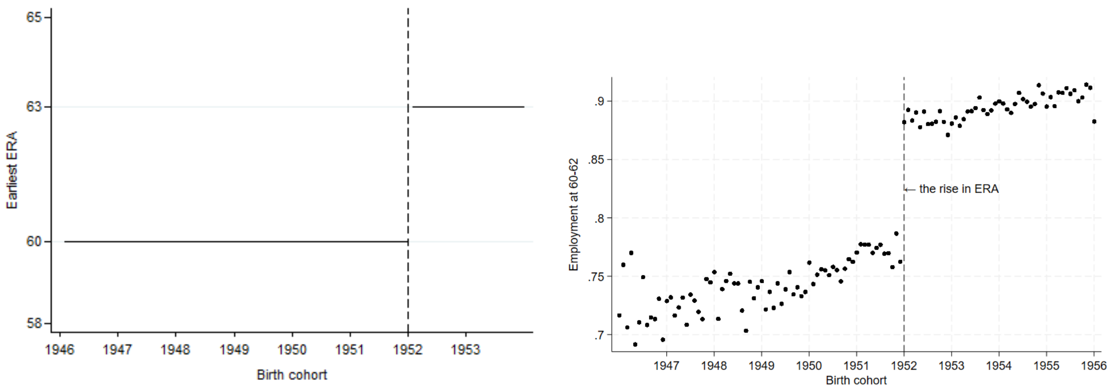
6 The Equal Treatment Act protects older workers from unjustified dismissal (*Allgemeines Gleichbehandlungsgesetz – AGG*, General Act on Equal Treatment of 14 August 2006 (Federal Law Gazette I, p. 1897), as last amended by Article 4 of the Act of 19 December 2022, Federal Law Gazette I, p. 2510).

7 The public pension system consists of a pay-as-you-go scheme. Pay-as-you-go means that current workers pay for current pension claimants.

There are several pathways to retirement, including regular, disability, long-term insurance, women's, and unemployment. While rules surrounding some pathways changed and some were abolished altogether, workers eligible for the regular pathways to retirement had a single statutory retirement age. ERA followed by NRA applied to vulnerable groups, including women, the unemployed, and the long-insured. Some of these pathways were modified or abolished, including the women's pathway that I analyze in this paper.

The 1999 reform abolished the women's pathway to early retirement at 60 years old by making the statutory retirement ages gender-neutral.[8] Before the reform, women could start claiming pensions earlier than men. Therefore, gender neutral statutory retirement ages induced by the reform raised women's early retirement age. The 1999 reform officially came into force on January 1, 1999, (Gohl et al., 2020), and affected women born from January 1, 1952. Hence, the change was discontinuous in terms of birth cohorts. For those who had accumulated enough contributions to be eligible for the long insurance pathway, the ERA rose by three years, while for workers on a regular pathway to retirement, the ERA rose by 5.5 years. Overall, the reform increased ERA for women by at least three years (left Panel of Figure 1).[9]

Figure 1.: Discontinuity in birth cohorts



Notes: The **left Panel** shows the policy rule for the earliest age a person could claim pensions by birth cohort. The **right Panel** shows the scatter plot of the fraction of women employed at the ages 60-62 over the birth cohorts 1947-1956. The dashed line presents the birth cohort cutoff, January 1952, starting from which the ERA rose by at least three years.

8 While the reform also abolished early pathways to retirement for the unemployed and for persons under a progressive retirement plan (Lorenz et al., 2018), I focus primarily on the abolishment of women's pathway to early retirement because the other two categories are not recorded in the data.

9 Before the 1999 reform, the NRA of women's pathway to retirement was fixed at 65. After the abolishment of women's pathway to early retirement, women were also affected by another reform that affected the regular pathway to retirement. In particular, due to the 2007 reform, workers on the regular pathway experienced a retirement age increase starting from 1946 by one month per birth year (Figure A1), and their retirement age is expected to reach 67 for the 1963 birth cohort by 2029. This 2007 reform affected the women under my study because the NRA of those born in 1951 was 65, while that of those born in 1952 became 65.5.

2.2 Conceptual framework and implications

Firms operating in imperfect labor markets face frictions in replacing experienced workers, particularly those with occupation- or firm-specific skills. As these workers approach retirement age, firms risk productivity losses and incur hiring costs due to turnover. Early retirement eligibility grants workers considerable autonomy in deciding when to exit the labor force. This paper studies how a policy reform that raised the early retirement age (ERA) from 60 to at least 63 alters the interaction between worker substitutability and retirement behavior.

The model builds on the idea that retirement is not only a worker's choice, but also reflects the relative bargaining power of workers and firms. When early retirement is an option, workers with valuable skills may leverage this as a bargaining chip in wage negotiations. When the option is removed, firms can retain even valuable workers without raising wages. To understand how firms respond to a rise in early retirement age, I first develop a static model in which the firm's decision to retain a worker depends explicitly on the worker's substitutability and the policy environment. I then extend the framework with a Nash bargaining model, allowing wages to be endogenously determined. This yields testable implications conditional on employment.[10]

2.2.1. Firm's problem of employment decisions

Setup. Consider a firm employing worker i, who is approaching retirement age. Continued employment at ages 60–62 depends on whether the match between the worker and the firm remains viable. I model this using a latent retention condition, in which both the firm and the worker must benefit from continued employment. While the firm's willingness to accommodate employment reflects the economic value of the match, the worker's outside option plays a key role in the joint decision. I do not model active dismissal, consistent with strong employment protections. Instead, I interpret "retention" as the match continuing when both parties find it preferable to separation. The reform removes a key voluntary exit channel (early retirement), extending employment among older workers, especially those with high specificity.

[10] The theoretical framework presented in this section builds on Nash bargaining models of labor market frictions (e.g., Pissarides (2000)), adapting them to retirement contexts by incorporating outside options shaped by policy. It also draws on Acemoglu/Pischke (1999) in the implications of firm-specific skills for wage setting and turnover, and from Gruber/Wise (2008) the responsiveness of retirement to institutional incentives. Lastly, the interaction between substitutability and tax incidence in determining the incidence of adjustment costs is conceptually linked to Gruber (1997), and is applied here to changes in retirement age.

$$\max_{d_i \in \{0,1\}} \left\{ \pi_i = d_i \cdot \underbrace{(y - o(R_i, s_i))}_{\substack{\text{surplus if} \\ \text{match continues}}} + (1 - d_i) \cdot \underbrace{(-c(s_i))}_{\substack{\text{cost if} \\ \text{match ends}}} \right\} \tag{1}$$

where:

y_i is the worker's output. I abstract from heterogeneity in output across workers, as substitutable workers may be either more or less productive depending on job fit and skill specificity.[11]

The model uses a single specificity parameter s_i to capture employer-side turnover costs. These costs increase when workers are more difficult to replace (due to specialized knowledge or task-specific skills) and when they have limited outside options (due to thin external markets for their skills). In this sense, specificity s_i represents a reduced-form measure encompassing both skill specificity and substitutability.

$o(R_i, s_i)$ is the outside option, shaped by the policy reform R_i and worker specificity s_i. It is decreasing in R_i ($\frac{\partial o(R_i,s_i)}{\partial R_i} < 0$) because pension eligibility is delayed post-reform, and decreasing in s_i because workers with specific skills face thinner external labor markets, especially after age 60 ($\frac{\partial o(R_i,s_i)}{\partial s_i} < 0$). Such specificity could, for example, decrease the likelihood of leaving the social insurance for other pathways than retirement (move to another country, start self-employment, etc.).[12]

$c(s_i)$ denotes the replacement cost of an employee, increasing in specificity, and reflecting dismissal, severance, and other compensation payments, as well as hiring, training, and productivity ramp-up costs that rise with the degree of human capital specificity ($\frac{\partial c(s_i)}{\partial s_i} > 0$).

Solution. The match continues if the joint surplus from continuing exceeds the cost of separation:

$$y - o(R_i, s_i) + c(s_i) > 0 \tag{2}$$

Interpretation. While the equation is modeled as a firm-side optimization problem, the

[11] I thank Wolfgang Dauth for this discussion.
[12] Such argument is also in line with the finding of literature on displaced workers: those unemployed who switch to another industry or occupation experience much larger declines in earnings (e.g. Neal (1995); Addison/Portugal (1989)).

interpretation reflects a joint agreement between the firm and the worker: continued employment occurs when both benefit relative to separation. The reform shifts this condition by removing early retirement as a fallback, thus altering the outside option $o(R_i, s_i)$ and increasing the likelihood of match continuation, especially for workers with high specificity.

Implication 1: A higher ERA rises employment of older workers.

$$\frac{\partial}{\partial R_i}\left(y - o(R_i, s_i) + c(s_i)\right) = -\underbrace{\frac{do(R_i, s_i)}{dR_i}}_{<0} > 0 \tag{3}$$

Delaying pension eligibility reduces $o(R_i, s_i)$, making continued employment more attractive to the firm and less avoidable for the worker, thereby increasing employment.

Implication 2: Workers with higher specificity (s_i) are more likely to remain employed.

$$\frac{\partial}{\partial s_i}\left(y - o(R_i, s_i) + c(s_i)\right) = -\underbrace{\frac{do(R_i, s_i)}{ds_i}}_{<0} + \underbrace{\frac{dc(s_i)}{ds_i}}_{>0} > 0 \tag{4}$$

Less substitutable workers are more likely to remain in employment due to both weaker outside options and higher replacement costs. As a result, the joint surplus of continued employment is larger, sustaining the match.

2.2.2. Wage determination under Nash bargaining

Conditional on retention ($d_i = 1$), the firm and the worker bargain over the wage w_i based on the total surplus generated by employment:

$$S_i = y - o(R_i, s_i) \tag{5}$$

With worker bargaining power $\beta \in (0, 1)$, the Nash wage splits the surplus between the worker and the firm. The wage thus depends on both the worker's productivity y and their outside option $o(R_i, s_i)$. The general form of the Nash-bargained wage is:

$$w_i = \underbrace{\beta \cdot y}_{\text{productivity-based reward}} + \underbrace{(1-\beta) \cdot o(R_i, s_i)}_{\text{outside-option fallback}} \tag{6}$$

Intuition. The worker's wage is a weighted average of what they contribute to the firm's output (through y) and what they could earn elsewhere (via $o(R_i, s_i)$). Workers with high productivity naturally command higher wages, all else equal. However, their outside option—such as retirement income or alternative employment—also determines their bargaining position. If their fallback option weakens, the firm can offer a lower wage even if the worker is productive.

To understand how wages change due to the reform and differences in substitutability, I examine how w_i responds to changes in R_i and s_i.

Implication 3: The reform lowers wages via weaker outside options.

$$\frac{\partial w_i}{\partial R_i} = (1-\beta) \cdot \underbrace{\frac{do(R_i, s_i)}{dR_i}}_{<0} < 0 \tag{7}$$

When the policy raises the early retirement age (i.e., increases R_i), the outside option $o(R_i, s_i)$ declines. This reduces the worker's fallback position in wage negotiations, shifting surplus toward the firm. The wage falls even though the worker remains employed. This mechanism is stronger when the worker has low bargaining power β, and when the reduction in outside options is large.

Implication 4: The effect of specificity on wages is ambiguous.

$$\frac{\partial w_i}{\partial s_i} = \beta \cdot \frac{dy(s_i)}{ds_i} + (1-\beta) \cdot \frac{do(R_i, s_i)}{ds_i} \tag{8}$$

Higher specificity s_i affects both productivity and outside options. If more specific workers are more productive ($\frac{dy(s_i)}{ds_i} > 0$), then wages may increase through the first term. However, specificity also reduces outside options ($\frac{do(R_i,s_i)}{ds_i} < 0$), which lowers wages through the second term. If outside options deteriorate faster than productivity improves, or if the worker has low bargaining power, the overall effect on wages may be negative.

Intuition. Even if the worker is valuable to the firm (due to difficult-to-replace skills), the firm may exploit their lack of external alternatives. The reform amplifies this asymmetry by removing early retirement as a viable fallback, especially for workers in thin external labor markets (e.g., managers, specialists). This is a form of *monopsony power*, where the employer's ability to set wages below marginal product is strengthened by the worker's limited exit options.

Summary. Implications three and four jointly imply that the wage response to the reform depends on the interaction between substitutability and bargaining frictions. For workers with low specificity (who are easy to replace), wages fall mostly due to the loss of retirement options. For workers with high specificity, the story is more nuanced: while their productivity makes them costly to replace (increasing employment), their weakened fallback position gives the firm the leverage to suppress wages. Thus, employment may rise while wages fall or stagnate, despite high skill specificity, due to increased employer monopsony power post-reform.

3 Data

This section consists of two parts. First, I describe the data I utilize, its sampling procedure, and its suitability to my research question. Second, I describe how I constructed my sample, 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), a random 1.5% sample of all establishments in Germany. The establishment identifiers are fixed by industry, ownership, and location at the municipality level; hence, an establishment is not equivalent to a firm in all cases. Nevertheless, I use the terms firms and establishments interchangeably. Employers are obliged to report data on all of their employees subject to social security contributions. Self-employed and civil servants are not covered by the data. At the end of each year, employers report the start and end date of employment, wages, and other occupational, educational, and demographic indicators of all of their workers. Typically, the data is a snapshot of the employment state as of June 30th of each year. Employers are also obliged to report changes in employment contracts.[13]

[13] One of the data limitations is the lack of working hours; hence, I am limited to the analyses of only the extensive margin of employment.

For each of these establishments, the entire employment biographies of all employees are included over the observation period 1975-2018 for West Germany and 1992-2018 for East Germany. Hence, the data also include the establishments that did not constitute the random 1.5% of the establishments originally sampled, in case the workers from the establishments originally sampled were ever employed elsewhere. Observing the entire workforce of the sampled establishments is critical for my analyses, because I study substitutability mechanisms behind employment reactions to the raise in retirement age, which requires observing all coworkers of a given establishment.
Schmidtlein/Seth/Vom Berge (2020) describe the data sampling in more detail.

3.2 Sample construction for analyses

To construct the final sample for my analysis, I keep only women born in 1951, the control group, i.e., women who were potentially eligible for wfor the women's pathway to early retirement, if they accumulated enough years of social security contributions in later life; and 1952, the treatment group, i.e., women who experienced the rise in the women's ERA. I drop women who were ever employed as miners and sailors (for clarity) because their retirement rules differ from those in other occupations.[14]

To address the issue of parallel spells in the data, which is possible, for example, due to dual earners (employed at several establishments simultaneously), I keep the spells in the randomly selected 1.5% establishments. If both spells come from randomly sampled establishments, I keep the spells where the worker accumulated more tenure. In cases where the employee works in two randomly selected establishments and has accumulated an equal amount of tenure in each of them, I keep the job with the highest wage. Dropping parallel spells allows me to construct Panel data and study the firm mechanisms for only the establishments to which the dual workers are more attached.

The final data consists of person-age entries (in age-month), where I observe women from the age of 42 (age-month 504) 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 employment (1) before the reform announcement, (2) between the reform announcement and its inaction at

[14] The seminal work by Geyer/Welteke (2021) on labor supply responses to the 1999 reform makes a restriction of keeping only women who are eligible for the women's pathway to retirement at the age of 60. I make restrictions that proxy for eligibility, following Lorenz et al. (2018). I do not explicitly make sample restrictions that keep the women eligible for the women's pathway (e.g., 15 years of contributions in total and ten years after 40 years old, etc), because I do not observe the unemployment spells that also contribute to the contribution years. Because unemployment spells still count towards the contributions to social security, not making this restriction results in smaller treatment effects in my sample, compared to that of Geyer/Welteke (2021)

60, (3) and workers who continue working beyond both the ERA (60 or at least 63) and NRA (65 or 65.5). First, studying employment before the reform announcement shows whether the treatment and control groups had different labor supply frequencies before the reform announcement. Second, studying employment between 47-60 can show whether the rise in ERA leads to different employment choices during middle age, in expectation of a longer employment period. Finally, studying the effects beyond the new ERA shows how the effect of raising ERA also spills over to post-ERA employment, which could show indirect employment effects beyond the age targeted by the reform, further increasing its effectiveness in keeping workers in employment longer.

