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Crowded Career Ladders? Intra-Firm Spillovers of Raised Retirement Age

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Crowded Career Ladders? Intra-Firm Spillovers of Raised Retirement Age*

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Abstract

I study how delayed retirements reshape firms' internal labor markets, leveraging a German reform that raised women's early retirement age by at least three years. The reform increased retention of older women and reduced both internal promotions and external hiring of younger coworkers. Spillovers are structured: promotion crowd-outs arise in thick internal labor markets with intense competition, while hiring declines are largest in thin external markets with high turnover costs. Crowd-out effects concentrate within jobcells, whereas coworkers in different jobcells can benefit when retained older workers possess specific human capital. The evidence supports slot-constraint theories—augmented by firm-specific human-capital mechanisms.

Keywords: aging, internal labor markets, human capital, worker substitutability
JEL Codes: H55, J21, J23, J24, J26, J31, J63, M51

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1 Introduction

Population aging has prompted many OECD countries to raise the official retirement ages, often through transitions to gender-neutral rules that sharply increase women's statutory retirement ages. These reforms change not only the age at which individuals exit work, but also offer rare opportunities to observe how internal and external labor markets operate, and how substitution and complementarity between older and younger workers unfold. While existing studies imply that older workers may crowd out the young, they provide limited insight into the structure of these spillovers. In this paper, I leverage a German reform that increased women's early retirement age by at least three years and a well-defined firm-level identification strategy to study how retaining older women affects both internal promotions and external hiring. By examining spillovers within occupational career ladders and across occupations, I document who is most exposed to crowding-out and who may benefit from worker retention, opening the "black box" behind these mechanisms and challenging the prevailing view that retention of older workers is uniformly detrimental to the careers of their younger coworkers.

My analysis starts by estimating retention and overall spillover effects of the reform based on the quasi-random age structure of job cells within firms near the retirement reform age cutoff. A unique reform setting with a large rise in retirement age allows me to quantify its impacts on firms and workers. I study the 1999 reform that abolished the female pathway to early retirement in Germany, raising the early retirement age (hereafter ERA- the minimum age that a worker can start claiming a pension) by at least three years, starting from the cohort of women born in 1952. This marks the largest rise in retirement age for consecutive cohorts in Germany. Studying the direct impact of this reform, Geyer and Welteke (2021) find that abolishing the female pathway to early retirement in Germany led to a 13.5 percentage point increase in employment of directly affected women at age 60-62 (an approximately 30% increase relative to the pre-reform cohort mean). Badalyan (2025) finds slightly larger effects of 17.3 p.p., conditional on employment immediately before reaching the age of 60, that is, for women who were more attached to the labor market. Although the reform was announced when the first affected cohort of women was only 47, Geyer and Welteke (2021) do not find differential labor market behavior of older women around the cutoff until they reached 60. The pre-announcement provides the opportunity to examine not only the "*main reform*" effects, that is, in 2012-2017 as the reform was enacted, but also the "*upstream*" (that is, before affected workers reach 60 years of age) adjustments in 1998-2011, following the terminology of labor supply literature (Rabaté, Jongen, and Atav, 2024).

Causally identifying the effects of an aging workforce on firm-level demand for incumbent

workers and external hires is challenging because older workers do not randomly choose to be employed at older ages, and firms that employ older workers may differ significantly from those that do not; hence, the amount of time an older worker spends working at a firm at older age may not correspond to random exposure at the firm level. To identify spillover effects, I leverage the fact that the rise in retirement age affected only cohorts born in and after 1952. For firms employing the same number of women who were born around the reform cutoff, there is a random variation in the number of women born just before or after the cutoff. This gives rise to quasi-random treatment intensities in older worker retention, thanks to the reform. I focus on firms that employed at least one woman born near the reform cutoff, that is, in 1950-1953 (I call these *focal* workers). I compare firms with similar observable characteristics (including the total number of focal workers and total workforce) but that plausibly differ exogenously in terms of the numbers of their workers who were born just after the reform cutoff (1952–1953 cohorts, i.e., *treated focal workers*). I utilize a generalized difference-in-difference approach in the retirement setting (Hut, 2024) to analyze the effect of an *additional* treated focal worker, who was subject to the rise in the ERA, on the demand for workers within the workplace and from external sources.

Germany offers large, high-quality social security data on establishments—single locations of multisite firms.[1] The data allow me to observe the universe of workers of affected establishments (that is, those with at least one focal worker) and the full employment histories of all workers employed at these establishments. I use two samples because I am interested in upstream adjustment strategies and in the main reform effects during the years when focal workers reached the ages of 60-65. The data for upstream effects comprise private-sector firms observed in 1998 (the pre-reform year). The data include approximately 140,000 establishments, about 250,000 focal workers, and roughly ten million coworkers employed in those establishments. The data for the main reform effects are sampled similarly, except that they are based on firms that employed at least one focal worker in 2008, two years before all the focal worker cohorts were younger than 60. It consists of approximately 160,000 establishments of all sizes, and over 400,000 focal workers and eight million workforce employed in 2008.

I establish the following set of results. First, I start with the upstream period and study the direct effects of this reform on retention at older ages of focal workers. An additional treated focal worker employed in 1998 leads to increased focal worker retention, but only after workers turn 55 years old. Second, I test whether such increased retentions and competition for internal promotions lead to downward pressure on the promotions of younger workers in the upstream period, thus hindering the career progression of

[1] Throughout this paper, I use the terms establishments and firms interchangeably.

coworkers and influencing external hiring practices. I do not find significant spillover effects on coworker promotions or on external hiring in the upstream period.

Because firms show virtually no adjustment in the upstream period, I re-sample the analysis in 2008—before any treated workers reached pensionable ages—which provides a fresh firm cohort and improves precision. Turning to the main reform period, I find that having an additional treated worker exposed to the rise in the ERA in 2008 leads to approximately 0.163 more focal worker retentions, generating 0.075 fewer coworker promotions and 0.103 fewer external hires in 2012–2017. Scaled by the increase in focal worker retention, these estimates imply that each additional older worker retained reduces external hiring by about 0.63 workers and about 0.46 fewer coworker promotions. The magnitude of these spillovers aligns with evidence from shocks that remove rather than retain workers.[2]

Next, I examine which demographic segments (by gender and age) are most affected by the reform. Lazear and Oyer (2004) find that internal hiring (promotions) constitutes 44-88% of job postings, with the highest percentage occurring among the highest level of occupational hierarchy. If older workers occupy high-ranked jobs, their retention could slow the career progression of middle-aged workers who are closer to them on the career ladder. The results reveal that middle-aged workers—especially women—experience the strongest crowd-out in promotions, consistent with their being the closest substitutes for older women who remain employed.[3] The crowd-outs on external hiring do not display such gendered patterns.

Understanding how delayed retirements affect firms requires distinguishing between two competing theories of internal labor markets. One view, rooted in incentive-contract models (Lazear, 1979), holds that long-tenure workers are costly to retain because their wages exceed their marginal product of labor; when retirement is postponed, firms face higher labor costs. These effects arise even in the absence of deferred compensation, because many firms operate under *slot constraints*—a fixed number of positions within each occupational ladder—so that when an older worker stays longer, the slots available for promotions or external hires shrink mechanically, reducing opportunities for internal promotions and external hiring (Boeri and van Ours, 2021). This mechanism predicts negative spillovers on younger workers, especially in hierarchical labor markets such as Germany's, where a large share of lifetime wage growth reflects career ladders (Bayer and

[2]Employers replace workers who die suddenly by increasing hiring by about 0.4 workers (Jäger and Heining, 2022), and hire 0.35 additional workers when women go on maternity leave in Denmark (Brenøe et al., 2024), and about 0.3 in Germany (Huebener et al., 2024).

[3]This pattern is consistent with Carta et al. (2024) in parental-leave settings, who show that firms tend to replace women on leave with female workers, and with Ginja, Karimi, and Xiao (2023), who document gender-specific spillovers following paternity leave.

Kuhn, 2018).

A second view emphasizes the productivity advantages of older long-tenure workers arising from accumulated firm- and job-specific human capital (Bartel et al., 2014; Friedrich and Hackmann, 2021; Jäger and Heining, 2022; Jaravel, Petkova, and Bell, 2018). When skills are highly specific, internal or external hires are imperfect substitutes (Chan, 1996; Herrmann and Rockoff, 2012; Waldman, 2003), making retention of older workers valuable to employers and potentially beneficial for coworkers through complementarities. Replacement hiring is costly—around two months' wages for high-skilled workers (Muehlemann and Pfeifer, 2016), even understating true turnover costs (Bertheau et al., 2022)—therefore, firms often move workers internally rather than recruiting externally (Becker, 1962; Bertheau, 2021).

Recent evidence supports the relevance of these frictions. Badalyan (2025) shows that the 1999 reform disproportionately increased retention among older workers who are costly to replace—managers, workers in highly specific occupations, and those in firms with few internal or external substitutes—underscoring that turnover frictions and firm-specific skills shape which older workers remain employed. In this paper, I show that these same forces also determine how delayed retirements spill over onto promotions of coworkers and onto external hiring, helping to differentiate between the incentive-contract view and the human-capital/complementarity view.

To shed light on these theories, I estimate the structure of worker substitution across occupations and age groups. I define "true" substitution elasticity as the elasticity that applies in an ideal case when a suitable younger worker is easily available (at low search costs) on the internal or external labor market. To uncover this "true" elasticity, I decompose the estimated crowd-out (spillover) effects of older worker retention into the element corresponding to labor market frictions (availability of suitable replacements) and the remaining part, which corresponds to the "true" elasticity of substitution between older focal workers and external hires. I do this separately for promotions and for external hiring, because I have ideal proxies for labor market thickness across both dimensions of worker replacement. My elasticities differ from those in the retirement literature because I can control the frictions firms face in external labor markets and the competition on career ladders. I outline a conceptual framework that characterizes the firm's decision problem, explicitly incorporating these frictions.

Recruiting new staff depends on the availability of suitable external candidates, which varies sharply across space because workers are not perfectly mobile across sectors or regions (Yi, Müller, and Stegmaier, 2024). In a frictionless market, spillovers from delayed retirements would directly reveal the elasticity of substitution between older and

younger labor. But when external hiring is costly or constrained, firms cannot easily replace older workers retained, and turnover frictions inflate the observed crowd-out. Thin external labor markets—commuting zones and industries in which suitable replacement workers are scarce—should therefore exhibit larger hiring declines, allowing me to separate true substitutability from constraints imposed by local hiring frictions. In line with this mechanism, I find that the negative hiring effects are concentrated in thin external labor markets. In thick markets, where turnover frictions are weaker, hiring responses are much smaller. These patterns show that external labor market thickness is a key mediator of spillovers from delayed retirements and that interpreting baseline crowd-out estimates as structural elasticities of substitution would be misleading unless one accounts for local hiring frictions.

Internal labor-market thickness also plays a central role. Several predictions that are consistent with those outlined in the model follow. First, crowding-out should be larger for promotions than for hiring, because incumbent workers are more substitutable than external hires. Second, crowding-out should be strongest within the occupations of older workers—especially in jobcells[4] with many coworkers competing for the same rungs on the ladder—while workers in other occupations may benefit from cross-occupation complementarities (Jäger and Heining, 2022). In establishments in which many coworkers share the older worker's occupation, promotion losses are substantially larger, reflecting more intense competition for limited slots. Because the reform increased promotions of focal workers by about 0.02 per additional focal worker, it pushed more workers into top positions and intensified congestion along the career ladder. This scarcity of advancement slots contributed to the fourfold crowd-out of coworker promotions. These patterns also appear in wage-bill adjustments: while focal workers' wages rise, coworker wage bills decline, especially in thick internal labor markets. Employers thus adjust the composition, rather than the scale, of their workforce, implying compressed hierarchies and persistent career costs for coworkers.

Imperfect substitutability among workers within a firm (Chan, 1996; Herrmann and Rockoff, 2012; Waldman, 2003) implies that retaining older workers with accumulated firm-specific knowledge can yield benefits, in line with human-capital theories. To test whether such complementarities operate across rather than within occupations, I decompose spillovers in multi-jobcell firms. Within jobcells—where tasks and ladders overlap—the promotion crowd-out is substantial. Across jobcells, however, I find no systematic declines and, when older workers possess substantial firm- or task-specific human capital, I find small positive wage spillovers. This highlights that complementarities among workers on distinct internal ladders can offset crowd-out pressures, consistent

[4]I define jobcells as 3-digit occupation groups within establishments.

with classic and modern theories of firm-specific human capital (Becker, 1962; Lazear, 2009).

Taken together, the results show that spillover magnitudes and signs depend jointly on limited external substitutability, cautioning against interpreting crowd-out estimates on external hiring as pure elasticities of substitution, and on the concentration of skills within internal labor markets, highlighting the value of older workers.

This paper contributes to the retirement literature by shifting focus from individual labor supply responses[5] to the smaller but growing body of work on intra-firm spillovers of rising retirement ages, thereby linking the retirement and internal labor market literature (Doeringer and Piore, 1971; Lazear and Oyer, 2004).[6] Existing evidence—mainly from Italy (Bianchi et al., 2023; Boeri, Garibaldi, and Moen, 2022; Carta, D'Amuri, and Von Wachter, 2024), and the Netherlands (Hut, 2024; Ferrari, Kabátek, and Morris, 2023)—offers mixed findings on whether older worker retention crowds out or complements younger colleagues.[7] One explanation for these discrepancies lies in firm heterogeneity: positive effects in Carta, D'Amuri, and Von Wachter (2024) may reflect differences in firm characteristics, such as firm size and underlying hiring practices correlated with such characteristics, as their heterogeneity analysis highlights that positive impacts are concentrated in larger firms. This paper provides new evidence from Germany, a large and institutionally distinct labor market, and offers a reconciliation of prior puzzles through incorporating internal and external labor markets into spillovers and novel mechanisms. The evidence presented here also contributes to earlier related literature that examines aggregate trade-offs between retirement and youth unemployment; policies in the 1980s and 1990s promoted early exits with this objective (Gruber and Wise, 2010), but the earlier studies find little systematic support and provide limited insight into underlying mechanisms. The nuanced answer to this question appears to be that the effect depends on the state of the internal labor markets and on the human capital

[5]See, among others, Geyer and Welteke (2021), Lalive, Magesan, and Staubli (2023), Manoli and Weber (2016), Mastrobuoni (2009), and Ye (2020).

[6]Related work studies firm-level spillovers and worker substitutability in settings of labor force exit, such as sudden worker death (Becker and Hvide, 2022; Bennedsen, Pérez-González, and Wolfenzon, 2020; Bertheau et al., 2022; Jäger and Heining, 2022; Isen, 2013; Illing, Schwank, and Tô, 2024; Poege et al., 2025; Sauvagnat and Schivardi, 2024), workers quitting (Kuhn and Yu, 2021), emigration (Dicarlo, 2022), childbirth and parental-leave absences (Bonney, Pistaferri, and Voena, 2025; Brenøe et al., 2024; Carta et al., 2024; Schmutte and Skira, 2023; Gallen, 2019; Ginja, Karimi, and Xiao, 2023; Corekcioglu, Francesconi, and Kunze, 2025; Friedrich and Hackmann, 2021; Huebener et al., 2024), and the introduction of paternity leave (Johnsen, Ku, and Salvanes, 2023).

[7]Further related work studies spillovers of retirement or labor force exit at different levels of aggregation. Evidence on intra-firm and local labor market spillovers includes studies from Portugal (Martins, Novo, and Portugal, 2009), Norway (Hernæs et al., 2023), and the 1992 German reform (Berg et al., 2025), as well as commuting-zone analyses in the US (Mohnen, 2025) and Italy (Bertoni and Brunello, 2021), and cross-country evidence (Kalwij, Kapteyn, and De Vos, 2010). Differences in institutional context, data coverage, and identification strategies make these studies informative but not fully comparable to the setting examined here.

of the older workers.

Unlike settings in which financial frictions shape workforce adjustments (Hut, 2024), the long pre-announcement horizon of the German reform makes liquidity constraints unlikely.[8] My findings are broadly consistent with slot-constraint logic (Bianchi et al., 2023): additional retention of older workers is nearly fully offset by reduced promotions and hiring among younger workers. Nevertheless, the average pattern conceals important heterogeneity. When suitable external replacements are scarce, turnover frictions amplify hiring crowd-out—echoing evidence on high replacement costs and thin labor markets (Ginja, Karimi, and Xiao, 2023; Jäger and Heining, 2022; Huebener et al., 2024; Schmutte and Skira, 2023). Inside a firm, promotion spillovers depend on internal structure and human-capital specificity: using establishment–occupation identifiers, I show that within jobcells—where tasks and ladders overlap—retained older workers intensify competition and reduce promotions of coworkers, whereas across jobcells their firm- and task-specific expertise can complement those of younger coworkers. Together, these patterns suggest that neither liquidity constraints nor pure slot-constraint mechanisms alone explain firms' responses. Instead, delayed retirements interact with thin external markets and concentrated internal skill hierarchies—consistent with human-capital and internal labor-market theories (Baker, Gibbs, and Holmstrom, 1994; Huitfeldt et al., 2023). This unified view helps reconcile divergent findings across countries and firm types as being possibly related to variations in hiring frictions and competition hidden in internal labor market structure, and cautions against treating crowd-out estimates as direct elasticities of substitution.

The rest of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 describes the data source, the sample construction, and the corresponding identification strategy. Section 4 presents the baseline results, followed by section 5, which outlines a simple model of firm decisions and quantifies the elasticity of substitution. Section 6 shows intra- and inter-jobcell spillovers. Finally, I conclude in section 7.

2 Institutional Setting

Features of the labor market in Germany.

Wage setting and rigidity. Germany features relatively decentralized wage setting, allowing firms to deviate from collective agreements (Dustmann et al., 2014; Jäger and Heining,

[8] While prior work examines upstream labor supply responses to pension reforms (Carta and De Philippis, 2024; Mastrobuoni, 2009; Rabaté, Jongen, and Atav, 2024; Staubli and Zweimüller, 2013), I provide the first evidence on upstream spillovers within firms.

2022). Despite this flexibility, wages remain downward-rigid due to unions and incentive contracts for lower-skilled workers, and to firm-specific human capital for higher-skilled workers (Franz and Pfeiffer, 2006).

Employment protection. Older workers are strongly protected under the Equal Treatment Act (AGG),[9], and severance pay rises steeply with tenure (Hut, 2024). Germany has relatively stable jobs and relatively high severance pay, compared to countries such as the US. Together, wage rigidity and dismissal protections imply that deferred compensation (Lazear, 1979) makes separating from older workers costly, so firms are more likely to adjust to workforce aging by reducing hiring rather than by dismissing older workers.

Industry segregation by gender. Germany exhibits pronounced gender segregation across industries. Table E.1 shows that women are overrepresented in service-oriented sectors, while men dominate goods-producing and infrastructure-related industries. A comparison between Panels A and B indicates that this pattern remains largely stable over time.

Key features of the German public pension system. The German pension system has three main pillars: public, occupational pensions, and private provisions. Public pension insurance is the most popular choice among the working population, covering approximately 90% of the German workforce (Geyer and Welteke, 2021; Zwick et al., 2022). The public pension system consists of a pay-as-you-go scheme, where the retirement pensions are financed by social security contributions of the insured workers and taxes.

Germany has two statutory retirement ages: the early retirement age (ERA), the earliest age at which a pension can be claimed, and the normal retirement age (NRA), the earliest age to claim a full pension without actuarial deductions. On the regular pathway to retirement, which requires only five years of social-security contributions, the ERA equals the NRA. Reduced ERAs exist only for special pathways targeted at specific groups— including women, long-term insured workers, and the unemployed—upon them meeting the relevant eligibility criteria. Retiring between the ERA and NRA entails actuarial deductions of 0.3% per month.[10] Workers respond strongly to these statutory ages, which function as reference points and generate pronounced bunching (Seibold, 2021). Consequently, reforms that shift the ERA or NRA induce significant adjustments in labor supply (Riphahn and Schrader, 2021; Geyer and Welteke, 2021). Importantly, the 0.3% monthly deduction is low by international standards and not actuarially neutral (Queisser and Whitehouse, 2006), making ERA claims particularly attractive. Cross-country evidence shows that ERA are more successful in raising the effective age of

[9]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).

[10]For a woman in the 1951 cohort, retiring at age 60 implies a 10.8% permanent reduction.

retirement than changes in NRA (Boeri and van Ours, 2021). Although Germany does not mandate retirement, continued employment beyond the NRA requires contract renewal, which employers can decline. These institutional features make early-retirement reforms especially well-suited for studying intra-firm spillovers of workforce aging.

The 1999 reform: Abolishment of the women's early-retirement pathway. Before 1999, women could claim the *"Old-Age Pension for Women"* at age 60 if they had accumulated at least 15 years of social security contributions, including ten after age 40; roughly 60% of the 1951 cohort qualified (Geyer and Welteke, 2021). The 1999 reform abolished this pathway for women born on or after January 1, 1952, creating a sharp discontinuity in retirement eligibility. Another common route into early retirement was the *"Long-Insurance Pathway"*, available to workers with sufficiently long contribution histories. For most affected women—about 90% of those previously eligible, who also met the 35-year contribution requirement for the long-insurance pathway—the early retirement age (ERA) rose from 60 to 63. Women eligible only for the *"Regular Pathway"* experienced an even larger increase of up to 5.5 years.[11] Further institutional details, such as the description of pathways, are provided in Appendix A.

Discontinuity in birth cohorts and labor supply responses. This discontinuous three-year rise in ERA is the largest statutory shift affecting adjacent cohorts in recent years in Germany (Panel A of Figure 1). It generated substantial labor-supply responses: Geyer and Welteke (2021) document a 13.3 p.p. increase in employment at ages 60–62 with no offsetting rise in disability, unemployment, or inactivity. Conditional on employment at ages 58–59, the employment discontinuity at ages 60–63 reaches 17.3 p.p. (Badalyan, 2025) (Panel B in Figure 1). Consistent with these patterns, Figure D.1 shows pronounced bunching at the relevant ERA (60 and 63) and NRA thresholds (65 and 65.5) for the 1951 and 1952 cohorts.

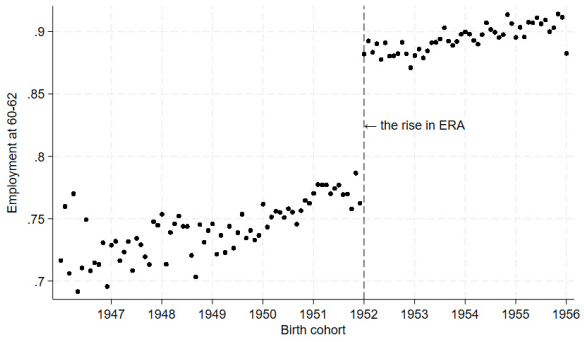
Reform timing: upstream, main reform, and downstream periods. The 1999 reform was approved on January 1, 1999 (Gohl, 2023), when the first affected cohort was only 47 (see Figure A.1). Because the first treated cohort reached the pre-reform early retirement age (ERA) of 60 in 2012, the policy generates three analytically distinct periods. The upstream period spans 1999–2012, when firms could anticipate the reform and adjust their workforces before workers reached ERA. The main reform period covers the years when the 1952 cohort transitioned through the pre- and post-reform ERAs (ages 60–62), during which the delayed-retirement shock materializes. The downstream period begins when affected cohorts pass the post-reform ERA and regain the option to exit at age 63. While the main reform period is the core focus, the upstream and downstream periods

[11] The regular pathway retirement age also increased due to the 2007 reform, which raised the NRA in small increments; thus, the NRA rose from 65 for the 1951 cohort to 65.5 for the 1952 cohort.

Figure 1: Early retirement age and employment at ages 60–62: Discontinuities across birth cohorts

Panel A: Assignment rule

Panel B: Scatter plot (Badalyan, 2025)



Notes: **Panel A** shows the policy rule for the earliest age a woman could claim a pension by birth cohort. **Panel B** plots the fraction of women employed at ages 60–62 across 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.

are informative about anticipatory adjustments and post-shock recovery.

The reform offers a useful case to study firm responses to workforce aging. Its sharp, cohort-based three-year increase in early retirement age provides clean identification and primarily affected women, limiting general-equilibrium concerns, such as shifts in industry composition by gender. The pre-announcement enables analysis of anticipatory behavior. Because firms cannot foresee exits at the early retirement age—contracts end only at the NRA—the reform generated unexpected retention, plausibly affecting younger workers who would normally be replacing retirees.

3 Data and Empirical Framework

Identifying the effects of increased older-worker employment on coworkers and external hires is difficult due to nonrandom selection of workers into late-career employment based on their unobserved characteristics and of firms that employ more older women. I leverage the 1999 reform, which exogenously raised women's early retirement age. I first describe the data source and sample construction—private-sector establishments with 5 to 500 employees that employed at least one focal worker in 1998 (upstream analysis) and 2008 (main reform period). I then outline the outcome variables, descriptive patterns, and the identification strategy used to estimate the reform's impact on coworker promotions and on external hiring.