I keep workers who are continuously (in each age-month) employed at 58 and 59. To make such restriction plausible, I have to assume that the employment at 58-59 is not is unaffected by the reform. Geyer/Welteke (2021) show that there are no employment effects before the age of 60, therefore, such restriction is not likely to lead to a selection bias. Because most of the main heterogeneity variables are constructed at the establishment level, this restriction helps me to obtain a sample of workers with sufficient attachment to their establishments. The final data consists of 32,770 workers, and 9,036,582 worker-age months (Table B1 records the number of workers after each restriction). Out of these workers, 15,640 are in the control group (born in 1951), and 17,130 are in the treatment group (born in 1952).

4 Identification

First, I describe the identification strategy based on reform discontinuity in birth dates, and then I provide some descriptive results that confirm the presence of discontinuity in the data.

4.1 Regression discontinuity design

I follow Geyer/Welteke (2021) to locally identify the effect of the reform that raised the ERA on employment, τ_m, in an RDD framework[15]:

[15] There are several differences from the identification in Geyer/Welteke (2021). First, I do not control for the presence of children in my RDD regression as I do not observe such variables in the data. Second, because the most recent year observed in my data is 2018, the data allow me to pool all the age months corresponding to 60-62 years of age in the baseline regression and beyond 63 in the supplementary analyzes, while Geyer/Welteke (2021) pooled only 60-62 due to their right-censored data in 2016. Finally, I use the mean square-based optimal bandwidth, while they use a 12-month ad-hoc bandwidth selection procedure.

$$
\begin{aligned}
y_{im} = \alpha_m + \tau_m \mathbb{1}\{b_i \geq b^*\} + \\
+ \beta_{0m} \mathbb{1}\{b_i < b^*\}(b_i - b^*) + \beta_{1m} \mathbb{1}\{b_i \geq b^*\}(b_i - b^*) + X_i' \beta_m + \epsilon_{im}
\end{aligned}
\tag{9}
$$

where y_{im}- is employment state, recorded for each woman i at every age-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 after the cutoff b^* (January 1952), i.e., experienced the rise in the ERA (treatment group); while $\mathbb{1}\{b_i < b^*\}$ includes the individuals who are below the cutoff (control group). I use a local linear regression, and by interacting the running variable $(b_i - b^*)$ with the treatment indicator, I allow for different slopes in treatment and control groups. Figure 1 shows that a linear trend in the running variable is a plausible assumption, and there is a clear discontinuity that is unlikely to be attributed to a wrong functional form of polynomials. To compute the RDD estimates, I use a triangular kernel function and the optimal bandwidth choice based on mean square error (Imbens/Kalyanaraman, 2012). As a result, I calculate the bias-corrected RDD estimates with a robust variance estimator.

I also control for calendar month, a dummy for Western German residence, wages at the age of 46, and two education categories (out of 3), because previous literature confirms that education is an important determinant of employment at an older age (Geyer et al., 2022). I cluster the standard errors at the birth month level to account for the potential correlation of standard errors ϵ_{im} for the women belonging to the same birth cohort.[16] In robustness and sensitivity checks, I re-run the regressions, altering all the specification parameters- the procedures for estimating the parameters and covariance matrices, polynomial order, kernel weights, bandwidth choice, included covariates, and clustering level.

The baseline regressions pool the 60-62 age (720-756 age months) together, because this is the age frame that was affected by the ERA reform. This identification results in a local average treatment effect of higher ERA on employment outcomes at ages 60-62 (coefficient τ_m in equation Equation 9).[17]

Identification assumptions. This identification relies on two main assumptions.

(1) Smoothness in density. This assumption requires 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. This assumption holds by construction

[16] Clustering at the level of birth dates aligns with literature suggesting clustering the standard errors at the treatment level.

[17] Because I cannot claim that all the women included in my sample were eligible for women's pathway to early retirement, the coefficient could also capture the Intention-to-Treat (ITT) effect. However, Lorenz et al. (2018) show which sample restrictions are likely to lead to eligibility imputations, and because most of my restrictions match their proposed restrictions, my sample likely captures most of women eligible for the women's pathway to early retirement.

because it is impossible to change one's own birth date.[18] Nevertheless, in the sensitivity tests, I re-estimate the main regressions by omitting the observations close to the cutoff and confirm the robustness of the results.

(2) Smoothness in covariates. This assumption requires continuity of the distribution of the observed and unobserved variables around the threshold, showing that the assignment of the treatment around the cutoff is as good as random. Table B2 shows that there is no sizeable significant discontinuity in pre-determined variables. In particular, I choose a variable showing whether a woman has Western origin (proxied by the place of living according to the first biographical spell) and nationality, as these variables are fixed over time and hence are pre-determined.

Main outcome variables. In terms of outcome variables, at each age month, I create three mutually exclusive main labor market categories - employment, nonemployment, and retirement. I further disentangle the employment into three 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 with the last labor market activity of a worker. Figure A2 displays the evolution of the three main employment states over age by treatment status, i.e., the gap in employment and retirement statuses at 60-62.

In addition to these employment state categories, I also define wages, because I am also interested in wages conditional on employment.[19] Wages are created at the detailed monthly level, and are non-zero only if the worker is employed.

Effect heterogeneity. To study the mechanisms behind these effects, I perform subsample analysis using several categories of variables, which show turnover costs associated with retirement in the next section. Because the research question relates to the labor demand factors influencing employment at ages 60-62, I define these variables at the age of 58, just before the pre-reform retirement age of 60.

[18] One could argue that the reform cohorts could be chosen by policy-makers in a way that violates the assumption, for example, by the cohort of baby-boomers, etc. However, because I compare cohorts born around the cutoff, and the cutoff does not appear in any other reforms, policies, or characteristics (both of these cohorts are typically classified in the baby-boomer generation) that would make the 1951 cohort different from the 1952 cohort, there is no reason to believe that the assumption is likely to be violated.

[19] Although wages are top-coded in the social security data, this data feature is unlikely to constitute an issue for the analyses as women are less likely to cross the threshold for wage censoring.

4.2 Descriptive evidence on the presence of discontinuity

The abolishment of women's pathway to early retirement led to a large increase in employment rates at 60-62, as shown in the right Panel of Figure 1. While overall there is an upward-sloping employment trend at 60-62 over the birth cohorts, there is also a clear discontinuity around the 1952 cohort. Only around 75% of women born in 1946-1951 were employed at 60-62[20]. However, the employment rate jumped to approximately 90 percent starting with the 1952 cohort, the reform cutoff.

Figure A3 extends the analyses to display employment rates by treatment status at all age months (corresponding to the ages between 42 and 66), and confirms the presence of a discontinuity in employment rates at 60-62 (due to the 1999 reform that I study) and to a smaller magnitude of discontinuity 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 the next section, I causally quantify the largest employment discontinuity that happens due to the 1999 reform, i.e., at 60-62.

5 Results

In this section, I first focus on the effect of the 1999 reform on employment, confirming the results of prior studies on this reform (Geyer/Welteke, 2021). I show the effects of retirement on employment trajectories before studying the labor demand mechanisms of employment, because I want to provide a general picture of the labor supply behavior overall before zooming in on the total employment mechanisms.

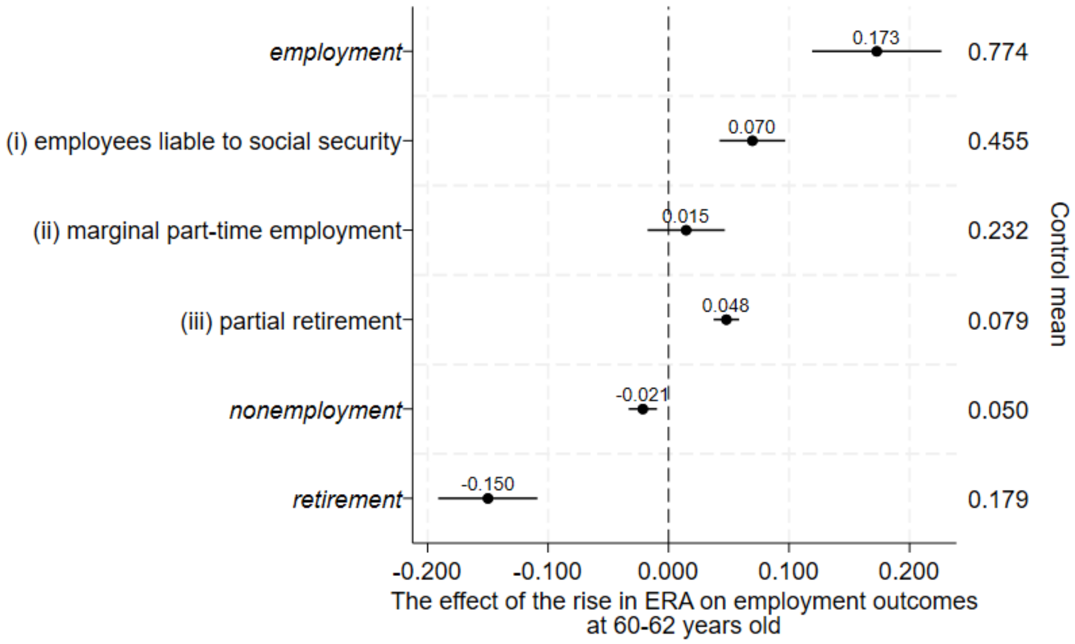
Based on the theoretical framework, I expect that the rise in the ERA should extend employment among affected workers, particularly those whose exit would impose high turnover costs on firms. These costs are likely higher for workers with specific skills or those employed in occupations with limited internal or external substitutes. Therefore, I expect the employment effects of the reform to be stronger for such workers. On wages, the model predicts ambiguous effects depending on workers' outside options and replacement difficulty: lower bargaining power due to the loss of pension eligibility may lead to wage decreases, while high replacement costs for specific or non-substitutable workers could result in wage premiums to incentivize retention.

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

5.1 The effect of the rise in ERA on employment states

I start by analyzing how the employment states (employment, non-employment, and retirement) change at the ages targeted by the retirement reform, i.e., at 60-62; hence, I confirm the result of Geyer/Welteke (2021).[21] In this section, I analyze several employment states as outcome variables- (1) *employment* (which is further disentangled into *employment subject to social security*, *marginal part-time employment*, and *partial retirement*), (2) *nonemployment*, and (3) *retirement*[22].

Figure 2.: The effect of the rise in ERA on the employment state (overall and from each category)



Notes: Coefficient plots. Each row corresponds to the RDD regression of the share of the employment state of the corresponding category (left axis) around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 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 state in the corresponding category over the control group (born in 1951). A corresponding table with more details can be found in Table B3.

[21] I pool the ages 60-62 together because when analyzing the employment effects separately by performing RDD for each age-month in Figure A5, there are two 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 60-62, corresponding to the effects of the rise in ERA per the 1999 reform, while the rise at 65-65 years and six months corresponds to the 2007 reform's NRA response. Even though the 2007 reform resulted in an NRA rise for the same cohorts under study, the direction of effects is the same and is unlikely to cause any threat to the identification of the 1999 reform under study. Analyzing the effects of the 2007 reform is beyond the scope of this paper.

[22] See chapter 4 for more details about these variables

The right column of Figure 2 shows the causal effect of the rise in ERA on employment statuses at 60-62. I find that 77.4% of women in the control group (born in 1952) are employed at 60-62, while 5% are non-employed, and 17.9% are already retired. Higher ERA leads to an increased likelihood of being employed at 60-62 by 17.3 percentage points (pp) ($p < 0.01$; a 22.4% increase relative to the control mean).[23] Although most of the increase in employment is attributed to employment subject to social security, i.e., 7 p.p. ($p < 0.01$; a 15.4% increase relative to the control mean), there is also some evidence for an increase in partial retirement claims by 4.8 p.p. ($p < 0.01$; a 60.8% increase relative to the control mean). Hence, the employees respond at the extensive margin, but not necessarily the intensive margin of employment.

The likelihood to retire at 60-62 falls by 15 p.p. ($p < 0.01$; an 83.8% decrease relative to the control mean), and there is a negative effect on nonemployment: 2.1 p.p. ($p < 0.01$; 42% decrease relative to the control mean). Overall, these results show that workers are likely to work longer in response to the reform. In the next subsection, I confirm that the results presented are robust to specification and have a credible specification.

Employment beyond 63, the new ERA. Figure A5 displays the RDD coefficients at each age month. The workers whose ERA rises do not only work until they reach pensionable age, but are also more likely to extend their employment beyond 63 and to bunch at their Normal Retirement Age of 65.5, before the effects fade away at 66.[24]

While the effects of the rise in ERA on employment beyond 62 are smaller than those at 60-62, they are significant. In most of the sections below, I concentrate on employment at 60-62, as this is the main retirement age shift impacted by the reform.

5.2 Robustness and sensitivity checks for the baseline RDD results

Below, I perform robustness and sensitivity tests that confirm the specified model findings by altering specific parameters of the model. In particular, I alter the estimation procedure for the coefficients or variance estimators, the polynomial order, and specified weights in RDD regressions, and cluster levels of standard errors. I also alter the number of covariates included in the baseline regression and remove the observations close to the cutoff, to have a more robust estimation with respect to potential bunching. All the tests indicate that the coefficient estimates presented above are within the confidence intervals of all the

[23] Figure A4 zooms in on the *employment* outcome in a regression discontinuity plot, and confirms once more the presence of a discontinuous jump.
[24] The ineligibility of some women for long-insurance pathway could explain the bunching at the NRA, which is the age when workers on the regular pathway to retirement can begin to claim pensions.

alternative models below. Finally, I perform a falsification test by re-estimating the RDD regressions around placebo cutoffs and find no jumps, confirming the validity of the estimation strategy.

Sensitivity to the estimation procedure. Table B4 shows the sensitivity of estimates with respect to the three different coefficient and variance estimators procedures, and shows that the choice of bias-corrected or conventional coefficient estimates or robust vs conventional variance estimators does not lead to significantly different results.

Sensitivity to the choice of polynomial order. Table B5 changes the linear regressions to the second-order polynomials. Even the 4th order polynomial choice shows discontinuity in the running variable (the second graph in Figure A4). Hence, the discontinuity in the running variable is not due to the wrong polynomial choice.

Sensitivity to the specified weights. Table B6 shows that the estimates with uniform and Epanechnikov kernel function specifications do not significantly differ from the baseline specification that uses triangular weights.

Ad-hoc bandwidths and "donut RD". One of the concerns related to RDD estimation is the potential bunching at the cutoff. To show that bunching would not alter the results, I repeat the estimation and inference without the data points in the area just around the treatment threshold, i.e., the December 1951 and January 1952 birth cohorts, and compare the results to the ad-hoc bandwidth of twelve months.[25] Table B7 confirms that excluding the observations close to the cutoff does not alter the results of regressions with 12-months bandwidth.

Sensitivity to the inclusion of covariates. Table B8 reports an RDD regression (1) controlling for Western German origin and nationality in addition to the covariates in baseline specification (calendar months, western residence and education dummies), and (2) the specification with no control variables at all. I do not have enough evidence to argue that the specification is sensitive to the included covariates, as the confidence intervals in all three specifications include the coefficient of the baseline specification.

Sensitivity to clustering level. In an alternative specification, I cluster the standard errors at the establishment level, which captures the main mechanisms discussed later, as opposed to the birth months in the baseline specification, which captures the treatment level. The significance of results does not change from the alternative clustering method,

[25] The "donut RD" does not work in combination with the optimal bandwidth selection procedure due to missing data around the cutoff; hence the necessity to perform such analyses in comparison with the ad-hoc bandwidth.

and the confidence intervals generated by this clustering method include the coefficients from the baseline regressions (Table B9).

Placebo cutoffs. Finally, I perform falsification tests by using placebo cutoffs. I test whether employment at 60-62 rises for women at the other birth cohort cutoffs who were not affected by the reform.[26] I use the cutoffs corresponding to January 1947, January 1948, January 1949, January 1950, and January 1951, as women were eligible for the women's pathway to retirement at these cutoffs. Table B10 shows that all the placebo cutoffs yield insignificant effects ($p > 0.05$).

5.3 The effect of the rise in ERA on wages

While the direct effect of the reform is to delay retirement and extend employment at older ages, its broader implications also depend on the quality of these additional years in the labor force. Wages provide a natural measure of labor market returns, productivity, potential employer valuation, and bargaining power. Studying wages allows me to assess whether not having an outside option for a pension at 60-62 results in lower bargaining power of workers, and hence, lower wages, as derived in a theoretical model in chapter 2. Below, I extend the analyses of the rise in ERA on wages, conditional on employment.

The estimated discontinuity in wages reflects the effect of the reform on those who remained employed at 60–62. The last column of Table B3 shows that, among those who remain employed, the rise in ERA is associated with 116.522 EUR lower wages (6.8% decrease relative to the control mean). However, because the reform extended employment, the composition of employed individuals may have changed, introducing selection into the observed wage sample. While the observed wage declines are consistent with reduced bargaining power due to fewer outside options, they may also partially reflect increased incidence of part-time work or partial retirement among older workers. Therefore, this effect is not necessarily representative of the impact on wages in the full population due to selection, and I interpret the results with caution.

[26] Table B11 shows the RDD around the 1952 cutoff for male workers. Although male workers were affected by the 1999 reform to a lesser extent than women (due to the abolishment to early retirement programs for workers on 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 one month around the cutoff, so at 60-62 they could extend their employment as a forward-looking approach towards retirement after 65 and five months vs 65 and six months. Still, I report the results, and as expected, there is a discontinuity in the employment at 60-62 years old, but it is very small in magnitude.

6 Labor demand mechanisms: replacement costs

After analyzing the overall employment effects above, as a next step, I investigate the labor demand mechanisms of the employment response to the reform through subsample analyses. As outlined by the theoretical model in chapter 2, workers associated with higher turnover costs are predicted to be significantly more likely to remain employed after age 60 following the reform. This suggests that firms retain older employees when replacement is costly, consistent with a labor demand mechanism in which internal firm frictions shape post-reform employment outcomes. Before the reform, older workers with low substitutability could leave the labor force due to generous early retirement options, despite their high firm-specific value. The reform thus can reduce outside options, making retirement less accessible and shifting the relative cost-benefit calculus in favor of retaining less substitutable workers whose departure would impose higher replacement costs on firms.

I create two groups of labor demand measures - worker job-specific skills and market-level worker substitutability, both of which proxy for turnover and replacement costs for the employers. While neither of these groups is preferred over the other, they show different dimensions of substitutability and complement each other for a fuller picture. Worker skills may be firm-specific; hence, with turnover, some information may be lost, making incumbent workers less substitutable by potential new hires. Meanwhile, the internal and external labor market thicknesses show the availability of potential hires. Searching for suitable replacements in the labor market or through internal hiring is costly due to hiring costs.

For the remainder of the paper, I focus only on *employment* as an outcome variable to study the labor demand mechanisms. I show that women who possess high skills and are more difficult to substitute internally (by coworkers) or externally (by external hires) are more likely to extend their employment years in response to the reform, confirming that the reform helps the firms to avoid replacement costs associated with worker turnover.

6.1 The role of job-specific skills

The first group of variables showing turnover costs and worker substitutability is worker skills. In the presence of firm- and occupation-specific human capital and knowledge that is difficult to substitute for, turnover can be costly for the establishments. Hence, workers possessing such skills may be more likely to remain employed at an older age. I create two measures: (1) return to occupation (which I interchangeably call human capital specificity of occupation); (2) managerial occupations.

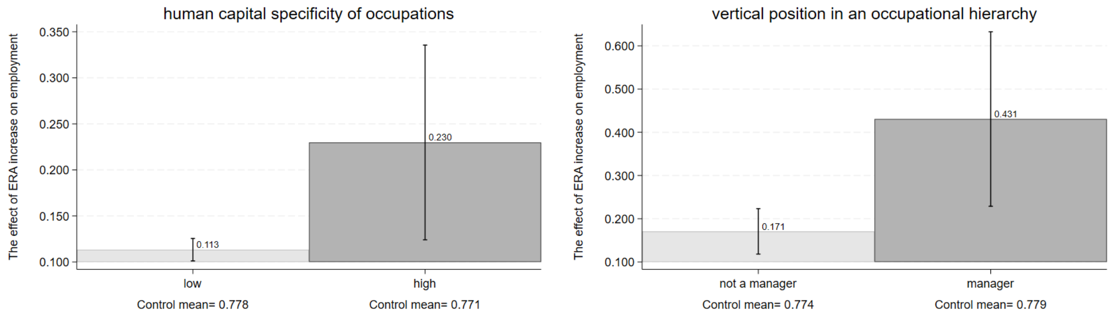
Human capital specificity of occupation. Guvenen et al. (2020) show that wage growth is largely tied to firm and occupation-specific factors, supporting the idea that human capital specificity can shape workers' ability to remain employed after a retirement age increase. If a worker's human capital is very occupation-specific (skills tied closely to their current job/occupation), they are valuable in their current job, and their employer might want to keep them because their replacement would be costly. In terms of the worker's perspective, they may have higher returns to staying and face difficulties in switching occupations if needed. In contrast, if a worker's human capital is more general (easily transferable skills), they can more easily move to other jobs if needed, and employers might replace them more easily followed their retirement.

Human capital specificity, proxied by the return to experience, thus constitutes an important mechanism moderating the effects of retirement age reforms on employment outcomes at older ages. To obtain a measure of the human capital specificity of an occupation, I follow a strategy similar to those used by Jäger/Heining (2022) and Bleakley/Lin (2012) to estimate Mincer equations for each of 3-digit occupations.[27] I use the occupation-specific returns to experience, which essentially quantify the impact of on-the-job training and skill accumulation on an individual's wage, and classify the specialization as high if this return is greater than the median value (0.12, i.e., 12% increase in wages associated with an additional year of experience).

I examine treatment effects separately for occupations requiring high levels of specific human capital and those that require less specific human capital to investigate potential heterogeneity. The left Panel of Figure 3 displays that the workers employed in occupations with above median value of returns to experience have significantly higher employment effects. Among the occupations requiring less specific human capital, the treatment increases employment by 11.3 percentage points (14.5% relative to the control mean of workers who did not experience the rise in ERA and were employed in occupations requiring low human capital specificity). For workers with high human capital specificity, the treatment effect is 23 p.p (29.8% increase relative to a control mean). The difference in point estimates (11.7 p.p.) suggests that the employment response to the treatment is substantially larger for the workers performing occupations requiring specific skills. Thus, human capital specificity (proxied by returns to experience) moderates the effect of retirement age reforms.

[27] Given my smaller sample size, I use only 3-digit occupations as opposed to Jäger/Heining (2022), who use 5-digit occupations.

Figure 3.: The effect of the rise in ERA on employment at ages 60-62 by return to experience in a given occupation and occupational hierarchy level



Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and the highest education. The subsample analysis in the left Panel is performed by the human capital specificity of occupation. The right Panel stands for managerial status. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B12.

Managerial status. Managerial occupations often entail a higher degree of firm-specific and occupation-specific human capital, due to their reliance on accumulated institutional knowledge, leadership skills, and relationship-specific investments within the firm. Managers are typically more difficult to replace than are non-managers, particularly at older ages when experience and firm-specific knowledge peak. Therefore, distinguishing between managers and non-managers offers a meaningful way to capture heterogeneity in turnover costs and the value of worker retention following a rise in the early retirement age. Even if two workers have the same returns to experience, managerial roles may imply extra firm-specific value.

In a related study, Jäger/Heining (2022) find that the death of a manager or a worker in a specialized occupation results in more negative effects on the coworkers in other occupations. In my setting, if a worker is a manager, she likely has many coworkers under her hierarchy, communicates with them more, and has more information, making her less substitutable, and thus making the extension of her working life more valuable. I create a variable showing managerial or supervisory status based on the last two digits of the 5-digit occupations. I pool the supervisors and managers into the dummy variable *manager*.[28]

[28] Depending on occupation type, some occupational hierarchies have managers, while others have supervisors as the highest occupation level in a hierarchy. I thank Philipp vom Berge for the help with the data.

The right Panel of Figure 3 shows that workers in managerial positions are significantly more likely to remain employed at older ages in reaction to the reform. The workers in managerial positions extend their employment by 43.1 p.p. (55.3% relative to the control mean- the managers whose retirement age was not altered by the reform), while the non-managers raise their retirement ages by 17.1 p.p. (22.1% increase relative to the control mean). The difference in point estimates (26 p.p.) suggests that the employment response to the treatment is substantially larger for workers performing managerial occupations.

Alternative measures of skills and specificity. To test whether the results presented above are sensitive to approximating worker skills, I explore alternative proxies for worker skill specificity. The baseline analysis relies on hierarchical job positions as indicators of skill-specific roles. As an alternative, I use an occupational classification by Blossfeld (1985). This classification groups occupations into ten categories and shows the occupational split by required skills- simple vs professional.[29] Figure A6 shows that across all occupational groups, workers in skilled (i.e., professional) categories exhibit greater employment gains after the reform than those in corresponding simple roles. Managers and professionals are particularly likely to remain employed longer, reinforcing the idea that skills and job specificity drive retention. Although one might suspect that this is driven by longer job tenure, Figure A7 shows that employment gains do not increase monotonically with tenure. This suggests that hierarchical position captures more than tenure alone.[30]

I further explore whether employment responses differ across occupational task types, offering another dimension of worker substitutability. Following Dengler/Matthes/Paulus (2014), I categorize jobs along two dimensions: (i) skill content—analytical, interactive, cognitive, or manual tasks—and (ii) routineness—routine vs non-routine.[31] High-skilled workers typically perform analytical or interactive tasks, while low-skilled workers perform manual non-routine tasks. Figure A14 shows that workers in high-skill task occupations experience the largest post-reform employment extension. In contrast, workers in routine occupations—often more replaceable by automation—do not exhibit systematically different employment responses. This suggests that, in the studied period, task routineness and the potential for automation play a lesser role in driving employment effects than overall skill specificity. While automation may become a more relevant channel in the future, the evidence here points primarily to skill-related substitutability as the key mechanism.

[29] I use the codes from material published by Schmieder/von Wachter/Bender (2016) to implement this classification. Education level is not a suitable candidate for skill differentiation in this context, as it is directly controlled for in the baseline specification due to institutional reasons (chapter 2 and chapter 4). The results by education level are shown in Panel C of Table B22 and exhibit no meaningful differences.

[30] Tenure is an imperfect proxy for skills in this context because eligibility for retirement at age 63 depends on tenure. Thus, its use conflates eligibility rules with substitutability.

[31] This classification is matched to my main data using the 3-digit occupation identifier. Task types include analytical non-routine, interactive non-routine, cognitive routine, manual routine, and manual non-routine.

6.2 The role of internal and external substitutability

The next group of variables showing the turnover costs and substitutability of workers is based on the markets- internal (by availability of coworkers in the same job cell as an older woman) and external (potential hires in the local labor market). The main motivation for studying internal labor market thickness is that the scarcer the job performed is, the more difficult it is for the employer to replace potential retirees with coworkers, thus leading to higher employment responses to the retirement reform. Internal substitutability is particularly important, as internal workers are imperfect substitutes for external workers (Jäger/Heining, 2022); hence, often the internal substitutes weigh more than the external substitutes. When fewer workers are working in the specific occupation of an older woman in the commuting zone, the less substitutable such a woman is. Similarly, when fewer workers are working in the specific industry of an establishment in local labor markets, the less substitutable the older women of such establishments are by external hires.

Availability of internal substitutes.

To capture internal substitutability, I use the number of available coworkers in the same 3-digit occupation as women born around the reform cutoff. I count only workers in employment positions subject to social security. Following Huebener et al. (2024), I define three categories of such variables by the availability of coworkers in the same 3-digit occupation as the affected women: 0, 1-4, and 5 or more internal substitutes. I perform the analyses for establishments with fewer than 100 workers, as the levels of substitutability will be less dependent on establishment size (such restriction also closely follows Huebener et al. (2024) definitions).

The left Panel of Figure 4 shows that when there are no coworkers who perform the same job as the older workers, the older workers are more likely to remain employed at 60-62 following a retirement reform. The group with more than five substitutes has significantly lower employment responses than those with 0 coworkers in the given job cell. Workers who have no internal replacements respond to the reform by extending their employment by 26.8 p.p. (35% increase relative to the control mean of workers who were allowed to retire at 60 and were employed in non-substitutable establishments). While the effects are insignificant for the group of workers who have between one and four coworkers, the workers who have more than five coworkers in the same occupation extend their employment by 6.5 p.p. (7.9% increase relative to the control mean). The difference in point estimates (20.3 p.p.) suggests that the employment response to the reform that raised the ERA is substantially larger for workers who have no internal substitutes, relative to those who have at least five internal substitutes, in line with the prediction that firms retain workers who are more difficult to replace.[32]

Figure 4.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by number of internal and external substitutes for the given occupation

Notes: Coefficient plots from RDD regressions around the January 1952 cutoff. The estimates are obtained using local linear regressions with first-order polynomials, a triangular kernel, and mean square error–optimal bandwidth selection. Controls include calendar month of birth, Western German residence, wages at age 46, and education. Subsample analyses are conducted by internal substitutability in the left Panel and external substitutability in the right Panel. Internal substitutability is measured by the number of coworkers in the same 3-digit occupation as the old worker, restricting the sample to establishments with fewer than 100 workers. The right Panel shows *external labor market thickness (ELMT)*, based on the commuting zone at most half as concentrated in a given occupation relative to the country-level ($ELMT < 0.5$), or at least half as concentrated but less concentrated than the country-level ($0.5 < ELMT < 1$), and at least as concentrated as the country-level concentration ($ELMT > 1$). Vertical lines represent 95% confidence intervals based on robust standard errors clustered at the birth-month level. Control means (on the x-axis) refer to the average employment rate at ages 60–62 among the control group within each group's optimal bandwidth. The corresponding detailed tables are reported in Table B13 and Table B17.

External labor market thickness (ELMT). I define ELMT in two steps. First, I create 141 local labor markets based on high within-region and low between-region commuting for work, following Kropp/Schwengler (2011). Next, I create an index $ELMT_{kc}$, showing the local labor market share of 3-digit occupation (or industry) employment (E_{kc}/E_c) over the national share of occupation (or industry) employment (E_k/E). I count only workers between 18 and 64 years old who are either in employment subject to social security contributions or trainee workers.

[32] Figure A9 repeats the analyses for all establishments, regardless of size. When at least five coworkers perform the same job as a woman, the effects are still large, despite being slightly smaller (but not significantly smaller) than those of women with no internal worker substitutes. This pattern could be driven by the variation in treatment effects by establishment size. Indeed, in larger establishments, women are more likely to work longer in reaction to the reform than those in smaller establishments (Figure A8); hence, when analyzing internal substitutability, it is important to account for the establishment size by restricting the sample to those with at most 100 workers. Even in large establishments, if there are no internal substitutes, the effects are quite large, which highlights that, although in large firms workers stay in employment longer, those who have no substitutes still work longer regardless of the establishment size.