3.1 Data Source and Sample Construction

I proceed in two steps. First, I describe the data source, and next, the sample construction necessary to identify the intra-firm spillovers.

Integrated Employment Biographies Database. The source of the dataset used in this paper is the Integrated Employment Biographies (IEB) database, provided by the Data- and IT-Management (DIM) at the Institute for Employment Research (IAB).[12] It is based on the integrated notification procedure for health, pension, and unemployment insurance. Data are collected from employers on all of their employees subject to Social Security. Hence, such data exclude workers with self-employment spells and civil servants. Such exclusion does not matter for this study, because participation in the public pension system in Germany is mandatory for everyone except self-employed and civil servants. Employers must file notifications of their workforce at least once each year, by June 30^{th}, or whenever there is a change in employment spells, such as the start of employment, exit, or change of a contract. These data include all the workers subject to social security in Germany until 2021, with a starting date in 1975 for West Germany and 1992 for East Germany.[13]

The data include precise day-to-day information on the start and end dates of employment spells and wages, which include overtime pay and bonus payments.[14] The data are rich in demographic, occupational, and establishment-level variables. The demographic variables include birth month and year, gender, education level, nationality, and district-level place of residence. The workplace data include detailed 3-digit occupational codes based on the 1988 classification of occupations (5-digit occupational variables starting from 2011), contract type by the number of working hours (part-time, full-time), and employment type (for example, regular employment opposed to traineeships), etc. The establishment variables include 3-digit industries and the district-level location of the establishments. The detailed occupation and industry codes allow me to observe teams within establishments and to count the number of available internal and external substitutes for retiring workers, as described in section 5.

The data consist of a universe of single locations of multi-site firms, that is, establish-

[12] I use data from the full universe of German employment records *(IEB, version 17˙00˙00˙202212)* of the IAB. Due to its administrative origin, these data are confidential and can only be accessed on-site at IAB. Access for guest researchers requires clearance from the German Federal Ministry of Labour and Social Affairs.

[13] Throughout the paper, I refer to East Germany to define New Länder (including Berlin), and to West Germany as the current regions of the former Federal Republic territory.

[14] Wages are top-coded. Because the reform I study affected primarily women, and given that due to occupational and industry segregation by gender, women are less likely to pass the threshold, potential wage correction would impact only a small fraction of workers in my sample (Drechsler, Ludsteck, and Moczall, 2023); hence, I refrain from performing such imputations.

ments.[15] This data feature is a significant advantage in my research, because I can identify the spillovers on local coworkers. Nevertheless, I follow the existing literature and use the terms establishment and firm interchangeably throughout this paper (Card, Heining, and Kline, 2013; Dustmann, Ludsteck, and Schönberg, 2009; Jäger and Heining, 2022), because single-establishment firms constitute the majority of the data (Jäger and Heining, 2022). More information about the data can be found in Jacobebbinghaus and Seth (2007).

3.1.1 Sample Construction for the Upstream Period

First, I construct a worker–year panel following Dauth and Eppelsheimer (2020), aggregating annual records as of June 30$^{\text{th}}$—the date when employers submit annual workforce notifications and the reference point used in the IAB Establishment Panel.[16] I define *focal workers* as female employees born within two years of the 1952 cutoff—the first cohort affected by the abolition of the female early-retirement pathway—excluding miners and sailors, whose special retirement rules are not identifiable in the data (Lorenz et al., 2018).[17] I retain all establishments that employed at least one focal worker in 1998. Sampling firms prior to the reform avoids endogeneity arising from workforce adjustments after the policy change. The identification strategy compares firms with similar baseline characteristics and the same total number of focal workers, but quasi-random variation in the number born just after the cutoff (the treated focal workers; see section 3).

I restrict the sample to private-sector establishments[18] with 5–500 employees in 1998. I exclude the public sector because employment dynamics differ markedly (Oberfichtner and Schnabel, 2019) and substitutability responses may be muted by politically fixed budgets (Ginja, Karimi, and Xiao, 2023). Firms with fewer than five workers are removed to avoid cases in which focal workers are firm owners. Establishments with more than 500 employees are excluded due to administrative data limits; this threshold corresponds to the 98$^{\text{th}}$ percentile of the size distribution and is standard in spillover studies using similar identification strategies (Ginja, Karimi, and Xiao, 2023). After defining the firm sample, I keep all workers employed in these establishments from 1995 to 2019—four pre-announcement years for baseline trends and extending until all focal workers have reached ERA and NRA. I restrict the sample to workers in standard employment re-

[15] The assignment of establishment identifiers is based on ownership, location at the municipality level, and industry. Firms may be assigned multiple establishments if they belong to different sectors and/or locations.

[16] June 30$^{\text{th}}$ aligns reporting across datasets and captures the administrative employment stock.

[17] For details, see Deutsche Rentenversicherung Knappschaft-Bahn-See.

[18] Public-sector industries are defined as 5-digit classifications beginning with 84–85 and 99, based on 2008 classification of industries.

lationships (employees liable to social security and those in vocational training). As a result, individuals in partial retirement are not classified as employed in this analysis.[19] These restrictions (sampling in 1998, at least one focal worker, private sector, and firm size 5–500) and their impacts on sample size are summarized in Panel A of Table E.2.

3.1.2 Sample Construction for the Main Period

To estimate the effects of the reform between the pre-reform ERA and post-reform NRA, I employ the same sampling steps as above, except that now, instead of sampling the firms in the pre-reform year 1998, I sample them in 2008- a year in which all focal workers were under the age of 60. Resampling is necessary because many firms appear only after 1998 and employ focal workers. The choice of 2008 reflects a trade-off between proximity to retirement eligibility and measurement error in treatment assignment. Defining exposure substantially earlier would rely on workforce composition observed many years before retirement decisions are made, weakening the connection between baseline exposure and actual reform-induced retention. By 2008, all focal cohorts (1950–1953) remain below the pre-reform early retirement age (60) while being sufficiently close to the relevant retirement margin to provide an informative measure of potential exposure.[20]

Panel B in Table E.2 records the sample size after each sample restriction. The sample for the main reform period consists of 160,667 establishments that employed 1,234,969 workers in 2008, 414,209 of which were focal workers. Over 94% of these focal workers would be eligible for the women's pathway to retirement if it were not abolished.[21] Focal workers constitute approximately 10% of the total workforce on average (Panel A in Figure D.7).

To assess how the selection criterion affects my sample, Table E.3 and Table E.4 show comparisons of establishments in the analysis sample with the random sample of all the establishments in Germany in 1998 (Panel A), the pre-reform year, and 2008 (Panel B). The sampled establishments have, on average, more women and part-time workers, which is expected, given that I keep only firms that have at least one focal worker in my sample in 1998 or 2008. The sampled establishments and random samples have roughly equal

[19] Partial retirement corresponds to separate employment status categories in the data (e.g., codes 103 and 119) and is therefore excluded by construction. While partial retirement was an important pathway during this period, it reflects a transition out of standard employment relationships. The focus of this paper is on how retention of older workers in standard employment affects internal labor markets within firms.

[20] The choice of 2008 was determined ex ante when requesting the customized IEB extracts from the IAB. Because data access requires pre-specified extraction programs and variables, the baseline year was fixed prior to the empirical analysis, limiting the scope for ex post sample selection or year-specific data mining.

[21] Own calculations based on full employment biographies of focal workers.

likelihood of being located in the West or East, and overall, the industry composition of the establishments is also similar, except the construction sector is underrepresented, while the health sector is overrepresented in my analysis sample. This is likely because there is some gender segregation in industry employment in Germany (see the discussion in section 2). As noted above, public sector establishments are absent in the data by construction.

To assess the composition of establishments with different levels of reform exposure, Table E.5 reports baseline characteristics across quartiles of the share of treated focal workers in total employment. Because the sample contains a large number of establishments, even economically small differences are often statistically significant. Therefore, balance is evaluated primarily using standardized differences rather than statistical significance alone. Moreover, the balance table should be interpreted as descriptive evidence rather than a formal identification test. In the regression framework, treatment intensity is captured by the number of treated focal workers conditional on both the number of focal workers and total establishment employment. No single balance measure can fully reproduce this multidimensional treatment definition. The reported treatment share (number of treated focal workers in the total workforce), therefore, serves only as an informative summary of exposure.

The largest imbalances are observed for establishment size, the number of focal workers, and baseline hiring activity, while differences in industry composition and establishment age are also present. These patterns motivate the inclusion of controls for the number of focal workers, total employment, baseline hiring, and interactions of year fixed effects with industry, establishment age, and region. Workforce composition variables exhibit smaller imbalances and are considered in robustness specifications.

3.2 Main Outcome Variables

Below, I describe the main outcome variables used in this paper. There are two main outcome variable groups- turnover and profitability outcomes, and they are defined similarly for the upstream and main reform period.

To measure the turnover variables, I count the number of workers in given year t who are *hired* and *separated* from the establishment. To count establishment-level hiring, promotions, etc., I need to define these outcomes at the individual level and then aggregate them by year and firm. To further capture heterogeneity in career-stage exposure to the reform, these turnover variables are further decomposed by age group, distinguishing young workers (ages 15–34) from middle-aged workers (ages 35–54), and by gender, as

the reform primarily extended the working lives of women.

External hires and separations. I define *external hires* as workers employed in year t but not $t-1$. These workers can have employment biographies. For example, they may come from different establishments, from nonemployment, or be new graduates who have just entered the labor force. Similarly, *separation* is recorded if employment is recorded in a current year but not in the following year.

Promotions. I define *promotion* based on Ginja, Karimi, and Xiao (2023) and Bronson and Thoursie (2019), which rely on the relative real wage growth of individuals within a firm.[22] First, I deflate wages by consumer price indices in 2015, and, following the recommendation of Drechsler, Ludsteck, and Moczall (2023), I allocate lump-sum payments to regular employment spells weighted by spell length. Own-wage growth is computed as a logarithmic difference in real wages relative to the previous year. As a next step, I compute the mean wage growth of coworkers in the establishment. Relative wage growth is defined as the difference between a worker's own wage growth and the average wage growth of coworkers in the establishment. I define promotions as a dummy if an employee's own real wage growth is at least ten log points higher than their mean coworker wage growth. This measure of promotions captures wage growth through establishment-level wage hierarchies and excludes wage increases that are due to reasons unrelated to promotions, such as collective bargaining or firm performance. This measure captures upward movements within the firm's internal wage hierarchy rather than formal job title changes. In settings with structured wage ladders, such relative wage increases are closely aligned with promotions, as they reflect movement to higher-paying positions within the establishment. While this proxy does not directly observe occupational transitions, it is widely used in administrative data where job titles are either unavailable or are measured with error (Ginja, Karimi, and Xiao, 2023; Bronson and Thoursie, 2019).[23]

Changes in coworker wages could reflect both working hours and promotions. It is difficult to test for the intensive margin adjustments in German social security data because

[22] Although German social security data include 5-digit occupation codes, they are not a reliable measure of promotions. Establishments often fail to update codes when workers advance, many occupations lack a hierarchical 5-digit structure that maps onto internal ladders, and the detailed codes are only available from 2011. As a result, administrative occupation changes would misclassify both upward and lateral mobility. I thank Katja Wolf and Wolfgang Dauth from the Institute for Employment Research for these insights.

[23] The intra-firm spillovers literature often proxies promotions using large individual wage increases (Bianchi et al., 2023; Ferrari, Kabátek, and Morris, 2023). I prefer relative wage growth over individual wage growth because the measure is defined relative to coworkers within the same establishment and therefore nets out common wage shocks, including collective bargaining agreements and firm-wide pay adjustments. While individual bonuses or renegotiations may still contribute to observed wage growth, such changes are unlikely to systematically generate large relative deviations across workers within the same establishment. Appendix C.1 compares the baseline measure to several alternative promotion definitions and reports substantial agreement across measures.

working hours are available only for certain years, and the only variable available throughout the years is the part-time versus full-time indicator.[24] I define promotion conditional on a constant working-time status (part-time or full-time). I exclude workers who switch between part-time and full-time employment from the promotion measure to avoid mechanically classifying such transitions as promotions. Negative wage growth may still occur due to changes in hours within these categories, but such adjustments are unlikely to generate large relative wage increases that meet the promotion threshold. Hut (2024) finds no effect on hours worked or hourly wages in a similar reform in the Netherlands, and argues that these outcomes are set in collective labor agreements and are difficult to change (Cahuc, Carcillo, and Le Barbanchon, 2019); therefore, most of the wage effects captured likely reflect the promotions.

To assess the plausibility of the promotion measure, I compare it to three alternative indicators of career advancement: a measure based on individual wage growth (Bianchi et al., 2023; Ferrari, Kabátek, and Morris, 2023), an occupation-relative wage-growth measure, and occupational skill upgrading. The baseline measure exhibits strong agreement with the two wage-based alternatives (correlations of 0.714 and 0.724), while agreement with occupational skill upgrading is weaker, consistent with many promotions occurring within occupations rather than through changes in occupational classifications. In addition, the 10 log-point threshold corresponds approximately to the upper decile of the distribution of relative wage growth within establishments (Figure C.1, Table C.2). Together, these results suggest that the promotion measure captures economically meaningful career advancement while filtering out establishment-wide wage changes. Appendix Appendix C provides additional validation exercises and robustness checks.

I construct promotion measures separately for focal workers and coworkers (nonfocal workers). I define the coworker promotion measure excluding focal workers, so that it captures spillover effects on other workers without mixing in direct effects of the reform. After constructing the treatment and outcome variables at the worker level, I aggregate them to the establishment level to study how delayed retirement affects firms' employment and wage dynamics between 1995 and 2020.

Wage bills. Given that I do not observe profit outcomes for the establishments, I rely on the existing literature (Dustmann et al., 2022; Huebener et al., 2024) and use (1) wage bills, and (2) firm closure, defined as a transition to zero employment with no subsequent re-employment (Huebener et al., 2024).

[24] Fitzenberger and Seidlitz (2020) argue that more than half of women have been employed part-time in recent years in Germany.

3.3 Identification: Generalized Difference-in-Differences

In an ideal experiment, firms employing workers near retirement age would be randomly assigned to a regime in which the pensionable age is increased or left unchanged. In Germany, however, the pension reform applied uniformly to all women born after 1951, implying that exposure to the reform varies across firms only through differences in workforce composition. Firms employing substantially younger or older workers may differ systematically, creating a key identification challenge.

To address this concern, I restrict attention to establishments that employed at least one woman born within a narrow two-year window around the 1952 cutoff. This restriction ensures that firms are comparable in workforce composition while generating quasi-random variation in exposure to the reform through the number of women born just after the cutoff. Firms that happened to employ more women born in 1952–1953 in 1998 (for upstream analyses) or 2008 (for the main reform period) were mechanically more exposed to the increase in the early retirement age.

I exploit this variation to estimate the effect of employing an additional treated focal worker—i.e., a woman whose retirement age was raised—on firm-level outcomes, including older-worker retention, coworker promotions, and external hiring. The strategy is implemented separately for the upstream period and the main reform period, when no treated worker had yet reached retirement eligibility.

Upstream effects. To identify the firm responses to their labor inputs (coworkers and hired workers), I follow an identification similar to that employed by Hut (2024) and compare establishments with a similar workforce composition (total number of focal workers in the reform year) but with a variation in the number of treated focal workers, that is, workers who experienced a rise in the ERA (see Figure A.2 for the graphical illustration of identification strategy). The resulting estimation strategy is generalized difference-in-differences.

I estimate a generalized Difference-in-Differences (DiD):[25]

[25]The difference from the identification strategy employed by Hut (2024) is that my 1999 reform affected primarily women, while the Dutch reform in Hut (2024) affected both genders. In addition, the German reform was uniform by industries and other characteristics, while in the Dutch reform the raise of retirement age and its amount depended on the industry and other characteristics.

$$y_{jt} = \alpha_j + \lambda_t + \sum_{\substack{\tau=1995 \\ \tau \neq 1998}}^{2019} \mathbb{1}\{t = \tau\} \left(\beta_\tau \cdot N\_TreatedFocal_j + \gamma_\tau \cdot N\_Focal_j + \zeta_\tau \cdot N_j\right) + \epsilon_{jt} \tag{1}$$

where y_{jt} are the outcomes of interest (number of hires, promotions of both focal workers and their coworkers, number of separations, etc.), $N\_TreatedFocal_j$- number of workers in 1998 that belong to 1952-1953 (treated focal) cohorts, N_Focal_j- number of workers in 1998 that belong to 1950-1953 (treated and control focal) birth cohorts, N_j- total number of workers in 1998. λ_t- year fixed effects controlling for time-varying shocks common to all establishments and α_j- firm fixed effects.

The coefficient of interest, β_τ, shows the difference in the evolution of the outcome variable across firms with a similar workforce composition (including, the number of focal workers, number of old workers (men who will reach ERA and NRA around the main reform period), and the total number of workers) but different exposure to the reform (number of workers who experienced the rise in ERA among the focal workers) before and after the intervention. In other words, it estimates the effect of having an additional worker who experienced a rise in the ERA (born to the right of the cutoff, that is, 1952–1953) employed in the pre-reform year, 1998, on the outcomes of interest. The reference period is 1998 (treatment construction year).

I interact the treatment variables with time variables: either the flexible time dummies $\mathbb{1}\{t = \tau\}$ to observe how the treatment effects evolve over the years (where $\forall t = 1995, ..., 2019$). The interactions with time dummies help me to visually analyze the detailed effects of the reform over the years. To aid interpretation, I pool together the years when the workers in the 1950-1953 cohorts turn 60-65. In the simplified DiD model, I aggregate the time dummies to *Post*, which stands for the years from 2012, when the first treated cohort (born in 1952) turns 60, until 2017, when she turns 65.

Main reform and downstream effects.

The corresponding identification for main reform and downstream oeriod resembles that of upstream effects:

$$y_{jt} = \nu_j + \mu_t + \sum_{\substack{\tau=2005 \\ \tau \neq 2008}}^{2020} \mathbb{1}\{t = \tau\} \left(\delta_\tau N\_TreatedFocal_j + \rho_\tau N\_Focal_j + \omega_\tau N_j + \xi_\tau X_j\right) + u_{jt} \tag{2}$$

The coefficient on the interaction between treatment intensity and post-reform enaction years is therefore identified from within-firm changes in employment outcomes relative to pre-reform trends, across firms with differing exposure intensities. This specification exploits plausibly exogenous differences in exposure to the reform arising from historical workforce composition. Conditional on firm fixed effects, year fixed effects, and baseline controls, identification comes from variation in treatment intensity across otherwise comparable establishments. In some parts of the paper, I estimate analogous specifications at the firm-occupation level, adding the subscript c to denote occupations.

A potential concern is that firms with different levels of reform exposure may differ systematically in characteristics correlated with employment dynamics. To assess this issue, Table E.5 reports baseline characteristics across quartiles of treatment intensity. Because the treatment in the empirical specification is defined by the number of treated focal workers conditional on both the number of focal workers and total employment, no single balance measure can fully reproduce the variation used for identification. The balance tests should therefore be interpreted as descriptive rather than as a formal test of identification. They indicate that exposure is most strongly associated with establishment size, the number of focal workers, baseline hiring activity, industry composition, and establishment age. Accordingly, the specification includes firm fixed effects that absorb all time-invariant firm characteristics (ν_j), year fixed effects that capture common macroeconomic shocks (μ_t), and interactions of year fixed effects with the number of focal workers, total employment, baseline hiring in 2008, establishment age categories, broad industry groups, and East German location (X_j). These controls allow for differential trends across establishments with different workforce composition and labor-demand conditions, improving comparability across firms.

Identification assumption. The key identifying assumption is a parallel-trends condition: absent the reform, outcomes such as hiring and promotions would have evolved similarly across firms with different treatment intensities. While counterfactual post-treatment trends are unobservable, I assess this assumption by showing that event-study coefficients in pre-treatment years (1995–1997 for the upstream period and 2005–2007 for the main reform period) are statistically indistinguishable from zero.

I further support the identification strategy with falsification tests by estimating the effects on placebo cohorts and placebo gender groups in section 4. In addition, I exploit the structure of internal labor markets by estimating both intra- and inter-jobcell spillovers. If the estimated effects reflected unobserved confounders or aggregate shocks, they would plausibly appear across occupations within firms. Instead, crowd-out effects are concentrated within jobcells, while spillovers across jobcells are absent (section 6), lending support to the causal interpretation. Finally, Panel B of Figure D.7 shows that, within

the estimation sample, the share of workers born after the cutoff is close to 50% and varies smoothly across firms with different treatment intensities, consistent with quasi-random exposure to the reform.

Standard errors. I cluster standard errors at the level of treatment variation. In the baseline specifications (Equation 1 and Equation 2), standard errors are clustered at the establishment level. In specifications at the establishment-by-occupation level (section 6), standard errors are clustered at the establishment-by-occupation level to account for within-cell correlation.

Effect heterogeneity. The average effects that the Equation 2 estimates could hide substantial heterogeneity across different firms, industry, and/or labor market characteristics. I estimate the same regressions on the corresponding subsamples to study the effect of heterogeneity.

4 Intra-Firm Personnel Decisions

4.1 Upstream Effects

As discussed in section 2, there are three main effects of interest- the upstream effects (before the pre-reform ERA at the age of 60), the main reform effects (between the pre-reform ERA and NRA, that is, 60-65 years old), and the downstream effects after reaching the NRA, corresponding to the three periods in Figure A.1. First, I present the upstream effects on focal worker retention, followed by spillovers on promotions, external hiring, and the wage bill.

Although Carta and De Philippis (2024) find significant upstream effects of employment, Rabaté, Jongen, and Atav (2024) and Mastrobuoni (2009) find no sizeable effects in the Netherlands and the US. Neither of these studies analyzed spillover effects. I find upstream effects on employment and retention of focal workers starting from around 55 years old, and almost no significant upstream spillover effects.

Focal worker retentions. Panel A in Figure D.3 shows that establishments with more treated focal workers (1952–1953 birth cohorts) in 1998 retained more focal workers (1950–1953 birth cohorts) from around 2007. This pattern indicates that treated workers increasingly stayed with their employer beyond the age of 60, the former early retirement threshold. An additional treated focal worker employed in 1998 leads to around 0.140 more focal worker retentions. Although some of the retained workers were on part-time

contracts in 2012-2017 (31%), most were on full-time contracts (69%).

Null upstream spillover effects on coworker promotions and hiring. A natural follow-up question is whether the increase in the number of focal workers spills over to coworker promotions and external hiring. Having an additional treated focal worker in 1998 does not lead to any changes in coworker promotions (Panel B of Figure D.3) or in the number of external hires (Panel C in Figure D.3). There is a small decrease in the number of hired workers up to three years after the reform, but overall, there are no significant effects afterward, even in the years after the focal workers reach 60. The 1998 sample consists of firms observed long before the reform became binding and therefore disproportionately includes surviving, more established firms by the time outcomes are measured in pre-reform and post-reform retirement ages, 2012–2017. Bianchi et al. (2023) argue that spillover effects are more likely to be concentrated in non-growing firms. This sample composition may attenuate observed spillover effects on promotions and hiring, even though first-stage retention effects are similar across samples. For this reason, I focus on the 2008 sample for second-stage estimates, as it includes a large chunk of the labor market that is missing in the 1998 sample.

Firm closure. Panel D of Figure D.3 shows the effects of an additional treated worker employed in 1998 on the probability of firm closure. There are no effects up to 2010, followed by a small negative effect around 2010-2014, around the main reform period. These results show that, if anything, having an additional focal worker who faced an increase in retirement age (due to gender-neutral retirement ages) does not lead to firm closure, but, on the contrary, has a positive effect on firm closure around the reform years.