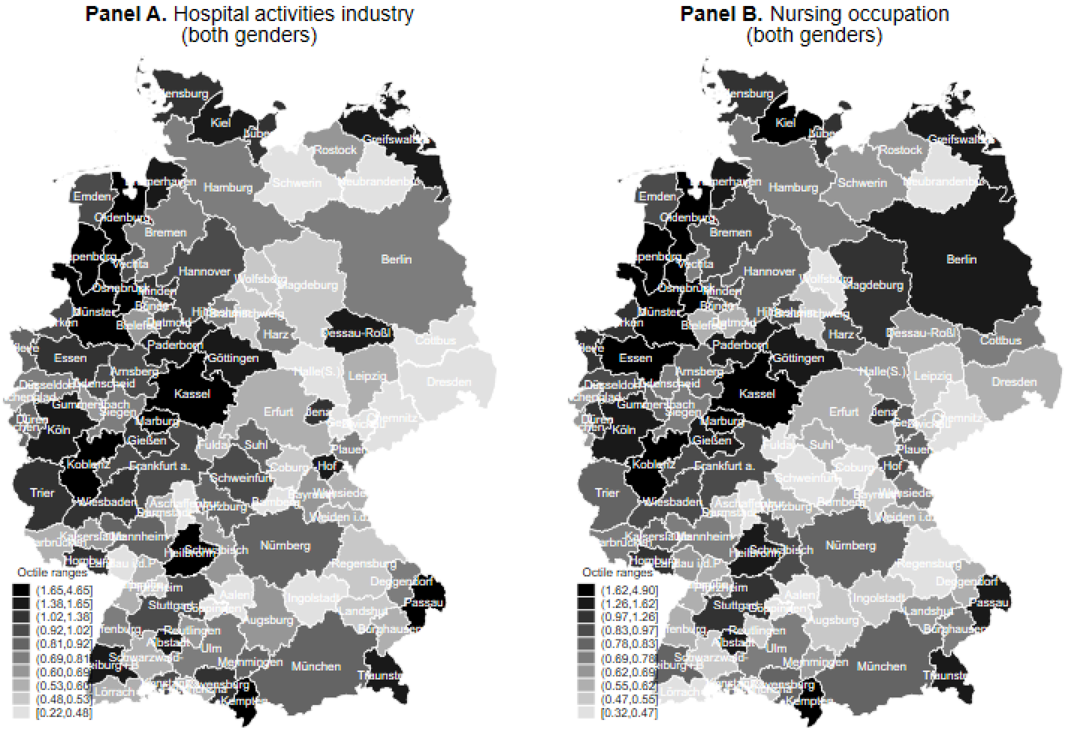
$$ELMT_{kc} = \frac{E_{kc}/E_c}{E_k/E} \tag{10}$$

where k is a 3-digit occupation (or industry), and c is a commuting zone, E_{kc} shows the number of workers employed in the occupation (or industry) k, and in the commuting zone c, E_c is the number of workers employed in the commuting zone c and all the occupations (or industries) together, E_k is the number of workers employed in the occupation (or industry) k in all the commuting zones together, while E is the number of workers employed in all the occupations (or industries) and all the commuting zones together (i.e., country).[33]

Figure 5 displays an example of this index construction for the nursing occupation and hospital activities industry. While Passau has many workers employed in these industries relative to the national level, Leipzig does not. This means that for an establishment located in Leipzig, an older worker in a given occupation and industry is more valuable (i.e., such a worker is associated with higher turnover costs) than for an establishment located in Leipzig. I call an external labor market thick if this index is over 1, i.e., if the thickness of an occupation (or industry) in a given commuting zone is denser than the thickness at the national level. Additionally, I define a group where the index $ELMT_{kc}$ is below 0.5 (i.e., the commuting zone at most half as concentrated in a given occupation or industry as the country-level), between 0.5 and one (at least half as concentrated but less concentrated than the country-level).

[33] All of these variables are defined based on my SIEED data, but becuase the sample is representative of all German establishments in the country (and the random sampling provides representativeness of workforce subject to social security at the commuting zone level), I expect these indices to proxy the country-level index well.

Figure 5.: External labor market thickness by German industry and occupation in 2010



Notes: This map shows the computed external labor market thicknesses (ELMT) for each of the 141 local labor markets based on the Kropp/Schwengler (2011) classifications, which are constructed based on high within-region and low between-region commuting. I compute ELMT based on Equation 10 for the industry and occupation largest share of female employees: “Hospital activities industry” (**left Panel**) and “Nursing occupation” (**right Panel**). I plot the ELMT indexes (Equation 10) on the map based on the ten deciles presented in the left corner of each graph.

The right Panel of Figure 4 displays the RDD results split by external labor market thicknesses of occupations. If women are employed in a commuting zone at most half as concentrated in a given occupation as the country-level, they extend their employment by 41.3 p.p. (58.4% increase relative to the control group). I find that if a woman is employed in a commuting zone at least half as concentrated but less concentrated than the country-level, the employment increase is 15.6 p.p. (19.9% increase relative to the control mean). Finally, in the commuting zones in which a given occupation is more represented than at the national level, the reform leads to an 11.7 p.p. increase in employment at ages 60- 62 (15.3 % increase relative to the control mean). This increase in employment is 29.6 p.p. lower than in commuting zones at most half as concentrated in a given occupation as the country-level. This result indicates that the response to the reform that raised the retirement age is higher for workers in occupations with thin external labor markets, where they are less substitutable than in thicker markets.

I examine heterogeneity in employment effects along external labor market thickness at the industry level (Figure A11). Unlike the baseline occupation-based results, which showed clear differences by substitutability, I find no significant heterogeneity in responses across industries with different levels of labor market thickness. One potential explanation is that industry-level measures are too broad to capture substitutability for specific skills or tasks. Additionally, larger firms, which are included in the full sample, may be less affected by external labor market conditions because they can rely more on internal replacement options.

To account for this concern, I re-estimate the analysis for a subsample of establishments with fewer than 100 employees, where firms are less likely to rely on internal labor markets. In this subsample, the effects of the reform do differ significantly by industry-level labor market thickness: I find that workers are more likely to remain employed in industries in which the external labor markets are thin. (Figure A12). This suggests that external substitutability matters more when firms face tighter external constraints and cannot rely on internal hires.

Overall, the occupation-based measure of external substitutability remains more informative than the industry-based measure, because thick industry labor markets may reflect broader agglomeration patterns rather than job-level substitutability. Moreover, industry thickness may not map well onto the specific skills that firms need to replace.

Gender-specific substitutability. The main results rely on gender-neutral measures of external labor market thickness (ELMT), pooling employment densities of both men and women. However, Germany exhibits pronounced occupational and industry segregation by gender, and the reform exclusively affected women. If women face limited competition or hiring barriers in male-dominated fields, their effective substitutability may depend on the gender composition within occupations and establishments.[34]

To explore this, I construct a gender-specific version of the ELMT index using only female employment densities (a modification of Equation 10) and re-estimate the main analysis across the previously defined three ELMT categories. As expected, the variation in the female-specific ELMT is smaller, and the results become statistically insignificant (Figure A13). One possible explanation is that employers do not confine their replacement pool to women and may consider male hires instead. In such cases, a gender-neutral ELMT measure may better reflect the labor supply elasticity firms actually face.

However, this interpretation is not definitive. Relying solely on female data reduces statistical power, and the resulting ELMT measure may be noisier and less correlated with true substitutability. Therefore, I cannot fully assess gender-specific substitutability with precision in this setting.

Nonetheless, to test whether gender segregation interacts with employment responses, I perform additional subsample analyses by the gender dominance of occupations and establishments.[35] I find no significant differences in employment responses between male- vs female-dominated contexts. One possible interpretation is that, conditional on occupation and firm size, women and men are generally substitutable from the firm's perspective, and the substitutability measures used in the baseline are robust to gender composition.

Does the external substitutability matter beyond the local level? Tradability of industries. The results above show that workers employed in less substitutable occupations in a given local labor market are more likely to extend their employment in response to the reform. I analyze the broad industry groups and discuss the results in terms of the conventional classification of industries by tradability to test whether the workers in tradable industries are more likely to respond to the raised retirement age. Such analyses allow me to test whether external substitutability matters beyond the local level. In tradable industries, firms can replace workers not only locally but also by outsourcing tasks globally, increasing substitutability (Drenik et al., 2023). I classify the industries by tradability following Gregory/Salomons/Zierahn (2022).[36] Figure A15 shows no difference between tradable and

[34] For example, Illing/Schwank/Tô (2024) find gender gaps in wages at the hiring stage for vacancies created by worker deaths in Germany.
[35] I follow Tophoven et al. (2015) and define gender-integrated occupations or establishments as those in which the proportion of men and women ranges from 21% to 79%. Gender-dominated occupations or establishments are those in which the share of one gender exceeds 80%.
[36] *Tradable industries* are: Mining (WZ08: B), Manufacturing (WZ08: C), Electricity, water supply (WZ08: D, E), Transport, storage (WZ08: H), Financial services (WZ08: K), Real estate (WZ08: L), Agriculture (WZ08: A), Information and communication (WZ08: J), Scientific and technical services (WZ08: M). *Non-tradable* industries are Construction (WZ08: F), Wholesale and retail trade (WZ08: G), Hotels, restaurant (WZ08: I),

untradable sectors. The result implies that substitutability does not matter beyond the local level when it comes to the effects of the reform on remaining in employment after 60.[37]

To conclude, I find that job-specific skills and low internal and external substitutability are associated with a stronger increase in employment at ages 60–62 following the reform. While the analysis captures equilibrium effects — that is, match-specific attributes shaped by both worker and firm — the pronounced retention of managers and specific workers is consistent with higher replacement costs, pointing to an important role for labor demand frictions.

6.3 The effect of raised ERA on wages by replacement costs

The theoretical framework in chapter 2 predicts that raising the early retirement age weakens older workers' outside options, most directly by removing the fallback of early pension access. Such elimination of outside options in the form of pensions reduces wages for affected workers on average. In this section, I analyze whether the effects on wages display heterogeneity by substitutability and job-specific skills.

There are two main opposite forces that display heterogeneity. On the one hand, the negative effect might be more pronounced for workers with high specificity (e.g., job-specific skills or high-level managerial roles), because their outside options may be especially limited. On the other hand, if such workers are more productive, firms may have incentives to offer wage premia to retain them, potentially offsetting the negative effect on their wages (see the derivations in chapter 2). Hence, the effect of the rise in ERA on wages by substitutability and the specificity of skills required to perform the given job may be both positive and negative.

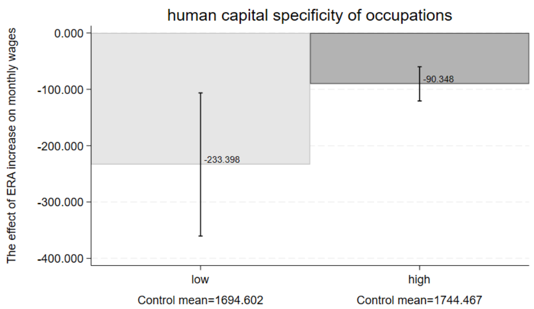
I test this implication by estimating RDD regressions with monthly wages as the outcome, focusing on subsamples that differ in job-specificity and substitutability. Figure 6 presents the results. As expected, the overall wage effect is negative, consistent with reduced outside options weakening employee bargaining power, but effects vary across groups. Among the more replaceable workers, wages decline post-reform. In contrast, managers and those in occupations that are difficult to replace externally sometimes experience wage gains after the reform, likely reflecting firms' reluctance to lose strategically important employees.

Public administration (WZ08: O), Education (WZ08: P), Health and social services (WZ08: Q), Cultural, social and personal services (WZ08: R, S), Household-related services (WZ08: T), Other economic services (WZ08: N), Extraterritorial organizations (WZ08: U). I thank Duncan Roth for the help with the data.

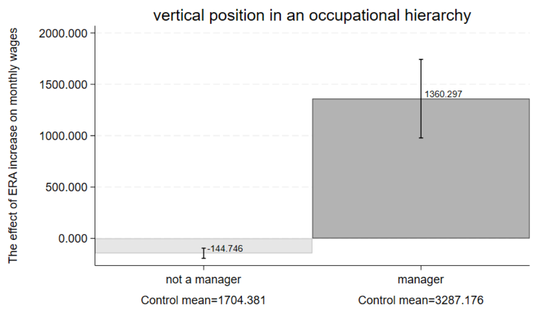
[37] In addition, the generalized categories of industries help me to test whether the external substitutability operates beyond the national level. I define industries by mapping based on the IAB establishment Panel, following the procedure described in Dauth/Eppelsheimer (2020). Figure A16 does not display significant differences by tradability.

Figure 6.: Subsample analyses for the effect of the rise in ERA on wages at ages 60-62 by substitutability measures

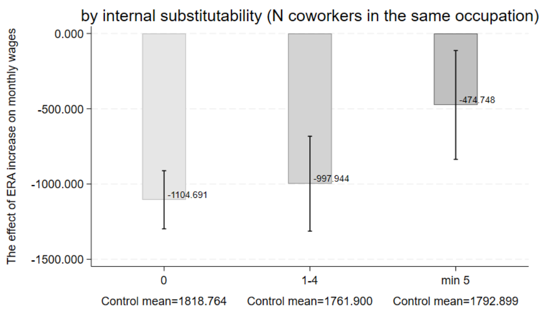
Panel A: Human capital specificity



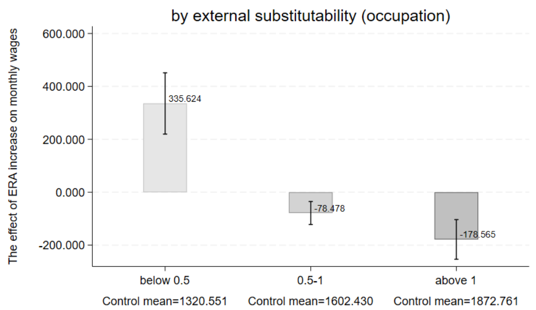
Panel B: Hierarchical positions



Panel C: Internal substitutability in the sample of small establishments



Panel D: External substitutability (occupations)



Notes: Coefficient plots from RDD regressions around the January 1952 cutoff. The estimates are obtained using local linear regressions with first-order polynomials, a triangular kernel, and mean square error–optimal bandwidth selection. Controls include calendar month of birth, Western residence, wages at age 46, and education. The vertical lines represent 95% confidence intervals based on robust standard errors clustered at the birth-month level. Control means (on the x-axis) refer to the average employment rate at ages 60–62 among the control group within each group's optimal bandwidth. The corresponding detailed tables are reported in Table B23, Table B24, and Table B25.

This result may reflect firm retention motives: when specific workers contribute more to firm profits, firms may offer wage premia despite weak outside options. However, selection into employment, whereby only the most productive or critical workers remain, may bias the upward wage effects observed in these groups. Overall, these findings highlight that the wage effects of the retirement reform are shaped by a complex interplay between retention needs and bargaining power, conditional on continued employment.

7 Conclusion

This paper highlights the often-overlooked role of worker substitutability in shaping firm responses to retirement age reforms. While raising the early retirement age extends working lives among older workers on average, this result masks substantial heterogeneity driven by differences in skill specificity to perform a given job and substitutability of a given occupation internally (coworkers in the same occupation) and externally (potential external hires in commuting zone for a given occupation or industry).

The results show that workers in occupations with high skill specificity required to perform a given job and limited substitutability are more likely to be retained post-reform. In these settings, raising the early retirement age reduces staffing frictions and extends working lives. However, these effects are not uniformly beneficial for the workers: less substitutable workers are retained more often, but may experience weaker wage growth due to reduced outside options, while more easily replaced workers face greater employment risk. These findings suggest that retirement age reforms can alleviate staffing constraints in rigid labor markets, but may also enhance firms' wage-setting power, especially when older workers have fewer fallback options. Evaluating such policies thus requires attention to both labor supply and firm-side frictions.

Future research could further investigate the exit routes taken by workers who leave employment before the statutory retirement age dependent on labor demand factors, such as transitions into self-employment, unemployment, other jobs, or informal retirement paths. For example, the employers have an incentive to encourage their substitutable workers to use up to two years of unemployment as a bridge to retirement. Due to the lack of unemployment spells in this paper's data, I am unable to test for this specific path. These mechanisms remain important for understanding the broader effects of retirement age reforms.