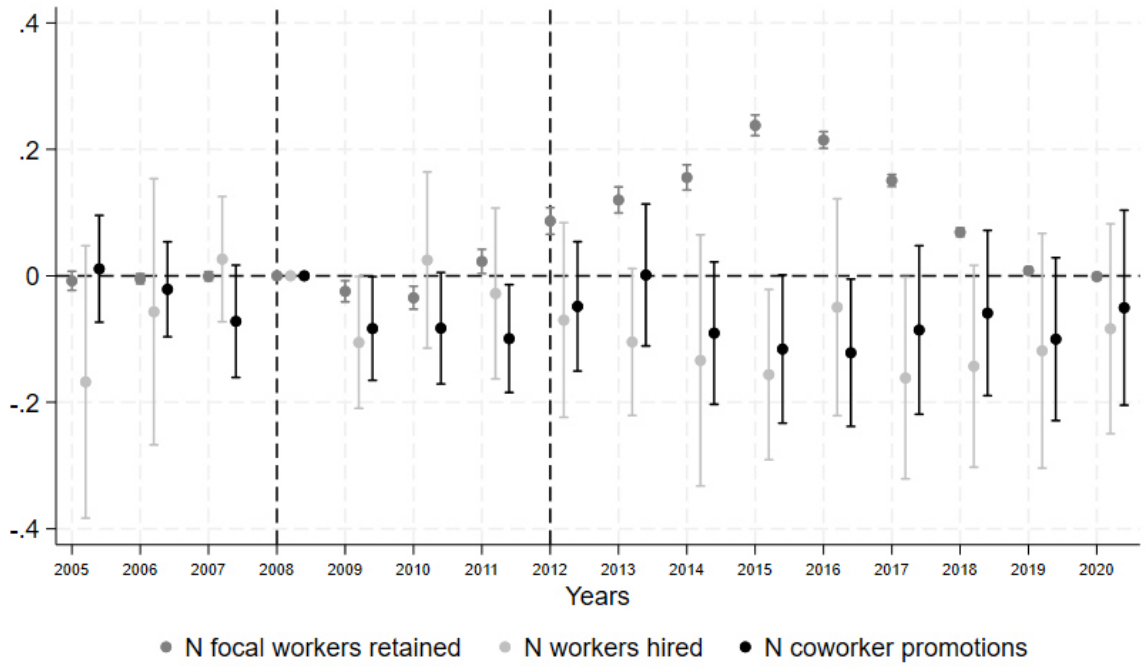
4.2 Main Reform Effects

In the previous section, I show that upstream firm responses occur only after focal workers reach older ages (around 55+), with almost no significant spillover effects on coworker promotions or external hiring before age 60. I now turn to the "main reform" period, when the cohorts directly exposed to the increase in the early retirement age (ERA) reach the ages of 60–65, and examine how firms adjust internal personnel policies and hiring in response to delayed retirements.

Positive direct effects on retention and promotion of focal workers. Figure 2 shows that having an additional treated focal worker (a woman born in 1952–1953) employed in 2008 leads to a sizeable increase in the number of focal workers (1950–1953 birth cohorts) who remain employed at the same establishment once they reach age 60. The event-study coefficients lie between 0.08 and 0.25 additional retentions per year dur-

ing 2012–2017.[26] When I aggregate over these main reform years, one additional treated focal worker in 2008 increases focal retention by $\Delta R = 0.163$ workers.[27]

Figure 2: The effect of an additional treated focal worker employed in 2008 on focal worker retentions, external hiring, and coworker promotions



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of focal worker retentions, external hiring, and coworker promotions in each year. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Negative spillover effects on coworker promotions and hiring. Next, I examine whether the retention of older workers comes at the expense of younger and middle-aged employees. Figure 2 summarizes the main margins: focal worker retentions, coworker promotions, and numbers of external hires. The aggregated treatment effects over 2012–2017 show clear evidence of negative spillovers: one focal worker employed in 2008 generates -0.075 fewer coworker promotions and -0.103 fewer external hires.[28]

[26]The temporary and small decline in adjusted retention during 2009–2010 may reflect short-run adjustment following the reform, such as changes in working hours. Retention effects become positive and economically meaningful from 2012 onward until 2017, suggesting that firms and workers required time to adjust to the new retirement incentives. Therefore, I pool together the 2012-2017 years.

[27]All treatment effects in this section refer to the impact of one additional treated focal worker employed in 2008, with coefficients averaged over the years 2012–2017.

[28]The timing of promotion effects reflects cohort-specific retirement eligibility rather than an immediate response to the reform. In particular, untreated focal workers reach the pre-reform early retirement age earlier and begin to exit before 2012, while treated cohorts remain employed. This already generates

Substitution rates: how many hires and promotions are forgone per retained older worker? To quantify how firms substitute between older workers and other labor inputs, I relate the reform-induced increase in focal workers' retentions to the corresponding decline in hires and promotions. For any outcome Y, I compute the ratio $-\frac{\Delta Y}{\Delta R}$, which measures how many forgone units of Y correspond to one additional older worker retained due to the reform.[29] For total external hiring, the substitution rate is 0.63. In other words, retaining one additional older worker reduces total hiring by about 0.63 workers. For coworker promotions, the corresponding rate is 0.46, so roughly 0.46 coworker promotions are lost per extra retention of an older worker.

Allocation of promotion "slots". An alternative normalization uses the increase in focal promotions, 0.017, as the denominator and captures how promotion "slots" are reallocated within a firm. The implied ratio of 4.4 suggests that, for each additional promotion of an older focal worker, about 4.4 coworker promotions are forgone. This large magnitude reflects that focal promotions are a relatively rare margin—most retained older workers remain in their positions—so the denominator is small. It also reflects that promotions and operate along hierarchical ladders, where a single blocked senior position can prevent multiple downstream promotions. Together, these patterns highlight that the reform does not simply keep older workers in place but reshapes internal promotion ladders.

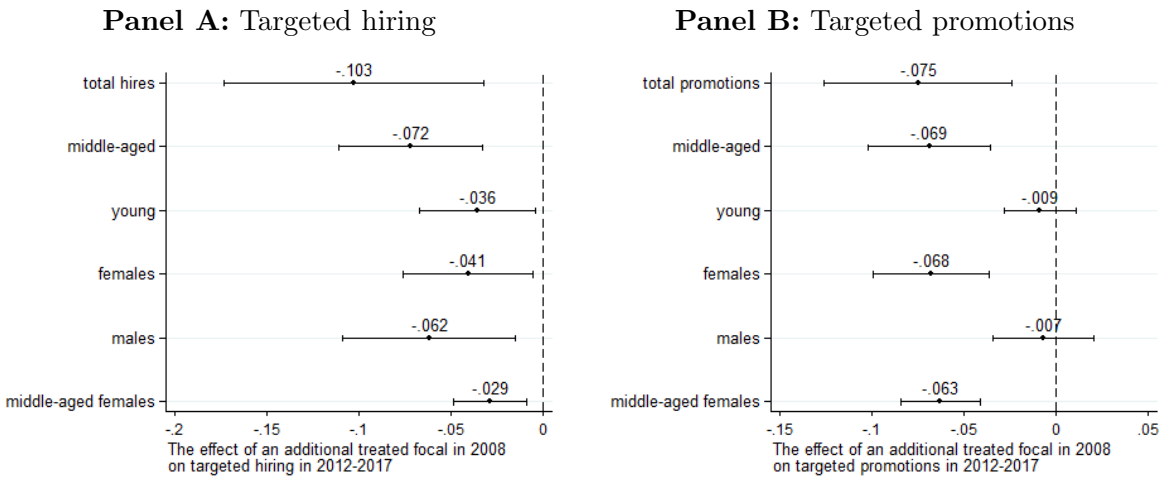
Who is crowded out? Heterogeneity by age and gender. To understand which groups of workers are most affected, I estimate treatment effects on hiring by age and gender. The aggregated coefficients for 2012–2017 are displayed in Figure 3.

To quantify how firms substitute between older workers and other labor inputs, I relate the reform-induced increase in the retention of focal workers to the corresponding decline in hiring across different groups. For each group g, I compute the ratio $-\Delta H^g/\Delta R$, which measures how many forgone hires of type g correspond to one additional older worker retained due to the reform. The resulting substitution responses are economically meaningful. Firms reduce overall hiring by 0.63 workers for each additional focal worker retained. The largest component is the decline in hiring of middle-aged workers (0.44), followed by a smaller substitution away from young workers (0.22). Such a result could

differences in workforce composition and promotion opportunities prior to 2012. The effects become fully pronounced from 2012 onward, when treated cohorts reach their pre-reform designated retirement ages but are no longer able to exit. For this reason, I focus on the period 2012–2017 when the reform is fully binding. These dynamics are therefore driven by differential retirement eligibility across cohorts.

[29] Formally, this ratio is a reduced-form *substitution rate in headcounts* rather than a structural Hicksian elasticity of substitution, because both ΔY and ΔR are estimated in levels (numbers of workers). Under the approximation that the changes are small relative to baseline employment, this ratio can be interpreted as an elasticity-like object, but I avoid a structural interpretation and use the ratio as a transparent measure of crowd-out per additional retained older worker.

Figure 3: The effect of an additional treated focal worker employed in 2008 on targeted external hiring and coworker promotions



Notes: Coefficient plots. Each row corresponds to the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of external hiring (**Panel A**) and coworker promotions (**Panel B**) in 2012-2017, between the new ERA and NRA, decomposed by age- and gender-based demographic groups (displayed in rows). The points represent the mean estimated coefficients δ_τ in Equation 2 over 2012-2017, and the bars represent 95% confidence intervals. Standard errors are clustered at the establishment level.

be driven by better substitutability of middle-aged workers with older focal workers, suggesting that the relevant margin of adjustment is movement within the core workforce rather than at the point of entry into the firm. Gender-specific estimates indicate that substitution occurs against both women (0.25) and men (0.38), with slightly stronger crowd-out among men. See Figure D.6 for the dynamic effects of hiring by gender and age groups.

Hence, the crowd-out is strongest for middle-aged hires overall, and somewhat more pronounced for men than for women. These patterns suggest that the relevant margin of adjustment is among workers who are the closest substitutes for the retained older women—middle-aged workers—rather than those at the bottom of the age distribution.

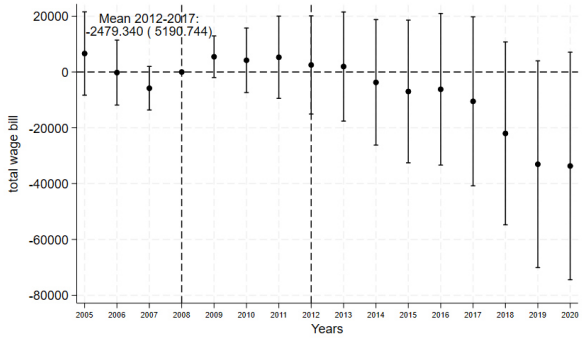
Panel B in Figure 3 reveals that crowd-out in promotions is concentrated among middle-aged and female coworkers, with no responses for young and male coworkers. Combined with the hiring results, these findings point to compressed career ladders in the middle of the age distribution rather than at entry.

Null effects on wage bills and probability of establishment closure. As a next step, I assess whether the aging workforce enlarged by the reform raises overall labor costs. Panel A of Figure 4 plots the effect of one additional treated focal worker on total wage bills at the establishment level. The aggregated effect over 2012–2017 is

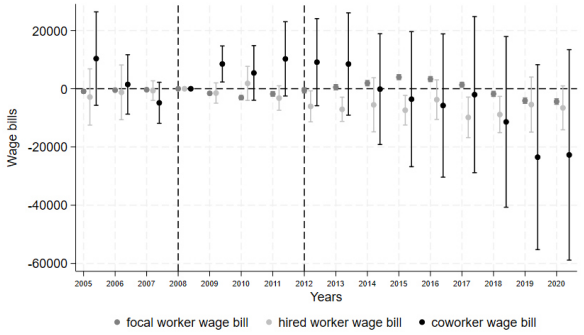
slightly negative: -2,479, indicating that firms do not experience higher wage bills despite retaining more older workers. To investigate how firms maintain low wage bills, Panel B of Figure 4 decomposes wage bills by worker type. Wage bills for focal workers increase by about 1,841, while wage bills of external hires decline sufficiently to more than offset this increase. Overall, the rise in focal wage bills is absorbed by lower wage bills of coworkers and external hires. This pattern is consistent with firms re-optimizing the composition of their workforce rather than expanding total employment. Overall, firms adjust primarily along the composition margin: they retain and pay more older focal workers, but economize on other labor inputs so that total wage costs do not rise.

Figure 4: The effect of an additional treated focal worker employed in 2008 on wage bills: total and by worker type

Panel A: Total wage bills



Panel B: Wage bills by worker types



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the total establishment wage bills in each year. **Panel A** displays the total wage bills, **Panel B** decomposes the total wage bill into the focal worker, coworker, and hired worker wage bills. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Turning to firm closure, I estimate the impact of the reform on the probability of an establishment closure on Figure D.4. The corresponding event-study coefficients are statistically insignificant. This suggests that, if anything, having more older workers whose retirements are delayed does not increase the likelihood that firms exit the market.

4.3 Downstream Effects

Finally, I examine whether treated and less-treated firms converge when the affected cohorts pass the new ERA. The event-study coefficients become small and statistically

insignificant after 2017 for most outcomes in Figure 2, suggesting that firms gradually return to similar trajectories when the main reform window closes. However, downstream effects should be interpreted with caution, because the abolishment of the women's early retirement pathway also raised the normal retirement age from 65 to 65.5, so part of the adjustment beyond 2017 reflects changes at the normal retirement margin.

In sum, the German reform that raised the early retirement age for women leads to (i) higher retention and slightly higher promotion rates for older focal workers, (ii) unchanged or slightly lower total wage bills due to offsetting declines in coworkers' and external hires' wage bills, and (iii) sizeable crowd-out of coworker promotions and external hiring, particularly among middle-aged workers. One additional treated focal worker crowds out roughly 0.63 external hires and 0.46 coworker promotions per establishment during the main reform period. These magnitudes are close to, although somewhat smaller than, those found in the Netherlands by Hut (2024), consistent with the idea that the more strongly pre-announced German reform allowed firms more time to plan their adjustment.

A natural follow-up question is whether the crowding-out promotion effects on middle-aged women cascade to younger workers. Though this is difficult to test empirically, the downward trend in the coworker wage bills in Panel B in Figure 4 and the limited separation pattern from the firm in Panel B of Figure D.5 imply pronounced and persistent negative effects that increase with retirement delay.

4.4 Robustness and Falsification Checks

Below, I conduct a set of robustness and sensitivity analyses that assess the stability of the baseline results to alternative modeling choices. Details on the corresponding sample construction and resulting sample sizes are provided in Appendix B.

Falsification tests: placebo birth cutoff and gender. A concern is that contemporaneous macroeconomic shocks, such as the 2008–2009 Great Recession, could differentially affect firms with more treated workers. This is unlikely, as such shocks should not correlate with the share of women born around the 1952 cutoff, and industry $\times$ year fixed effects absorb sector-specific business-cycle dynamics.

Nevertheless, to assess whether the estimated spillovers reflect the reform rather than coincident shocks, I conduct placebo tests that redefine the treatment along dimensions unaffected by the reform. Panel A of Figure D.8 uses placebo birth cohorts (1952–1955), assigning a false cutoff at 1954; Panel B redefines focal workers as men born between 1950 and 1953. In both cases, I find no evidence of intra-firm spillovers. These null results

support the interpretation that the baseline effects are driven by the retirement reform rather than by macroeconomic conditions or unrelated trends.

Sensitivity to the choice of the estimation bandwidth. The baseline specification uses a two-year bandwidth around the reform cutoff. In Figure D.9, I re-estimate the main results using a narrower one-year bandwidth. Focusing on cohorts born in 1951–1952 increases the share of firms with a single focal worker, effectively reducing the design to a difference-in-discontinuities framework with a binary treatment. The estimates remain qualitatively similar, though less precise, indicating that the results are robust to the choice of bandwidth and treatment definition.

Pre-reform behavior of focal workers. One of the identification concerns is that firms employing focal workers in 2008 may have non-random treatment intensity, potentially biasing the baseline estimates in the main reform period. To assess this, I analyze focal workers directly—independent of firm sampling—and test whether treated workers (born after January 1952) exhibit differential employment, hiring, or separation behavior prior to retirement ages.

Regressions of employment outcomes on treatment status show no differences until the mid-50s (Panel A of Figure D.2). The number of days worked (conditional on employment) is unaffected (Panel B of Figure D.2), indicating that responses operate on the extensive margin. Panels C and D of Figure D.2 show no differential hiring or separation by treatment status before the pre-reform ERA. Overall, there is no evidence of anticipatory worker or firm adjustments before age 55, consistent with prior findings (Badalyan, 2025; Geyer and Welteke, 2021). This supports the identifying assumption that sampling firms in 2008—before any treated worker reaches pensionable age—is plausible.

5 Elasticity of Substitution

The baseline results show that extending the employment of older women induces sizable within-firm reallocations: establishments retain more older focal workers and reduce both promotions and external hiring of younger employees. These average effects, however, mask substantial heterogeneity arising from differences in firms' internal structures and their access to an external labor supply. Understanding these heterogeneities is essential for interpreting whether the observed crowd-out reflects technological substitutability, turnover frictions, or constraints imposed by internal career ladders.

To organize this analysis, I distinguish between two complementary dimensions of adjustment capacity: the thickness of the internal labor market (ILMT) and the thickness of the

external labor market (ELMT). ILMT captures the extent to which coworkers within a firm represent viable substitutes for older incumbents on internal ladders, whereas ELMT captures the availability of suitable replacement candidates in the broader labor market. Variation along these dimensions provides a lens through which crowding-out of promotions and hiring can be mapped into economic parameters of interest—most notably, the elasticity of substitution between older and younger workers.

5.1 Definitions: Internal and External Labor Market Structures

Internal labor market thickness (ILMT). Internal labor market thickness is defined as the establishment-level concentration of employment in its largest occupation, following Ginja, Karimi, and Xiao (2023) and Cortes and Salvatori (2019). For establishment j in year t,

$$s_{jt} = \frac{N_{jt}^{\text{largest occupation}}}{N_{jt}}, \tag{3}$$

where N_{jt} denotes the establishment's total employment. An establishment is classified as having a "thick" internal labor market when this share exceeds the sample median (slightly above 0.5), meaning that a large share of workers occupy the same occupational ladder. Such settings are expected to exhibit strong internal competition and greater sensitivity of promotions to delayed retirements.

External labor market thickness (ELMT). To capture hiring frictions arising from local labor supply, I compute an index of external labor market thickness for each commuting zone. Using the full population of social security records, I define 141 commuting zones based on mobility patterns, following Kosfeld and Werner (2012).[30] For industry k in zone c at time t, external market thickness is measured as:

$$\theta_{kct} = \frac{N_{kct}/N_{ct}}{N_{kt}/N_t}, \tag{4}$$

where N_{kct} is local employment in industry k, N_{ct} is total local employment, and N_{kt} and N_t are the corresponding national values. Values above one indicate that an industry is locally overrepresented relative to the national distribution, implying a thicker external market and lower turnover frictions. Thin external markets, by contrast, constrain a

[30] I choose the finer 141-zone classification, rather than broader regional aggregates as in Jäger and Heining (2022), because women are typically less mobile across regions (Meekes and Hassink, 2022), making finer spatial units more relevant in this context.

firm's ability to replace or expand its workforce and are therefore expected to amplify hiring crowd-out. Figure Figure D.10 illustrates these patterns for motor vehicles and hospital activities.[31]

5.2 A Slot-Constraint Model with Human Capital and Turnover Frictions

Setup. A firm fills a fixed number of job slots in each period. For any given slot, the firm may (i) retain an incumbent older worker, (ii) promote an internal junior coworker, or (iii) hire externally. The indicator variables for these choices are denoted by $r, p, h \in [0, 1]$ with

$$r + p + h = 1 \tag{5}$$

The firm maximizes current net output from filling the slot:

$$\Pi(r, p, h) = r f_d + p f_p(\sigma_p) + h f_h(\sigma_h) - C(p, h; \tau) \tag{6}$$

where f_d is the output from retaining an older incumbent, $f_p(\sigma_p)$ the output from promoting a younger coworker (with σ_p measuring how close a promoted worker is to the productivity of an older incumbent), and $f_h(\sigma_h)$ the output from hiring externally (with σ_h measuring substitutability of a new hire).

Turnover frictions enter through the adjustment cost function $C(p, h; \tau)$, with

$$\frac{\partial C}{\partial p} > 0, \qquad \frac{\partial C}{\partial h} > 0, \qquad \frac{\partial C}{\partial \tau} < 0 \tag{7}$$

so that thicker external labor markets (larger τ, corresponding to thicker ELMT) reduce the cost of replacing a worker.

Solution. Because (5) implies $h = 1 - r - p$, the problem can be rewritten in (r, p) only. The first-order conditions for an interior solution are

$$f_d - f_h(\sigma_h) - C_r + C_h = 0 \tag{8}$$

[31] I thank Niklas Vetterer for help getting started creating these maps.

$$f_p(\sigma_p) - f_h(\sigma_h) - C_p + C_h = 0 \tag{9}$$

These conditions determine the firm's allocation (r, p, h) as functions of the primitives (f_d, f_p, f_h) and of turnover frictions τ.

Comparative statics: effects of a higher retention incentive. The reform increases the value of retaining older workers, f_d. dr denotes the induced marginal change in retention. Differentiating (8)–(9) gives the responses of promotion and hiring:

$$\frac{dp}{dr} = \psi(\sigma_p, \sigma_h, \tau), \qquad \frac{dh}{dr} = -\left(1 + \frac{dp}{dr}\right) \tag{10}$$

where $\psi(\cdot)$ is decreasing in external labor market thickness τ (i.e., increasing in external frictions).:

$$\frac{\partial}{\partial \tau}\left(\frac{dp}{dr}\right) < 0, \qquad \frac{\partial}{\partial \tau}\left(\frac{dh}{dr}\right) > 0 \tag{11}$$

Thus, in thin external labor markets (low τ), external adjustment is costly (i.e., hiring is a less attractive margin), and the firm relies more on internal adjustment, while reducing hiring more strongly because external hiring is a costly margin.

Observed crowd-out and its decomposition. Empirically, the observed effect of one additional retained older worker on younger-worker outcomes is

$$\Delta Y_{\text{obs}} = \sigma_p \frac{dp}{dr} + \sigma_h \frac{dh}{dr} \tag{12}$$

Substituting (10) yields

$$\Delta Y_{\text{obs}} = -\sigma_h + (\sigma_p - \sigma_h)\frac{dp}{dr} \tag{13}$$

Equation (13) highlights two determinants of observed spillovers:

(i) *Relative substitutability*: if promoted workers are closer substitutes than external hires ($\sigma_p > \sigma_h$), the promotion channel amplifies crowd-out; the opposite holds when $\sigma_p < \sigma_h$.

(ii) *Turnover frictions*: in thin markets (low τ), hiring is a costly adjustment margin, so firms reduce hiring more strongly when retention increases (i.e., dh/dr is more negative),

while in thick markets hiring adjusts more smoothly.

Implications for interpreting elasticities of substitution. A naïve elasticity-of-substitution estimate maps ΔY_{obs} directly into a structural substitution parameter. Equation (13) shows that such an interpretation is biased whenever firms face frictions (τ finite) or when promotion and hiring substitute imperfectly for each other.

Underestimation. If external hires are the closer substitutes (σ_h large), but hiring is difficult (low τ), firms reduce hiring strongly (i.e., dh/dr is more negative). However, because adjustment is distorted by frictions, the observed response reflects both substitutability and constraints, which may lead to underestimation of the underlying technological substitutability.

Overestimation. If promoted juniors are the closer substitutes (large σ_p) and the firm strongly reallocates internally when external hiring is constrained, the promotion response may exaggerate crowd-out relative to the frictionless benchmark, overstating substitutability.

In summary, observed spillovers equal a combination of technological substitutability (σ_p, σ_h) and turnover frictions (τ). The empirical heterogeneity by ILMT and ELMT maps directly onto these mechanisms: thick ILMT affects σ_p (internal substitution), while thin ELMT raises adjustment costs $C(\cdot;\tau)$ and shifts the firm toward internal responses.

Mapping to empirical estimates. The empirical specification estimates the effect of an additional retained older worker on promotions and hiring at the establishment level. In the model, these correspond to $\frac{dp}{dr}$ and $\frac{dh}{dr}$, respectively. The heterogeneity analysis by internal and external labor market thickness maps to variations in (σ_p, σ_h) and τ: thicker internal labor markets increase the scope for internal substitution (σ_p), while thinner external labor markets increase turnover frictions (low τ) and shift adjustment toward promotions. The empirical estimates can therefore be interpreted as reduced-form counterparts of these structural relationships, capturing how adjustment is distributed across margins.

Link to empirical specification. The model is written at the level of a single job slot, while the empirical analysis is conducted at the establishment level. To connect the two, I interpret establishments as consisting of multiple slots subject to similar decision problems. Under the assumption that slot-level decisions are additively separable and that the reform affects the marginal value of retention f_d uniformly across slots, aggregate firm-level responses can be approximated as the sum of slot-level adjustments. In this case, the empirical coefficients can be interpreted as averages of $\frac{dp}{dr}$ and $\frac{dh}{dr}$ across slots within the establishment. This interpretation highlights that the model is intended as

a conceptual framework to guide the empirical analysis rather than as a fully structural estimation.

5.3 Results

Below, I show the heterogeneous effects of the reform by internal and external labor market structures. The heterogeneity results should be interpreted with caution. While several patterns are consistent with the proposed mechanisms, differences across subgroups are not always statistically distinguishable, and the evidence should therefore be viewed as suggestive rather than definitive.

Focal worker retentions. Before analyzing the mechanisms behind crowd-out effects, it is important to understand how internal and external labor market thickness shapes the retention decisions of focal workers. Retention appears to vary systematically with internal labor structure. Panel A in Figure 5 shows that older workers tend to be retained more often when the ILMT is thin (0.175), where few internal substitutes exist, and the loss of experienced workers would be particularly costly. Estimated retention is somewhat lower when the ILMT is thick (0.147), meaning that internal candidates are more abundant. By contrast, the thickness of the external labor market does not appear to strongly affect retention: the effects are similar in magnitude in thin (0.153) and thick (0.167) ELMTs (Panel B).