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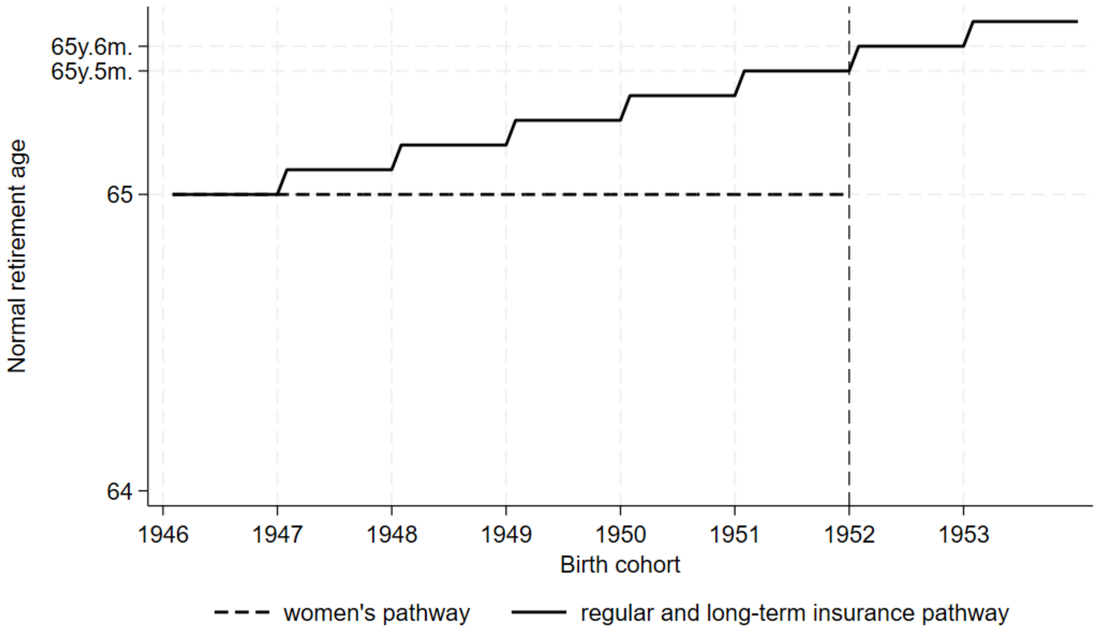
Appendix

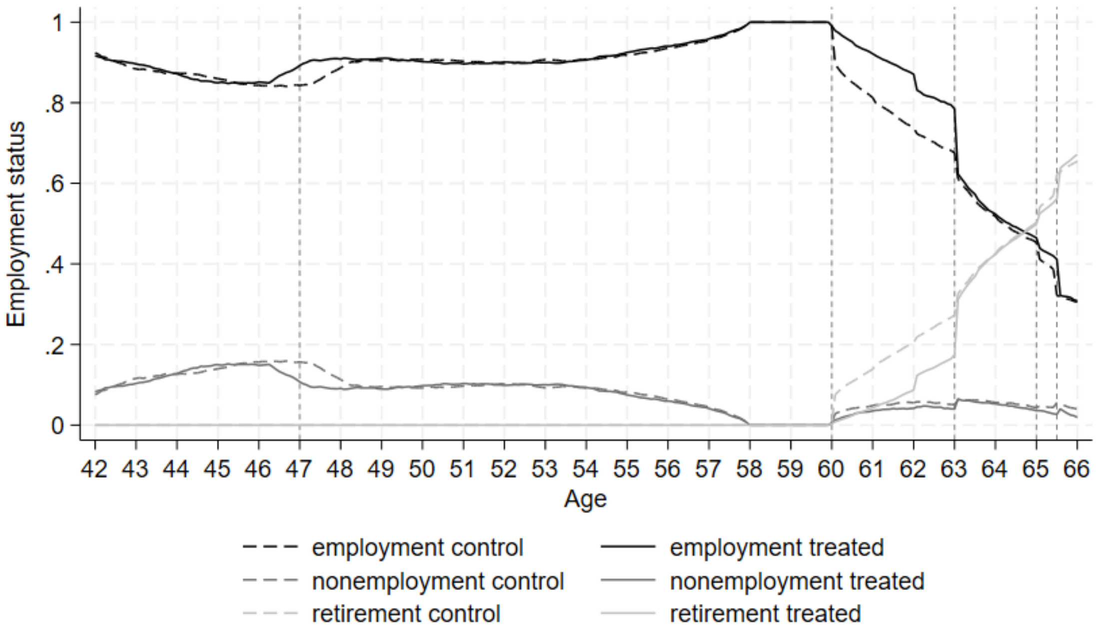
A1 Appendix figures

Figure A1.: The assignment of normal retirement age by birth cohorts



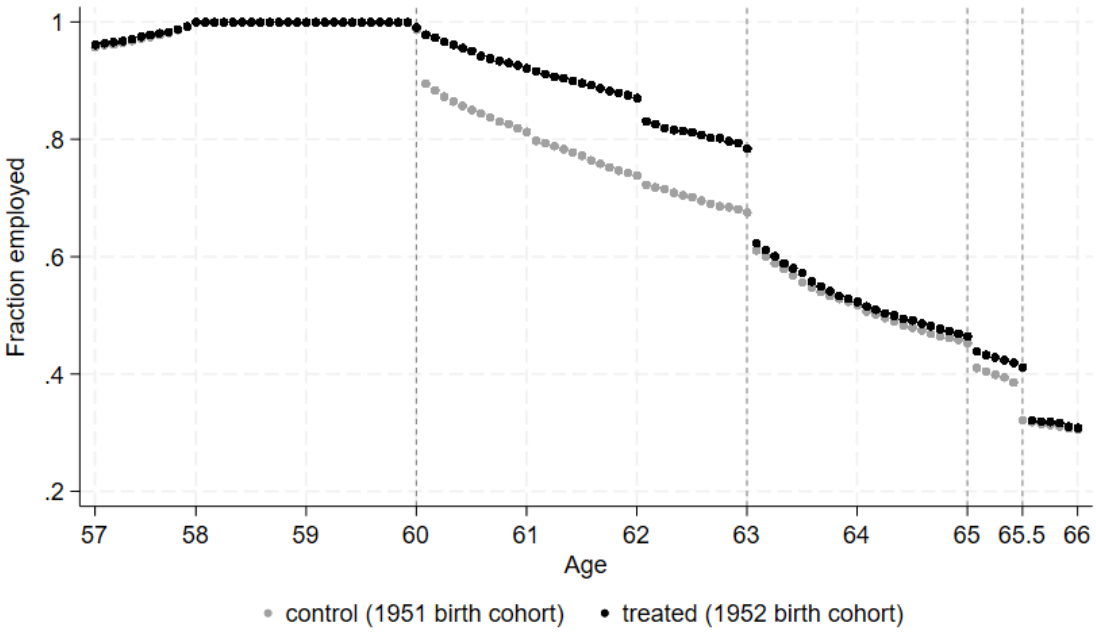
Notes: This figure depicts the assignment rule of normal retirement age by birth cohorts. Before the 1952 cohort, there was a women's pathway to retirement (dashed line). The vertical dashed line at the January 1952 cohort indicates the birth cutoff from which the women's pathway to early retirement was abolished. Starting from the 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, but increased by monthly increments per birth year starting from the 1947 cohort (black line).

Figure A2.: Fraction of women employed, nonemployed, and retired at each age-month by treatment and control group



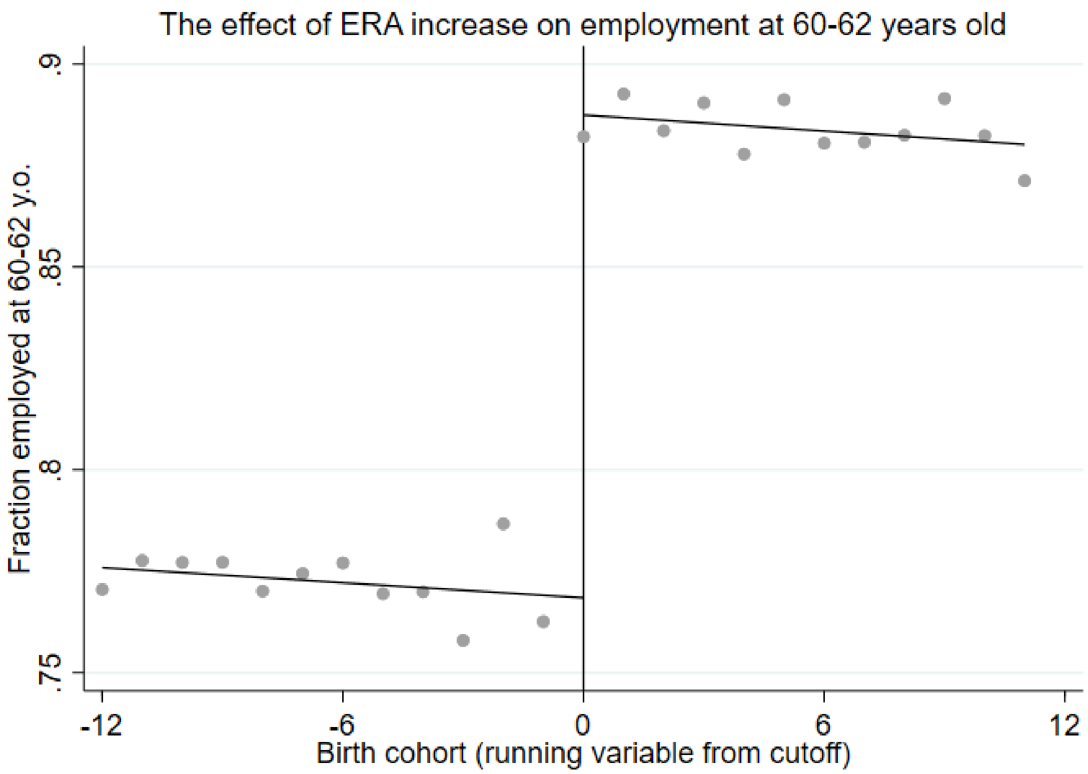
Notes: This figure displays the evolution of three main employment states (*employment* in black, *nonemployment* in dark gray, and *retirement* in light gray- see chapter 4 for more details) over age by treatment status: (i) *treated* - women born in 1952 (solid lines), and (ii) *control*- women born in 1951 (dashed lines). The first short-dashed vertical line (at age 47) corresponds to the age of the 1st treated cohort in 1999. The next two short dashed vertical lines show the age frame between the old ERA scheme (at age 60) and the new one (at least age 63) per the 1999 reform, while the last two short-dashed vertical lines show the old NRA scheme (at age 65) and the new one (at age 65 years and six months) per the 2007 reform.

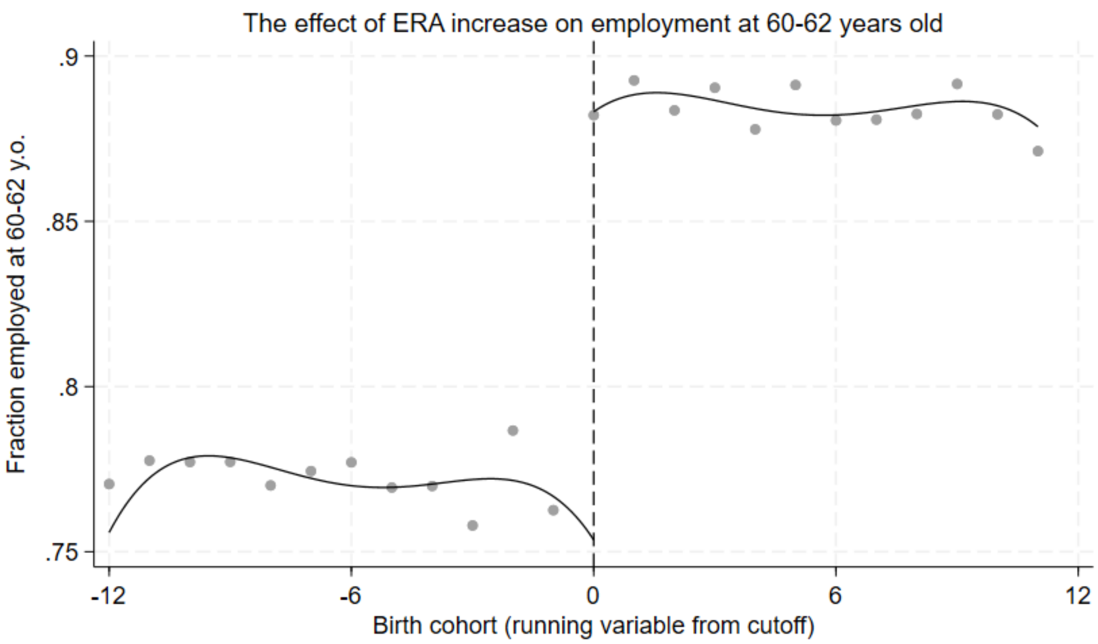
Figure A3.: Fraction of women employed at each age-month by treatment and control group



Notes: This figure displays the fraction of women employed at each age month by two treatment statuses: treated (the 1952 birth cohort, in black) and control (the 1951 birth cohort, in gray). The period between the two dashed lines at 60 and 63 years old indicates the gaps between the two groups due to the 1999 reform under study.

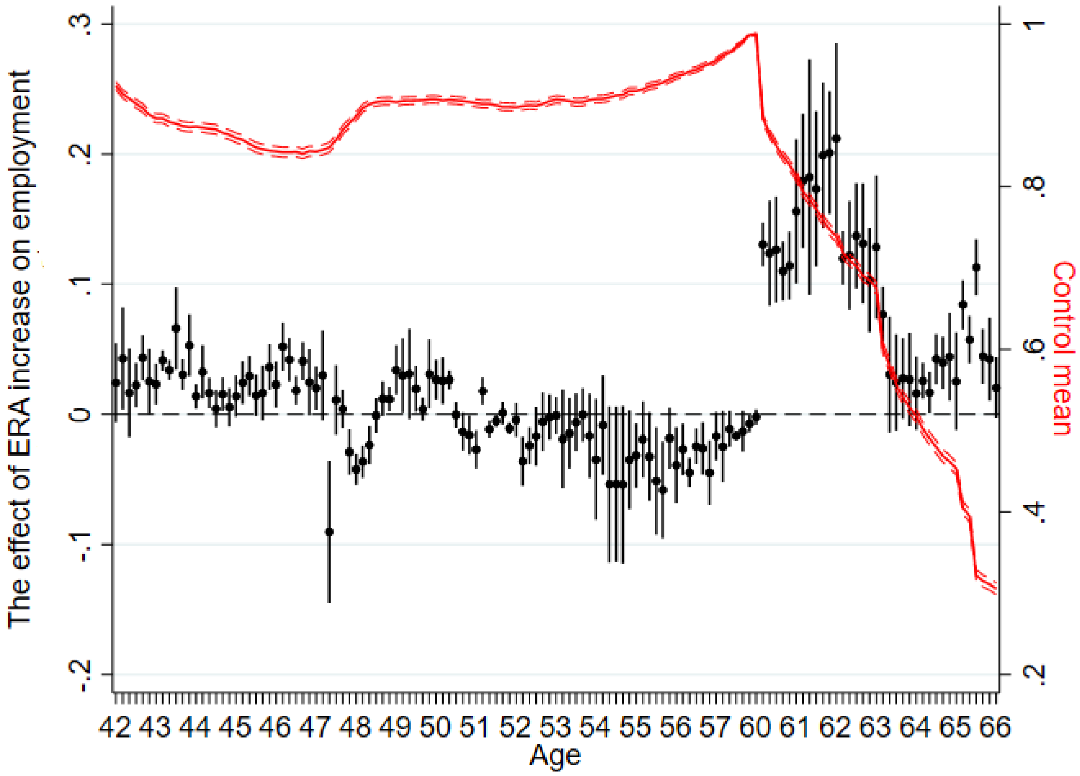
Figure A4.: The effect of the rise in ERA: RDD plot





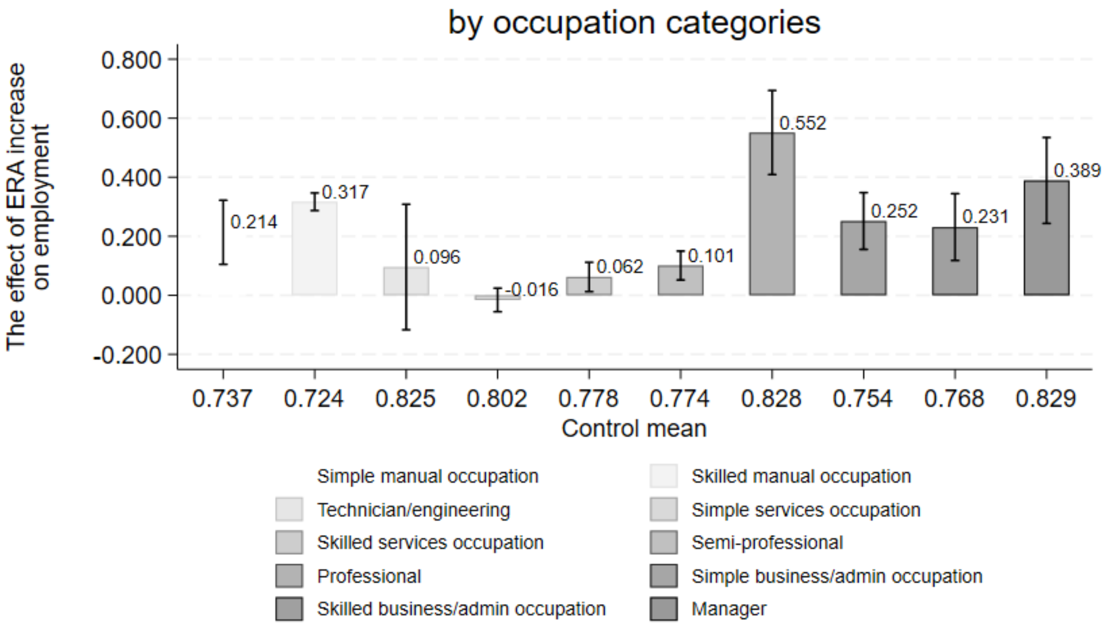
Notes: RDD regression of the share of employed at ages 60-62 around the 1952 cutoff. For computing the RDD estimates, I use first-order polynomials (upper graph) or automatic 4th order (lower graph), triangular kernel function, and mean square-based optimal bandwidth selection procedure. The vertical line marks the birth cohort threshold 1952 (e.g., 0 corresponds to January 1952, -6 corresponds to people born six months before, in June 1951).