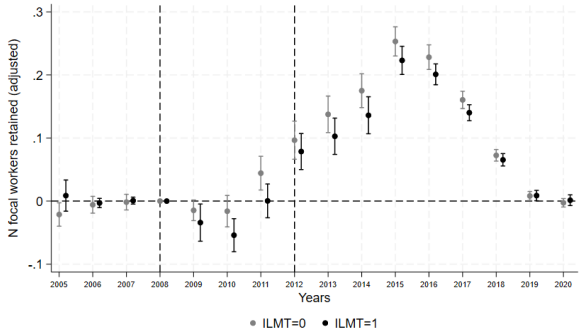
These results are closely aligned with Badalyan (2025), which, using individual-level regressions, finds that internal substitutability appears to strongly shape labor supply responses to this reform, while industry-based ELMT generates relatively little heterogeneity. That study also documents heterogeneity by occupation-based ELMT, showing that older workers in occupations with scarce external substitutes are more likely to remain employed.

Together with the firm-level evidence here, the pattern is consistent with the interpretation that retention decisions are driven primarily by firm-specific human capital and internal knowledge complementarities, rather than by external hiring frictions for given industries. Such specific skills and low substitutability may help explain the higher retention and lower hiring rates for older workers documented in the literature (Hutchens, 1986).

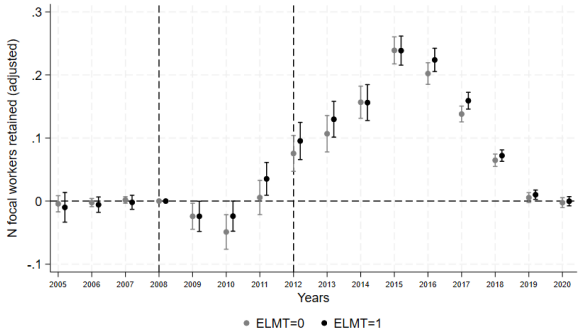
Hiring. The crowd-out of external hiring appears to vary with labor market thickness. In thin external markets, where firms face severe turnover frictions, hiring declines more strongly (–0.174). In thick external markets, the hiring response is smaller in magni-

Figure 5: The effect of an additional treated focal worker employed in 2008 on retentions by internal and external labor market thicknesses

Panel A: Retentions by ILMT



Panel B: Retentions by ELMT



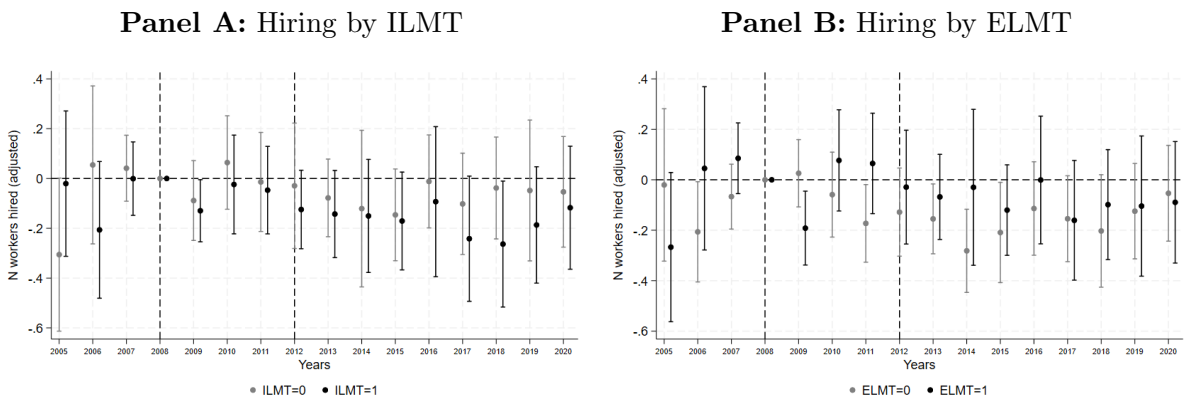
Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of retentions of focal workers. **Panel A** represents subsample analysis by internal labor market thickness- the share of the largest employment occupation in the establishment in the total workforce. **Panel B** represents subsample analysis by external labor market thickness (ELMT). The ELMT is categorized into two groups based on the commuting zone being at least as concentrated as the country-level concentration ($ELMT > 1$). The points represent the estimated coefficients δ_τ in Equation 2 and the bars represent 95% confidence intervals. Standard errors are clustered at the establishment level.

tude (–0.068). Because retention rates are similar across ELMT groups, these differences suggest that failing to account for external frictions may lead to overstating the substitutability between older workers and external hires. What may be interpreted as "strong crowd-out" could, in thin markets, partly reflect limited hiring possibilities rather than purely technological substitution.

Internal labor market structure also appears to shape hiring responses, although in a different direction: hiring declines more strongly when ILMTs are thick (–0.923) and less so when ILMT is thin (–0.081). When many workers share the same occupation, firms may adjust internally by reallocating career progression rather than by expanding the workforce. This is consistent with larger hiring reductions relative to retention effects in thick ILMTs, suggesting an apparent substitution that may partly reflect internal hierarchy constraints rather than purely technological substitution.

Promotions. Promotion responses provide additional evidence that internal bottlenecks may mediate the impact of delayed retirement. In high-ILMT establishments, coworker promotions decline more strongly (–0.134)—substantially more than in thin ILMTs (–0.026). Because focal-worker retention is similar across ILMT categories, this gap is consistent with tighter congestion along internal career ladders in thick markets. By contrast, promotion effects are more similar across ELMTs (–0.106 in thin vs. –0.057 in thick), suggesting that promotion crowd-out is more closely related to internal rank

Figure 6: The effect of an additional treated focal worker employed in 2008 on hiring by internal and external labor market thicknesses



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of hired workers. The points represent the estimated coefficients δ_τ in Equation 2 and the bars represent 95% confidence intervals. **Panel A** represents subsample analysis by internal labor market thickness- the share of the largest employment occupation in the establishment in the total workforce, and **Panel B** represents subsample analysis by external labor market thickness (ELMT). The ELMT is categorized into two groups based on the commuting zone being at least as concentrated as the country-level concentration ($ELMT > 1$). Standard errors are clustered at the establishment level.

constraints than to external hiring frictions.

These patterns are consistent with Bertheau (2021), who shows that a large share of firms fill vacancies internally, reflecting imperfect substitutability between internal and external candidates. Such imperfect substitution implies that coworker promotions may be more sensitive to delayed retirements than external hiring. Taken together, these results suggest that internal coworkers may be closer substitutes for older workers and therefore may experience larger crowd-out effects than external hires, although these differences should be interpreted with caution.

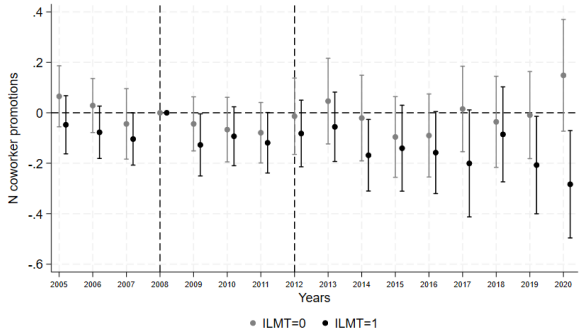
Industry tradability. To provide additional suggestive evidence on the role of turnover frictions, I analyze whether firms in more tradable industries exhibit different retention patterns for focal workers. Because production can be relocated across borders, tradable industries may offer greater scope for worker substitution through outsourcing than non-tradable sectors (Drenik et al., 2023). I classify industries by tradability following Gregory, Salomons, and Zierahn (2022).[32]

Figure D.11 shows that establishments in non-tradable industries tend to exhibit larger increases in the retention of older workers, which is consistent with their more limited

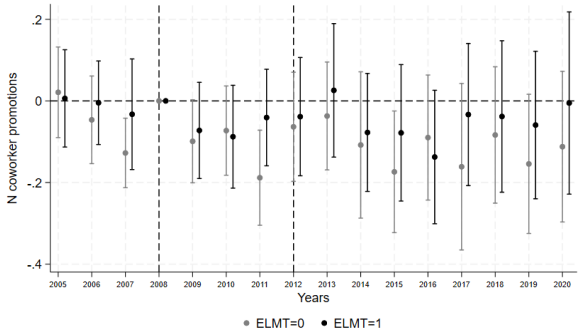
[32] *Tradable industries* are: Mining (WZ08: B); Manufacturing (WZ08: C); Electricity, water supply (WZ08: D, E); Transport, storage (WZ08: H); Financial services (WZ08: K); Real estate (WZ08: L); Agriculture (WZ08: A); Information and communication (WZ08: J); Scientific and technical services (WZ08: M). I thank Duncan Roth for the help with the data.

Figure 7: The effect of an additional treated focal worker employed in 2008 on coworker promotions by internal and external labor market thicknesses

Panel A: Promotions by ILMT



Panel B: Promotions by ELMT



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of coworker promotions. The points represent the estimated coefficients δ_τ in Equation 2 and the bars represent 95% confidence intervals. **Panel A** represents subsample analysis by internal labor market thickness- the share of the largest employment occupation in the establishment in the total workforce, and **Panel B** represents subsample analysis by external labor market thickness (ELMT). The ELMT is categorized into two groups based on the commuting zone being at least as concentrated as the country-level concentration ($ELMT > 1$). Standard errors are clustered at the establishment level.

exposure to external competitive pressures.[33] By contrast, the crowd-out of younger workers' promotions and hiring appears more pronounced in tradable industries. These patterns are in line with earlier evidence suggesting that external market conditions shape the adjustment margin: tradable industries may rely more on external hiring and therefore exhibit stronger displacement when retention rises.

These differences across industries should be interpreted with caution, as confidence intervals overlap and the estimates are imprecise. Nevertheless, the observed patterns suggest that firm-level averages may mask meaningful within-firm differences in adjustment. This motivates the subsequent analysis at the jobcell level, which more directly captures internal bottlenecks and the role of occupation- and task-based constraints in shaping spillovers.

Wage bills. If retention of focal workers is concentrated among workers who are less substitutable internally, this may affect firm-level wage bills, particularly in settings with thin internal labor markets. The average effects of an aging workforce may mask hetero-

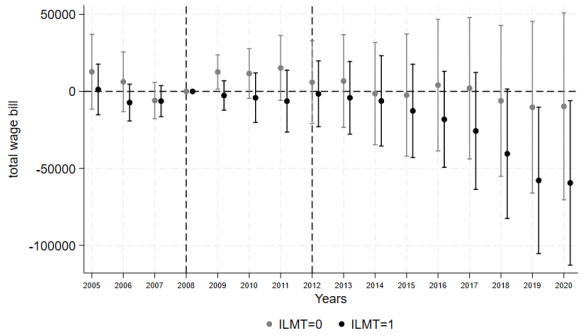
[33]The temporary decline in adjusted retention during the first post-reform years is concentrated among large establishments (see Figure D.12) and non-tradable industries. One possible explanation is that these firms possess greater scope for organizational adjustment through intensive-margin responses, such as reductions in working hours, phased retirement arrangements, or internal redeployment of older workers. As a result, the reform may initially affect labor input rather than headcount retention. Consistent with this interpretation, positive retention effects emerge more clearly in subsequent years.

geneity in how firms adjust total labor costs.

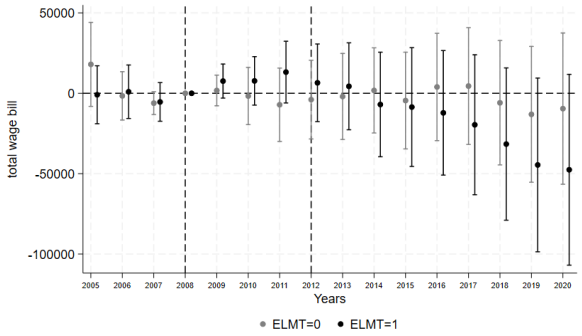
Panel A in Figure 8 shows that, in thick internal labor markets, the estimated wage bill response is negative (though not statistically significant): an additional treated focal worker is associated with a reduction in total wage bills of roughly EUR 11,415. This decline may reflect reductions in coworker wage bills documented in the previous section, consistent with compressed promotion opportunities and slower wage growth in more competitive settings.[34].