Figure A5.: RDD by age in months



Notes: Coefficient plots. Each vertical line corresponds to the RDD regression of the share of employed at a given age-month. For computing the RDD estimates, I use local linear regressions, a triangular 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, clustered at the birth month level. The red solid line represents the control mean (with corresponding values displayed on the reversed y-axis), while the red dashed lines represent the confidence intervals for the control means.

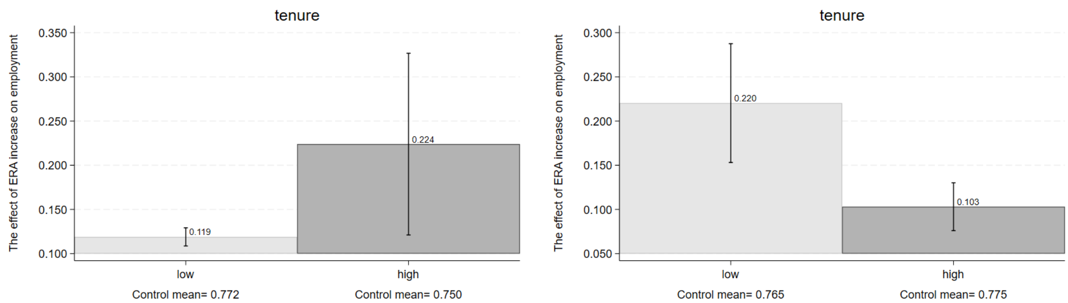
Figure A6.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by aggregate occupations



Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by ten categories of occupations based on occupational classification. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B15.

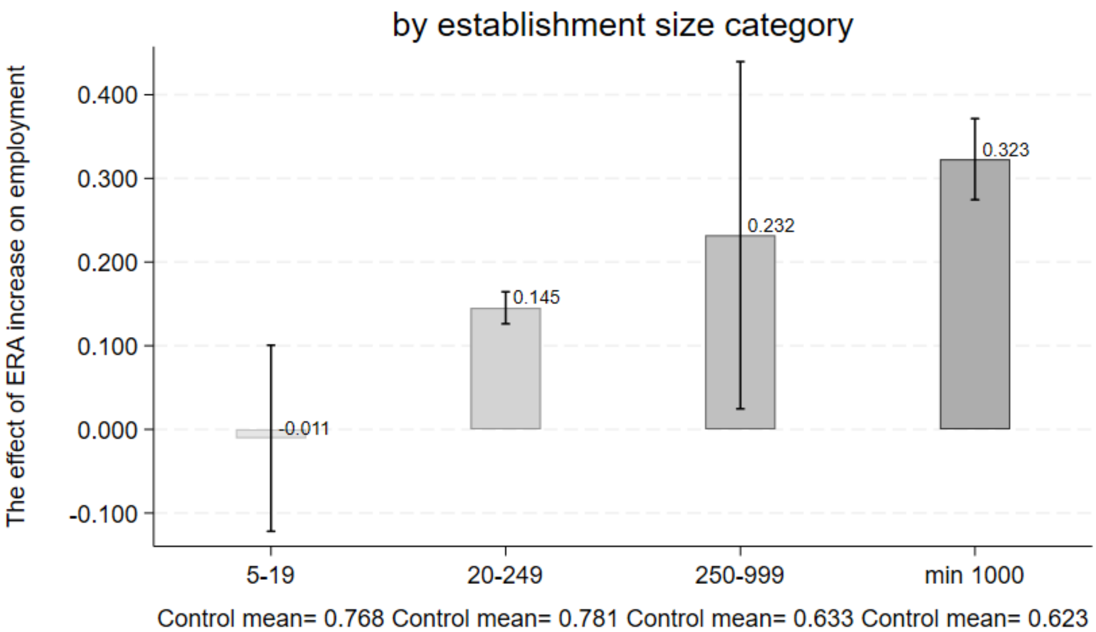
Figure A7.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by tenure

Panel A: tenure measured at 46 years old (Me=4.5 years) **Panel B: tenure measured at 58 years old (Me=7.7 years)**



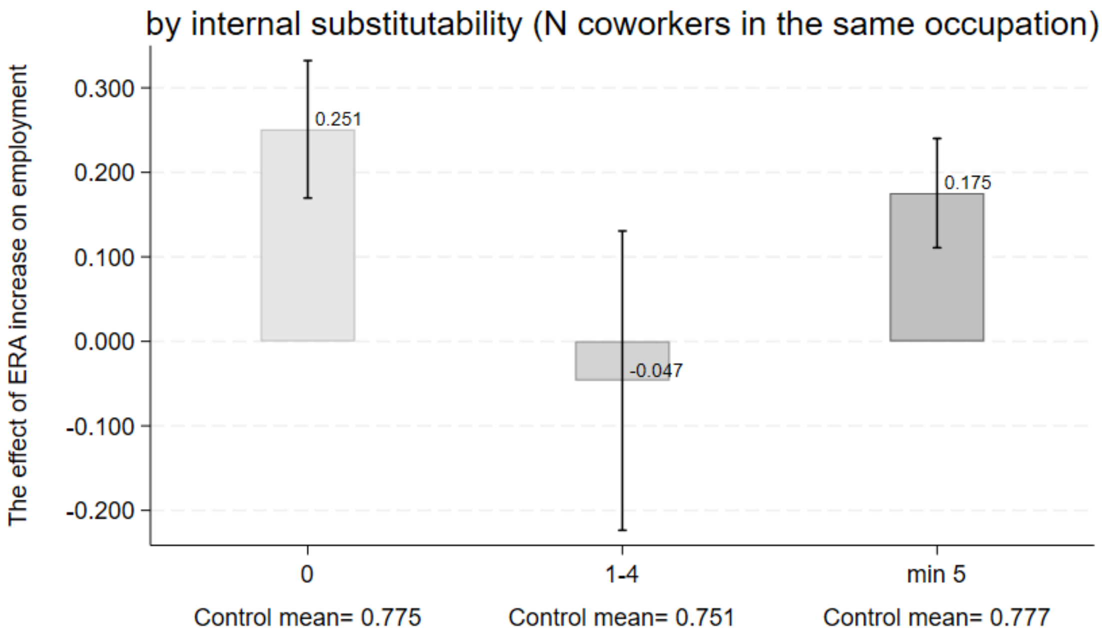
Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by median split of tenure recorded at 46 years old (**Panel A**), and 58 years old (**Panel B**)- 4.5 and 7.7 years, respectively. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B14.

Figure A8.: The effect of the rise in ERA on employment at ages 60-62 by establishment size



Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by *establishment size* categories. The three categories of establishment size are (1) up to 19, (2) 20-249, (3) 250-999, and (4) more than 1,000 workers employed at the establishment. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B16.

Figure A9.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by number of internal substitutes

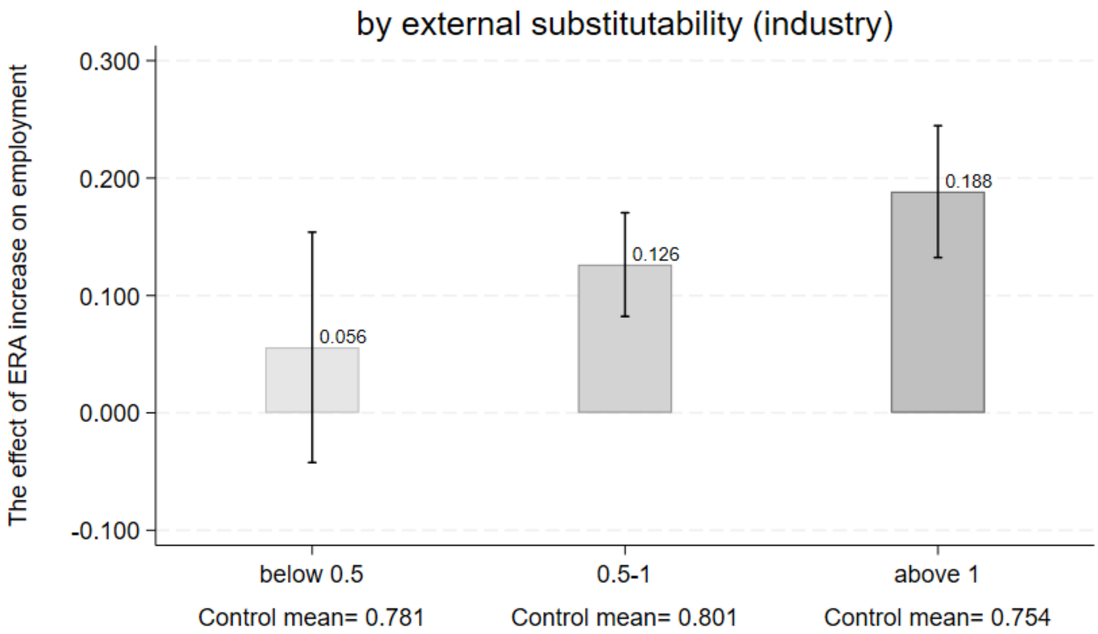


Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by the number of coworkers in the same 3-digit occupation, restricting the sample to establishments with fewer than 100 workers. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B13.

Figure A10.: The effect of the rise in ERA on employment at ages 60-62 by gender-composition of occupations and establishments

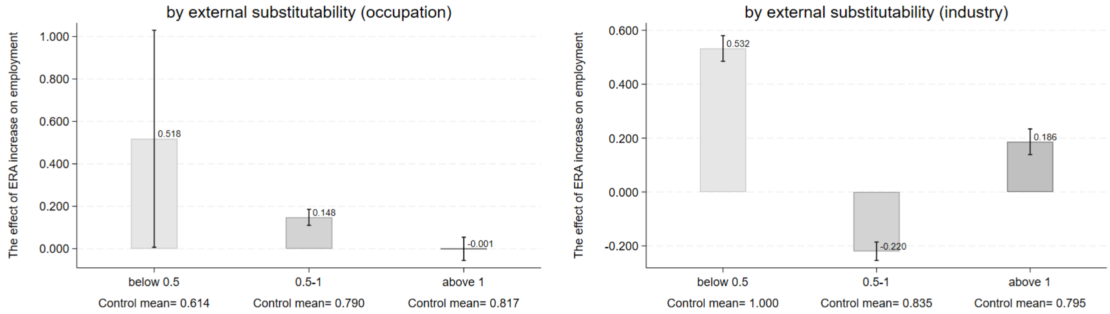
Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. The subsample analyses are performed by *gender dominance* of occupations (left Panel) and establishments (right Panel). *Gender-integrated* occupations/establishments are defined as those in which the proportion of men and women ranges from 21% to 79%. *Gender-dominated* occupations/establishments are those in which 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B21.

Figure A11.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by external substitutability of a given industry



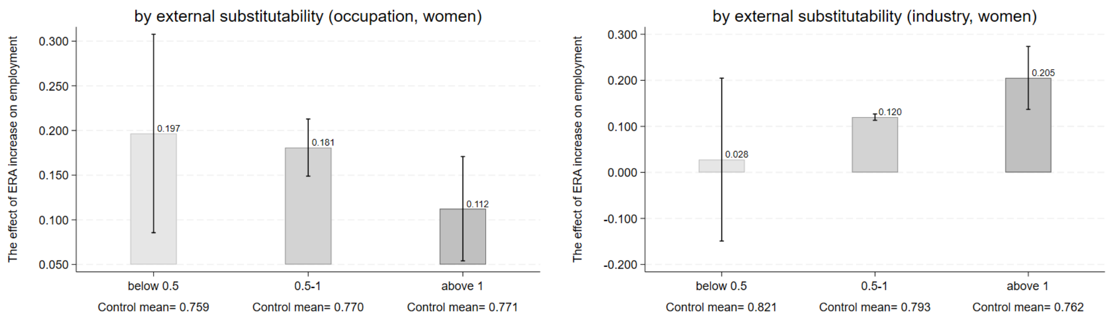
Notes: Coefficient plots from RDD regressions around the January 1952 cutoff. The estimates are obtained using local linear regressions with first-order polynomials, a triangular kernel, and mean square error–optimal bandwidth selection. Controls include calendar month of birth, Western residence, wages at age 46, and education. The *external labor market thickness (ELMT)* is categorized in three groups based on the commuting zone being at most half as concentrated in a given industry as the country-level ($ELMT < 0.5$), or at least half as concentrated but less concentrated than the country-level ($0.5 < ELMT < 1$), and at least as concentrated as the country-level concentration ($ELMT > 1$). Vertical lines represent 95% confidence intervals based on robust standard errors clustered at the birth-month level. Control means (on the x-axis) refer to the average employment rate at ages 60–62 among the control group within each group's optimal bandwidth. The corresponding detailed table is reported in Table B17.

Figure A12.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by external substitutability, restricting to small establishments with at most 100 workers



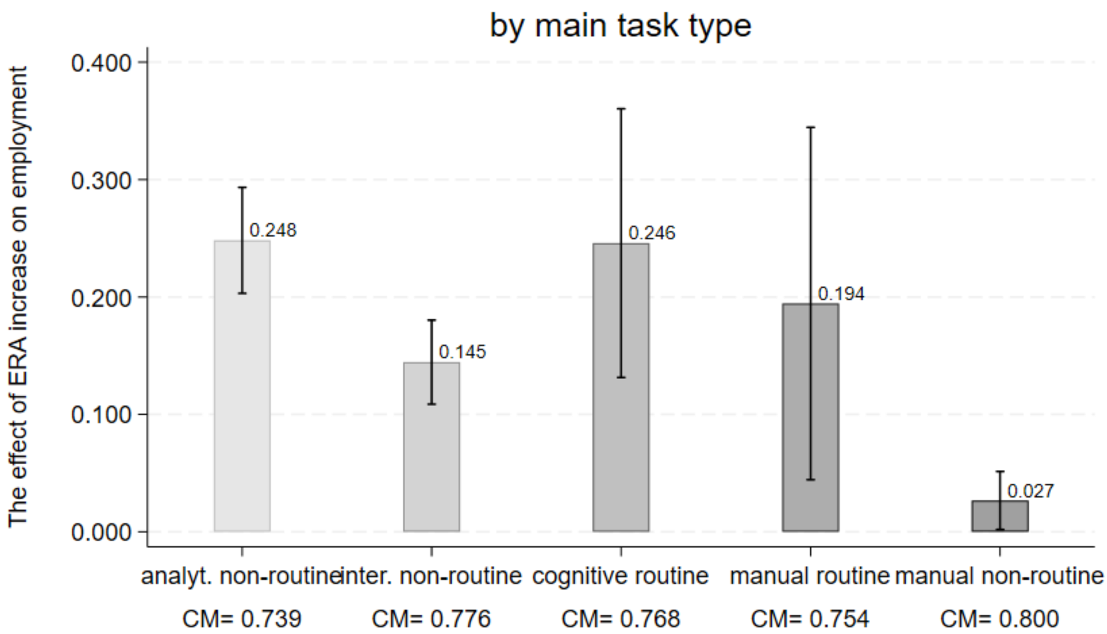
Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. Both Panels show the subsample analyses by *external labor market thickness (ELMT)*, based on the commuting zone being at most half as concentrated in a given occupation (**left Panel**) or industry (**right Panel**) as the country-level ($ELMT < 0.5$), or at least half as concentrated but less concentrated than the country-level ($0.5 < ELMT < 1$), and at least as concentrated as the country-level concentration ($ELMT > 1$). 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B17.

Figure A13.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by external substitutability, using only the female workforce for computations



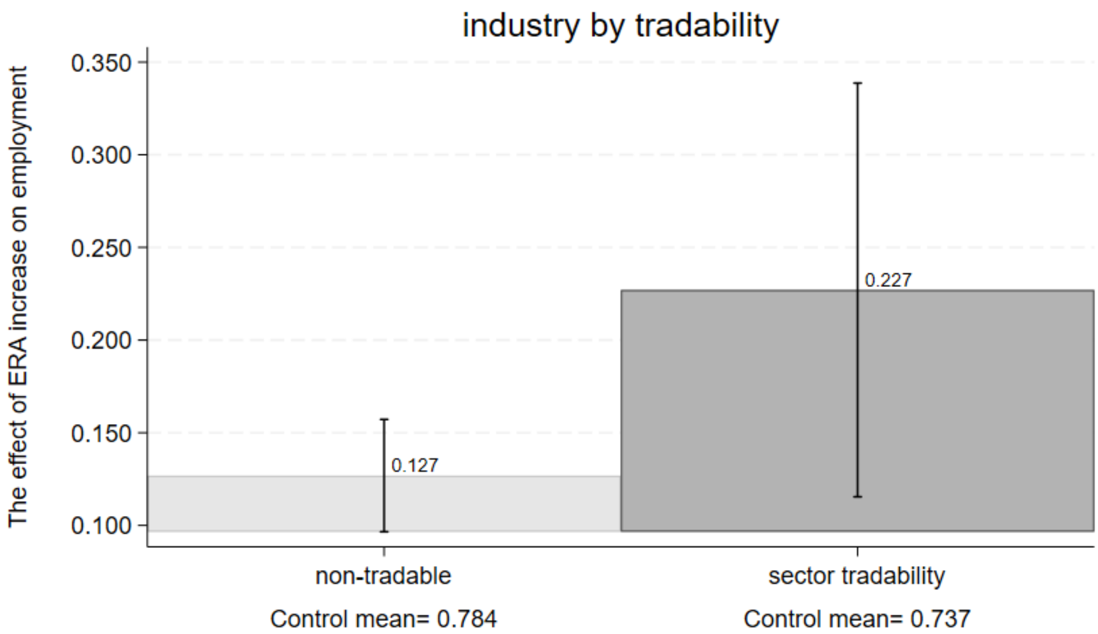
Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. Both Panels show the subsample analyses by *external labor market thickness (ELMT)*, based on the commuting zone being at most half as concentrated in a given occupation (**left Panel**) or industry (**right Panel**) relative to the country-level ($ELMT < 0.5$), or at least half as concentrated but less concentrated than the country-level ($0.5 < ELMT < 1$), and at least as concentrated as the country-level concentration ($ELMT > 1$). The difference from the baseline definitions in the paper is that I use data on women only to construct ELMT. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951).

Figure A14.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by task type



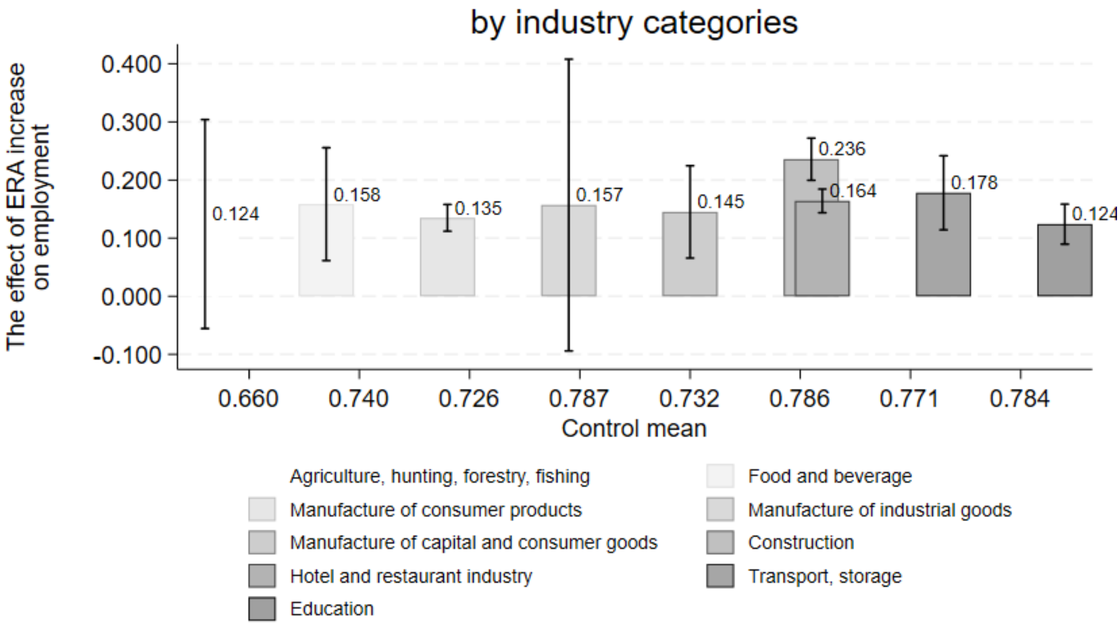
Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by five 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, abbreviated as "CM") show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B18.

Figure A15.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by tradability of industries



Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by tradability of industries. 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B19.

Figure A16.: Subsample analyses for the effect of the rise in ERA on employment at ages 60-62 by aggregate industry categories



Notes: Coefficient plots for RDD regressions around the 1952 cutoff. For computing the RDD estimates, I use local linear regressions, a triangular kernel function, and mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. I perform subsample analyses by aggregated industry 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) show the employment share at the ages of 60-62 in the corresponding subsample over the control group (born in 1951). A corresponding table with more details can be found in Table B20.

B1 Appendix tables

Table B1.: Baseline sample size after each restriction in German social security data

	N women, (birth cohort 1951)	N women, (birth cohort 1952)	N total
unrestricted	**34570**	**36776**	**71346**
delete miners	34562	36771	71333
delete sailors	34560	36768	71328
delete parallel spells	-	-	-
delete age-months below 42 years old	32236	34166	66402
delete age-months above 66	31988	33936	65924
delete repeating age-months	-	-	-
delete if not employed at 58-59	**15640**	**17130**	**32770**

Notes: This table records the sample size after each of the restrictions in German social security data. The first column names the restrictions. The second and third columns list the sample size of treated and control groups, while the last column records the total sample size, i.e., the sum of the two preceding columns.

Table B2.: Balance check. The effect of the rise in ERA on covariates

	(1) West origin	(2) non-German
The rise in ERA	-0.007	0.013***
	(0.009)	(0.005)
Bandwidth	2.8	3.4
Observations	1179720	1179720

Notes: This table shows the effect of the rise in ERA on *Western German origin* (column 1) and *non-German* nationality (column 2) (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA was raised by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday. I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. Robust standard errors in parentheses are clustered at the birth-month level.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B3.: The effect of the rise in ERA 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	(7) monthly wage
The rise in ERA	0.173^{***}	0.070^{***}	0.015	0.048^{***}	-0.021^{***}	-0.150^{***}	-116.522^{***}
	(0.027)	(0.014)	(0.016)	(0.005)	(0.006)	(0.021)	(23.368)
Bandwidth	2.9	3.9	3.9	4.5	3.0	3.0	3.4
Control mean	0.774	0.455	0.232	0.079	0.050	0.179	1719.644
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for 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 (the control group). Robust standard errors in parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure 2.

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

Table B4.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by altering the estimation procedure

	(1) employment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employment	(6) retirement	(7) monthly wage
Panel A: bias-corrected RD estimates with robust variance estimator (baseline)							
Robust	0.173***	0.070***	0.015	0.048***	-0.021***	-0.150***	-116.522***
	(0.027)	(0.014)	(0.016)	(0.005)	(0.006)	(0.021)	(23.368)
Panel B: conventional RD estimates with conventional variance estimator							
Conventional	0.166***	0.078***	0.003	0.051***	-0.020***	-0.144***	-64.181***
	(0.002)	(0.009)	(0.014)	(0.003)	(0.002)	(0.002)	(21.622)
Panel C: bias-corrected RD estimates with conventional variance estimator							
Bias-corrected	0.173***	0.070***	0.015	0.048***	-0.021***	-0.150***	-116.522***
	(0.002)	(0.009)	(0.014)	(0.003)	(0.002)	(0.002)	(21.622)
Bandwidth	2.9	3.9	3.9	4.5	3.0	3.0	3.4
Control mean	0.774	0.455	0.232	0.079	0.050	0.179	1719.644
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60^{th} birthday to their 63^{rd} birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for 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 (the control group). **Panel A** shows the bias-corrected RD estimates with robust variance estimator, **Panel B** -conventional RD estimates with conventional variance estimator, **Panel C** -bias-corrected RD estimates with conventional bias estimator. Standard errors in parentheses are clustered at the birth-month level.

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

Table B5.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by specified polynomial order

	(1) employment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employment	(6) retirement	(7) monthly wage
Panel A: polynomial function of order 1 (baseline)							
The rise in ERA	0.173***	0.070***	0.015	0.048***	-0.021***	-0.150***	-116.522***
	(0.027)	(0.014)	(0.016)	(0.005)	(0.006)	(0.021)	(23.368)
Bandwidth	2.9	3.9	3.9	4.5	3.0	3.0	3.4
Control mean	0.774	0.455	0.232	0.079	0.050	0.179	1719.644
Panel B: polynomial function of order 2							
The rise in ERA	0.254***	0.063***	0.131***	0.056***	-0.040***	-0.215***	-145.377***
	(0.047)	(0.022)	(0.023)	(0.010)	(0.014)	(0.032)	(33.774)
Bandwidth	3.3	4.6	3.2	4.6	3.4	3.3	4.9
Control mean	0.769	0.458	0.232	0.079	0.050	0.181	1724.441
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I use a first-order polynomial function in **Panel A**, and a second-order polynomial in **Panel B**. I control for calendar month, a dummy for Western residence, wages at 46, and education. The control means are the average values of the outcomes when I limit the sample to women born in 1951 (the control group). Robust standard errors in parentheses are clustered at the birth-month level.

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

Table B6.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by the specified kernel function

	(1) employ-ment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employ-ment	(6) retire-ment	(7) monthly wage
Panel A: triangular weights (baseline)							
The rise in ERA	0.173***	0.070***	0.015	0.048***	-0.021***	-0.150***	-116.522***
	(0.027)	(0.014)	(0.016)	(0.005)	(0.006)	(0.021)	(23.368)
Bandwidth	2.9	3.9	3.9	4.5	3.0	3.0	3.4
Control mean	0.774	0.455	0.232	0.079	0.050	0.179	1719.644
Panel B: Epanechnikov kernel							
The rise in ERA	0.171***	0.071***	0.008	0.048***	-0.020***	-0.148***	-99.628***
	(0.029)	(0.016)	(0.017)	(0.006)	(0.006)	(0.022)	(22.389)
Bandwidth	2.9	3.8	4.0	4.3	3.0	3.0	3.5
Control mean	0.774	0.455	0.231	0.079	0.050	0.179	1719.644
Panel C: uniform kernel							
The rise in ERA	0.168***	0.076***	0.002	0.047***	-0.023***	-0.146***	-133.632***
	(0.029)	(0.021)	(0.019)	(0.004)	(0.006)	(0.024)	(25.635)
Bandwidth	2.7	3.3	3.1	2.8	2.7	2.9	2.6
Control mean	0.774	0.455	0.232	0.080	0.047	0.179	1723.688
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function in **Panel A**, Epanechnikov kernel in **Panel B**, and uniform weights in **Panel C**. I use a mean square error-based optimal bandwidth choice. I control for 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 (the control group). Robust standard errors in parentheses are clustered at the birth-month level.

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

Table B7.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by ad-hoc bandwidth choices

	(1) employ-ment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employ-ment	(6) retire-ment	(7) monthly wage
Panel A: all the birth cohorts							
The rise in ERA	0.132^{***}	0.091^{***}	-0.010	0.051^{***}	-0.013^{***}	-0.119^{***}	-4.547
	(0.014)	(0.014)	(0.016)	(0.005)	(0.004)	(0.011)	(38.270)
Bandwidth	12.0	12.0	12.0	12.0	12.0	12.0	12.0
Control mean	0.772	0.458	0.228	0.086	0.050	0.178	1744.540
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346
Panel B: excluding December 1951 and January 1952 birth cohorts							
The rise in ERA	0.115^{***}	0.123^{***}	-0.055^{***}	0.047^{***}	-0.006	-0.109^{***}	97.799^{***}
	(0.023)	(0.018)	(0.015)	(0.007)	(0.007)	(0.017)	(23.570)
Bandwidth	12.0	12.0	12.0	12.0	12.0	12.0	12.0
Control mean	0.773	0.458	0.229	0.086	0.050	0.177	1744.764
Observations	1077408	1077408	1077408	1077408	1077408	1077408	895417
N workers	29928	29928	29928	29928	29928	29928	28662

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function and a 12-month ad-hoc bandwidth choice. **Panel A** displays the regressions with all cohorts born 1 year before or after the January 1952 cutoff, while **Panel B** removes the observations of women born 1 month around the cutoff. I control for 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 (the control group). Robust standard errors in parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure 2.

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

Table B8.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by the choice of covariates included

	(1) employment
Panel A: baseline (month dummies, education, and western German residence)	
The rise in ERA	0.173^{***}
	(0.027)
Panel B: additionally controlling for regional origin and foreigner (non-German) status	
The rise in ERA	0.173^{***}
	(0.027)
Panel C: no controls	
The rise in ERA	0.152^{***}
	(0.024)
Bandwidth	2.8
Control mean	0.772
Observations	1179720
N workers	32770

Notes: This table shows the effect of rise in the ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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 western origin and foreigner status. I have no control variables in **Panel C**. The control means are the average values of the outcomes when I limit the sample to women born in 1951. Robust standard errors in parentheses are clustered at the birth-month level.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B9.: Robustness and sensitivity checks. The effect of the rise in ERA on employment outcomes at 60-62 years old by the specified clustering method for standard errors

	(1) employ-ment	(2) employees liable to social security	(3) marginal part-time employment	(4) partial retirement	(5) non-employ-ment	(6) retire-ment	(7) monthly wage
Panel A: clustering at the birth date level (baseline)							
The rise in ERA	0.173***	0.070***	0.015	0.048***	-0.021***	-0.150***	-116.522***
	(0.027)	(0.014)	(0.016)	(0.005)	(0.006)	(0.021)	(23.368)
Bandwidth	2.9	3.9	3.9	4.5	3.0	3.0	3.4
Control mean	0.774	0.455	0.232	0.079	0.050	0.179	1719.644
Observations	1179720	1179720	1179720	1179720	1179720	1179720	1179720
N workers	32770	32770	32770	32770	32770	32770	32770
Panel B: clustering at the establishment level							
The rise in ERA	0.148***	0.070**	0.022	0.051**	-0.017	-0.131***	-136.181
	(0.027)	(0.035)	(0.022)	(0.021)	(0.011)	(0.026)	(100.397)
Bandwidth	3.3	3.2	3.3	3.6	3.3	3.4	2.9
Control mean	0.769	0.455	0.232	0.081	0.050	0.181	1723.688
Observations	1179720	1179720	1179720	1179720	1179720	1179720	980014
N workers	32770	32770	32770	32770	32770	32770	31346

Notes: These tables show the regression discontinuity design estimates around the cutoff of 1952, starting from which ERA rose by at least 3 years (Equation 9). I pool all observations from the month after a worker's 60$^{\text{th}}$ birthday to their 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). There are 3 mutually exclusive outcome variables: *employment* (column 1), *nonemployment* (column 5), and *retirement* (column 6). *Employment* can be further decomposed into columns 2-4. I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western German 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 (the control group). Robust standard errors in parentheses are clustered at the birth-month level in **Panel A** and establishment level in **Panel B**. The corresponding coefficient plot can be found in Figure 2.

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

Table B10.: Falsification test: RDD on employment at 60-62 years old around placebo cutoffs

	(1) employment
Panel A: 1948 cohort females	
Robust RDD	-0.025
	(0.101)
Bandwidth	4.0
Observations	728892
N workers	20247
Panel B: 1949 cohort females	
Robust RDD	0.004
	(0.784)
Bandwidth	3.7
Observations	853812
N workers	23717
Panel C: 1950 cohort females	
Robust RDD	-0.004
	(0.438)
Bandwidth	3.0
Observations	985104
N workers	27364
Panel D: 1951 cohort females	
Robust RDD	0.021 *
	(0.062)
Bandwidth	3.2
Observations	1083420
N workers	30095

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). **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 a worker's 60^{th} birthday to their 63^{rd} birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. Robust standard errors in parentheses are clustered at the birth month level.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B11.: Falsification test: RDD on employment at 60-62 years old around the reform cutoff for males

	(1) employment
Robust RDD	0.051***
	(0.016)
Bandwidth	3.2
Observations	1230624
N workers	34184

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9) for males. The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I control for 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 parentheses are clustered at the birth month level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B12.: The effect of the rise in ERA on employment at 60-62 years old by measures of worker skills

	employment	
	(1)	(2)
Panel A: human capital specificity of occupations		
	low	high
The rise in ERA	0.113***	0.230***
	(0.006)	(0.054)
Bandwidth	4.8	2.6
Control mean	0.778	0.771
Observations	547164	632340
N workers	15199	17565
Panel B: by hierarchical vertical position		
	not a manager	manager
The rise in ERA	0.171***	0.431***
	(0.027)	(0.103)
Bandwidth	2.9	3.1
Control mean	0.774	0.779
Observations	1165896	13824
N workers	32386	384

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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 returns to 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 occupations. **Panel B** shows managerial status, which is created as a dummy from the last 2 digits of the 5-digit occupational variables. I control for 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure 3.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B13.: The effect of the rise in ERA on employment at 60-62 years old by internal substitutability (number of coworkers in the same occupation)

		employment	
	(1)	(2)	(3)
	0	1-4	at least 5
Panel A: all the establishment categories			
The rise in ERA	0.251***	-0.047	0.175***
	(0.041)	(0.090)	(0.033)
Bandwidth	4.0	2.3	2.7
Control mean	0.775	0.751	0.777
Observations	53784	39888	1055808
N workers	1494	1108	29328
Panel B: establishments with fewer than 100 workers			
The rise in ERA	0.268***	-0.156*	0.065***
	(0.051)	(0.085)	(0.008)
Bandwidth	3.3	2.7	4.0
Control mean	0.764	0.793	0.822
Observations	22896	24156	56412
N workers	636	671	1567

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by 3 categories of internal substitutes: 0, 1-4, and at least 5 workers. **Panel A** displays the results for all the sizes of establishments, while **Panel B** zooms in on smaller establishments with fewer than 100 workers. I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Figure 4.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B14.: The effect of the rise in ERA on employment outcomes at 60-62 years old by tenure

	employment	
	(1)	(2)
	low tenure	high tenure
Panel A: at 46 years old (Me=4.5 years)		
The rise in ERA	0.220***	0.103***
	(0.034)	(0.014)
Bandwidth	2.9	3.4
Control mean	0.765	0.775
Observations	600444	579276
N workers	16679	16091
Panel B: at 58 years old (Me=7.7 years)		
The rise in ERA	0.119***	0.224***
	(0.005)	(0.052)
Bandwidth	4.2	2.8
Control mean	0.776	0.745
Observations	511092	503352
N workers	14197	13982

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by tenure, which is created as a dummy based on a median split across all workers - 4.5 and 7.7 years for the measure created at 46 and 58 years old, respectively. I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Figure A7 in the Appendix.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B15.: The effect of the rise in ERA on employment at 60-62 years old by occupation at 58 y.o.

					employment					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Simple manual	Skilled manual	Technician/ engineering	Simple services	Skilled services	Semi-profes-sional	Profes-sional	Simple business /adminis-tration	Skilled business /adminis-tration	Manager
The rise in ERA	0.214***	0.317***	0.096	-0.016	0.062**	0.101***	0.552***	0.252***	0.231***	0.389***
	(0.055)	(0.015)	(0.108)	(0.020)	(0.025)	(0.025)	(0.073)	(0.049)	(0.058)	(0.074)
Bandwidth	4.2	2.8	2.8	3.2	3.3	4.0	3.2	3.0	2.7	3.0
Control mean	0.737	0.724	0.825	0.802	0.778	0.774	0.828	0.754	0.768	0.829
Observations	87228	45576	23796	262908	82476	145692	20592	201744	288792	20520
N workers	2423	1266	661	7303	2291	4047	572	5604	8022	570

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9) by Blossfield categories. The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60$^{\text{th}}$ birthday to their 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by 10 categories of occupations based on Blossfeld's occupational classification. I control for 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 parentheses are clustered at the birth month level. he corresponding coefficient plot can be found in Figure A6.

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

Table B16.: The effect of the rise in ERA on employment at 60-62 years old by establishment size category

		employment		
	(1)	(2)	(3)	(4)
	small	medium	large	mega large
	$N \in [5; 19]$	$N \in [20; 249]$	$N \in [250; 999]$	$N \in [999, -]$
The rise in ERA	-0.011	0.145***	0.232**	0.323***
	(0.057)	(0.010)	(0.106)	(0.025)
Bandwidth	2.8	4.1	2.9	5.7
Control mean	0.768	0.789	0.633	0.623
Observations	48204	108936	36360	28080
N workers	1339	3026	1010	780

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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), large (250-999 workers), and mega large (above 1,000 workers). I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Figure A8 in the Appendix. * $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B17.: The effect of the rise in ERA on employment at 60-62 years old by external substitutability measures

	employment		
	(1)	(2)	(3)
	below 0.5	0.5-1	above 1
Panel A: external labor market thickness (industry)			
The rise in ERA	0.056	0.126^{***}	0.188^{***}
	(0.050)	(0.023)	(0.029)
Bandwidth	3.1	2.9	3.0
Control mean	0.781	0.801	0.754
Observations	64836	419904	687348
N workers	1801	11664	19093
Panel B: external labor market thickness (occupation)			
The rise in ERA	0.413^{***}	0.156^{***}	0.117^{***}
	(0.056)	(0.022)	(0.028)
Bandwidth	3.1	2.8	3.8
Control mean	0.707	0.783	0.767
Observations	47808	513396	610632
N workers	1328	14261	16962
Panel C: external labor market thickness (industry) for small firms			
The rise in ERA	0.532^{***}	-0.220^{***}	0.186^{***}
	(0.024)	(0.017)	(0.024)
Bandwidth	2.8	2.9	4.3
Control mean	1.000	0.835	0.795
Observations	3132	33444	73476
N workers	87	929	2041
Panel D: external labor market thickness (occupation) for small firms			
The rise in ERA	0.518^{**}	0.148^{***}	-0.001
	(0.261)	(0.019)	(0.028)
Bandwidth	3.7	3.7	3.4
Control mean	0.614	0.790	0.817
Observations	2232	50688	57132
N workers	62	1408	1587

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. **Panel A** shows subsample analyses by external labor market thickness (ELMT) for a given *occupation*, based on the index taking values below 0.5, 0.5-1, and above 1. **Panel B** shows subsample analyses by ELMT for a given *industry*. **Panel C** and **Panel D** display the same regressions for the establishments with fewer than 100 workers. I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Figure 4, Figure A11 and Figure A12.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B18.: The effect of the rise in ERA on employment by task type

			employment		
	(1) analytic non-routine	(2) interactive non-routine	(3) cognitive routine	(4) manual routine	(5) manual non-routine
The rise in ERA	0.248***	0.145***	0.246***	0.194**	0.027**
	(0.023)	(0.018)	(0.058)	(0.077)	(0.013)
Bandwidth	4.2	3.1	2.9	2.9	3.7
Control mean	0.739	0.776	0.768	0.754	0.800
Observations	91152	218952	417384	88416	320724
N workers	2532	6082	11594	2456	8909

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses in five task-type categories. I control for 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure A14.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B19.: The effect of the rise in ERA on employment at 60-62 years old by industry by tradability

	employment	
	(1)	(2)
	non-tradable	tradable
The rise in ERA	0.127***	0.227***
	(0.015)	(0.057)
Bandwidth	3.1	3.0
Control mean	0.784	0.737
Observations	838044	334044
N workers	23279	9279

Standard errors in parentheses
$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by tradability of sectors. I control for 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure A15.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B20.: The effect of the rise in ERA on employment at 60-62 years old by industry categories

	employment								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Agriculture, hunting and forestry, fishing	Food and beverage	Manu- facture of consumer products	Manu- facture of industrial goods	Manufacture of capital and consu mer goods	Cons- truc- tion	Hotel and res- taurant	Trans- port, storage	Edu- cation
The rise in ERA	0.124	0.158***	0.135***	0.157	0.145***	0.236***	0.164***	0.178***	0.124***
	(0.092)	(0.050)	(0.012)	(0.128)	(0.041)	(0.018)	(0.010)	(0.033)	(0.018)
Bandwidth	3.2	4.3	3.2	2.7	4.8	3.5	3.5	3.4	2.9
Control mean	0.660	0.740	0.726	0.787	0.732	0.786	0.786	0.771	0.784
Observations	17136	34668	30960	41400	44748	21960	279036	252252	424080
N workers	476	963	860	1150	1243	610	7751	7007	11780

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by industry categories. I control for 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure A16

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

Table B21.: The effect of the rise in ERA on employment at 60-62 years old by gender domination

		employment	
Panel A: gender domination in occupation			
	gender-integrated	female-dominated	male-dominated
The rise in ERA	0.122***	0.288***	0.245***
	(0.018)	(0.027)	(0.029)
Bandwidth	2.8	3.7	4.5
Control mean	0.736	0.778	0.724
Observations	174600	76752	20376
N workers	4850	2132	566
Panel B: gender domination in establishment			
	gender-integrated	female-dominated	male-dominated
The rise in ERA	0.188***	0.207***	0.178**
	(0.015)	(0.025)	(0.072)
Bandwidth	4.4	4.0	4.1
Control mean	0.741	0.782	0.681
Observations	144000	95184	19656
N workers	4000	2644	546

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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 in which the proportion of men and women ranges from 21% to 79%. *Gender-dominated* occupations/establishments are those in which the share of one of the genders exceeds 80%. I control for 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Figure A10 in the Appendix.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B22.: The effect of the rise in ERA on employment at 60-62 years old by demographic characteristics of employees

	employment		
Panel A: residence			
	East	West	
The rise in ERA	0.164***	0.174***	
	(0.039)	(0.024)	
Bandwidth	2.8	3.0	
Observations	228168	949392	
N workers	6338	26372	
Panel B: residence of origin			
	East	West	
The rise in ERA	0.172***	0.171***	
	(0.038)	(0.025)	
Bandwidth	2.9	2.9	
Observations	232776	945756	
N workers	6466	26271	
Panel C: education			
	high school	vocational	university
The rise in ERA	0.165***	0.183***	0.094***
	(0.029)	(0.039)	(0.024)
Bandwidth	3.4	2.8	3.7
Observations	160740	897840	155340
N workers	4545	24940	4315

Notes: This table shows the effect of the rise in ERA on *employment* (RDD regression in Equation 9). The cutoff is January 1952, starting from which the ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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** divides the workers by Eastern and Western German origin, proxied by the place of residence of the first worker as observed in the employment biography; and **Panel C** divides the sample by educational categories. I control for calendar month, a dummy for Western residence, wages at the age of 46, and education. Robust standard errors in parentheses are clustered at the birth month level.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B23.: The effect of the rise in ERA on monthly wages at 60-62 years old by measures of worker skills

	monthly wages	
	(1)	(2)
Panel A: human capital specificity of occupations		
	low	high
The rise in ERA	-233.398***	-90.348***
	(64.777)	(15.485)
Bandwidth	2.7	3.9
Control mean	1694.602	1744.467
Observations	458872	520927
N workers	14619	16721
Panel B: by hierarchical position		
	not a manager	manager
The rise in ERA	-144.746***	1360.297***
	(25.361)	(195.102)
Bandwidth	3.4	3.4
Control mean	1704.381	3287.176
Observations	968243	11771
N workers	30976	370

Notes: This table shows the effect of the rise in ERA on *monthly wages* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular 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 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 parentheses are clustered at the birth-month level. The corresponding coefficient plot can be found in Panel A and Panel B of Figure 6.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B24.: The effect of the rise in ERA on monthly wages at 60-62 years old by internal substitutability (number of coworkers in the same occupation)

		monthly wages	
	(1)	(2)	(3)
	0	1-4	at least 5
Panel A: all the establishment categories			
The rise in ERA	-217.352	-960.250***	-33.783***
	(327.504)	(156.779)	(6.537)
Bandwidth	3.5	3.4	3.6
Control mean	1566.372	1349.531	1754.040
Observations	44454	33085	877485
N workers	1427	1054	28054
Panel B: establishments with fewer than 100 workers			
The rise in ERA	-1104.690***	-997.944***	-474.748**
	(98.434)	(160.777)	(184.439)
Bandwidth	3.7	3.3	2.8
Control mean	1818.764	1761.9	1792.3
Observations	18601	20033	47272
N workers	610	641	1503

Notes: This table shows the effect of the rise in ERA on *monthly wages* (RDD regression in Equation 9). The cutoff is January 1952, starting from which ERA rose by at least 3 years. I pool all observations from the month after a worker's 60th birthday to their 63rd birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. I perform subsample analyses by 3 categories of internal substitutes: 0, 1-4, and at least 5 workers. The **Panel A** displays the results for all the sizes of establishments, while **Panel B** zooms in on the smaller establishments with fewer than 100 workers. I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Panel C and Panel D of Figure 6.
* $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$.

Table B25.: The effect of the rise in ERA on monthly wages at 60-62 years old by external substitutability measures

		monthly wages	
	(1)	(2)	(3)
	below 0.5	0.5-1	above 1
Panel A: external labor market thickness (industry)			
The rise in ERA	-155.193	-81.246***	-90.073***
	(214.400)	(12.265)	(21.603)
Bandwidth	4.1	4.0	4.1
Control mean	1403.030	1518.215	1903.207
Observations	54534	352647	567063
N workers	1738	11245	18164
Panel B: external labor market thickness (occupation)			
The rise in ERA	335.624***	-78.478***	-178.565***
	(59.037)	(22.142)	(38.253)
Bandwidth	4.1	4.0	3.3
Control mean	1320.551	1602.430	1872.761
Observations	38515	430331	505147
N workers	1264	13728	16148

Notes: This table shows the effect of the rise in ERA on *monthly wages* (RDD regression in Equation 9). The cutoff is January 1952, starting from which the ERA rose by at least 3 years. I pool all observations from the month after a worker's 60$^{\text{th}}$ birthday to their 63$^{\text{rd}}$ birthday (age months corresponding to ages 60–62). I use a triangular kernel function and a mean square error-based optimal bandwidth choice. **Panel A** shows subsample analyses by external labor market thickness (ELMT) for a given *occupation*, based on the index taking values below 0.5, 0.5-1, and above 1. **Panel B** shows subsample analyses by ELMT for a given *industry*. I control for 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 parentheses are clustered at the birth month level. The corresponding coefficient plot can be found in Panel E and Panel F of Figure 6.

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

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