Figure 8: The effect of an additional treated focal worker employed in 2008 on wage bills by internal and external labor market thicknesses

Panel A: Wage bills by ILMT



Panel B: Wage bills by ELMT



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on wage bills. The points represent the estimated coefficients δ_τ in Equation 2 (2012-2017 pooled together) and the bars represent 95% confidence intervals. **Panel A** represents subsample analysis by internal labor market thickness- the share of the largest employment occupation in the establishment in the total workforce. **Panel B** represents subsample analysis by external labor market thickness (ELMT). The ELMT is categorized into two groups based on the commuting zone being at least as concentrated as the country-level concentration ($ELMT > 1$). Standard errors are clustered at the establishment level.

Implications. Taken together, these results suggest that observed spillovers depend on market structure—both inside and outside the firm. This pattern can be related to the conceptual framework introduced earlier, where adjustment occurs through internal promotion, external hiring, or continued retention, and where these margins are shaped by internal substitutability and external turnover frictions. Two implications emerge.

First, not accounting for external labor market thickness may lead to overstating substitutability between older workers and young hires. In thin ELMTs, firms reduce hiring more strongly than in thick ELMTs despite similar increases in retention. This stronger

[34]By contrast, according to Panel B in Figure 8, in thin internal labor markets, the wage bill effect is small and positive (EUR 2,478), which may reflect the limited pool of internal substitutes and the higher relative value of retaining experienced workers. External labor market thickness appears to play a more limited role: wage bill responses are near zero in thin external markets (EUR –35), but reach a negative value (EUR –6,062) in thick external markets, where hiring adjustments may be easier to implement, although both estimates are statistically insignificant

hiring response is consistent with the interpretation that it reflects the limited availability of external candidates rather than a purely technological ability to substitute older workers for younger ones. Estimates that treat all hiring reductions as technological substitution may therefore overstate the implied elasticity of substitution. In my data, this corresponds to an increase in the implied substitutability of roughly a factor of 2.8 when ELMT is not accounted for. This pattern is consistent with the model case in which turnover frictions suppress the hiring margin, shifting adjustment toward other channels.

Second, not accounting for internal labor market thickness may understate the crowd-out of promotions. In thick ILMTs, delayed retirements are associated with substantially lower promotion rates (–0.134), whereas the same reform is associated with much smaller effects in thin ILMTs (–0.026). Averaging across firms may therefore mask environments in which internal competition is more intense and promotion ladders are more congested. Analyses that do not condition on ILMT may therefore understate the career costs borne by workers in occupations with dense hierarchies. In terms of the earlier conceptual framework, thick ILMTs can be interpreted as settings with high internal substitutability: when older workers stay longer, promotions appear to become the primary margin of adjustment.

Overall, the evidence suggests that spillovers from raising retirement ages reflect the interaction of: (i) internal substitutability and firm-specific human capital, (ii) turnover frictions in external labor markets, and (iii) hierarchical congestion within occupations. Adjustment across margins appears interrelated: when external hiring is constrained (thin ELMT), promotions tend to absorb more of the adjustment; when internal ladders are congested (thick ILMT), external hiring tends to decline, even when the labor supply is relatively abundant. These interactions suggest that reduced-form crowd-out estimates combine technological substitution with adjustment constraints, and should be interpreted in light of both internal and external market structure.

6 Intra- and Inter-Jobcell Personnel Decisions

The evidence that spillover effects vary systematically with the thickness of internal labor markets (ILMT) suggests that firms' adjustment mechanisms operate at a finer level than the firm-level used in the sections above. If internal promotions and hiring decisions depend on the pool of available coworkers within specific job ladders, then analyzing only firm-level outcomes may conceal important heterogeneity in how delayed retirements affect coworkers. This motivates a closer examination of intra- and inter-jobcell spillovers. By zooming into establishment–occupation cells (jobcell), I can disentangle

whether crowd-out effects arise primarily within job ladders (intra-jobcell) or through broader reallocation across occupations or establishments (inter-jobcell).[35]

6.1 Baseline Inter- and Intra- Jobcell Effects

Previous literature finds conflicting results on intra-firm spillovers of an aging workforce, with most papers finding negative impacts (Bianchi et al., 2023; Ferrari, Kabátek, and Morris, 2023), while others, using slightly larger firms, find positive impacts (Carta, D'Amuri, and Von Wachter, 2024). The positive impacts on larger firms could be driven by a lack of availability of more granular data, such as occupations within establishments; therefore, such analyses could hide negative spillovers. On the other hand, larger firms may find it easier to spread work to incumbent workers and to grant promotions, due to their capacity to make more flexible internal organizational adjustments (Hensvik and Rosenqvist, 2019; Jäger and Heining, 2022), for example, due to human resource management systems (Holzer (1987) as cited in Schmutte and Skira (2023)). If internal adjustments are more muted in larger firms despite zooming into occupations, then this is evidence for their flexibility in making internal adjustments.

First, I show that the hiring responses are relatively larger in small establishments than in larger ones. Then, I zoom into the jobcells, proxied by occupations that interact within establishments. Zooming in on larger firms enables me to test whether there are negative effects hidden within workplaces that are not apparent in firm-level estimates. Such analyses help to highlight the differences in the sign of spillover effects on hired workers, even in studies that use the same reforms (Carta, D'Amuri, and Von Wachter, 2024; Bianchi et al., 2023).

Firm size heterogeneity. A firm's size is a first-order determinant of how it adjusts to delayed retirements. Larger establishments typically operate with deeper internal hierarchies, more diversified jobcells, and more flexible redeployment possibilities. These features allow them to absorb shocks through internal reallocation—via promotions or transfers across jobcells—rather than through hiring or separations. Smaller firms, by contrast, generally have thinner internal structures: occupational ladders are short, jobcells are narrow, and external hiring often plays a relatively larger role. As a result, the same increase in retention may generate different patterns of spillovers in small versus large firms. Examining heterogeneity by firm size, therefore, provides an important bridge between the baseline establishment-level results and the more granular intra-, as opposed to inter-jobcell analysis that follows. It helps to clarify whether the observed adjust-

[35]The occupations are based on 3-digit classifications, as the ILMT was constructed above.

ment margins are driven by differences in organizational depth or by within-occupation constraints operating inside jobcells.

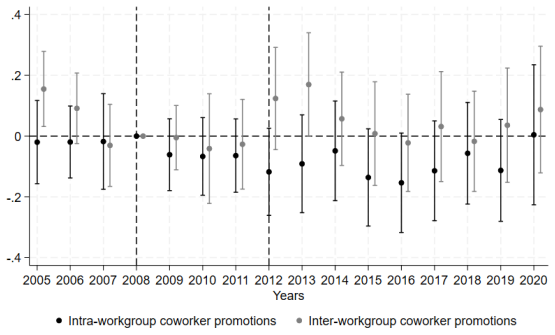
Figure D.12 shows that establishment size also moderates firms adjustments to delayed retirements. Smaller firms exhibit larger increases in the retention of older incumbents, consistent with their more limited internal substitution possibilities. By contrast, crowd-out coefficients are noisier but tend to be larger in establishments with more than 30 workers; however, the estimates for small firms lie well within the confidence intervals of large firms, suggesting no statistically significant difference in spillovers across size groups. These patterns motivate the next step of the analysis, which decomposes spillovers into intra- and inter-jobcell responses to examine more finely how firms reallocate tasks and mobility opportunities when internal substitution options vary.

Intra- opposed to inter-jobcell spillovers. I next decompose the firm-level spillovers into within- and across-jobcell adjustments. Jobcells are defined as 3-digit occupations (*Klassifikation der Berufe (KldB) 1988*) within establishments. I keep establishments that had at least two jobcells in 2008 to enable intra- opposed to inter-jobcell analyses.

Figure 9: Intra- and inter-jobcell effects

Panel A: Intra- opposed to inter hiring

Panel B: Intra- opposed to inter promotions



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on hiring (**Panel A**) and promotions (**Panel B**) in each year, decomposed by intra-jobcell (black) and inter-jobcell (gray) spillovers. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

The results reveal that crowd-out operates primarily within the focal worker's own jobcell. Panel A in Figure 9 shows that an additional treated older worker retained in 2008 reduces *intra-jobcell* hiring by approximately 0.086 workers, nearly twice the magnitude of the reduction in *inter-jobcell* hiring (0.052). Internal promotions show an even sharper

segmentation (Panel B in Figure 9): promotions decline meaningfully within the focal jobcell (by 0.109 per retained older worker), whereas promotions in other jobcells are essentially unaffected, with estimates close to zero.

The lack of spillover impacts on hiring across occupations is in line with the previous literature on worker substitutability (Brenøe et al., 2024; Huebener et al., 2024; Jäger and Heining, 2022; Schmutte and Skira, 2023). Moreover, the absence of spurious effects across occupations confirms that the intra-firm adjustments are the result of the aging workforce as opposed to establishment-specific impacts of the crisis or other reforms.

These patterns indicate that adjustment frictions are highly localized. Firms substitute most strongly among workers who share tasks, supervisors, or career ladders, foreshadowing the heterogeneities to come: the importance of internal labor market structure, firm-specific human capital, and bottleneck occupations is concentrated precisely in those segments where intra-jobcell substitution is feasible.

6.2 Mechanisms: Human Capital and Value of Old Workers

Existing evidence suggests that delayed retirements disproportionately retain older workers who are costly to replace—such as managers and workers in occupations with strong job-specific skill requirements—highlighting the importance of firm-specific human capital and turnover frictions (Badalyan, 2025). If firms optimally retain these workers because they embody valuable knowledge, relationships, or organizational capital, their continued presence may have implications beyond mechanical crowd-out effects. In particular, while slot constraints may intensify competition for promotions among workers on the same career ladder, retained older workers may simultaneously generate positive spillovers for coworkers in complementary roles by preserving firm-specific human capital that is difficult to replicate. This insight motivates an explicit analysis of inter-jobcell spillovers: distinguishing between competitive effects within narrowly defined career ladders and complementarities across occupations that rely on shared expertise, coordination, or managerial oversight.

Human capital specificity of occupations. Occupations differ by how much of a worker's productivity stems from job-specific skills. When human capital is highly occupation-specific, workers are harder to replace externally and may generate complementarities for coworkers whose tasks rely on their accumulated know-how. To measure this specificity, I follow the approach in Jäger and Heining (2022) and Bleakley and Lin (2012) and estimate occupation-level Mincer regressions for each 3-digit occupation.[36]

[36]I run these regressions on a random sample of all the workers to classify occupations, and merge

I use the occupation-specific return to experience as a measure of how strongly wages depend on on-the-job learning. Occupations with returns above the median are classified as having high human-capital specificity.

Figure D.13 displays the results. When older workers are employed in occupations with high firm-specific human capital, there are positive impacts on coworkers' promotions in intra-cell occupations. Overall, these results highlight that older workers can be particularly valuable to firms and can generate higher productivity for some coworkers.[37]

7 Conclusion

This paper exploits a large retirement reform—abolition of the female pathway to early retirement in Germany—to examine how firms adjust when a large group of older workers remains employed longer than prior cohorts. Using rich administrative data with detailed occupational information, I document how establishments respond to the extended retention of older female workers and how these adjustments cascade through internal promotion structures and external hiring pipelines.

The reform substantially raised older-worker retention when the affected cohorts reached pension-eligible ages, but firms made little systematic adjustment beforehand, suggesting that the long pre-announcement horizon muted any liquidity-driven responses. When older workers ultimately remained in their jobs longer, firms shifted their personnel decisions: internal promotions slowed, and external hiring declined, consistent with an internal substitution mechanism in which the delayed exit of older workers constrains advancement opportunities and reduces openings for new recruits. These effects were unevenly distributed. Promotion losses were concentrated in thick internal labor markets, where many workers compete for the same rungs on tightly structured ladders, whereas hiring declines were greater in thin external markets, where firms face limited replacement options.

A broader implication emerging from these findings is that the conventional view of older workers simply crowding out younger ones provides only a partial picture. Prior studies on worker exits highlight the costs of turnover—losses of tacit firm-specific human capital, the difficulty of replicating high-quality matches, and the time required for external hires to become productive insiders. My results illuminate the converse mechanism: when older

these classifications with my analysis data. I do not use tenure as an alternative measure of specificity because it is mechanically affected by retirement behavior and pension eligibility rules, making it difficult to disentangle specificity from the direct effects of the reform itself.

[37] Subsample analyses by alternative measures of human capital specificity, such as managerial status and tenure, are available upon request.

workers remain, these same forces can preserve valuable expertise and, across occupations, can benefit coworkers whose tasks complement the know-how accumulated by senior employees. This helps to reconcile seemingly contradictory findings—negative spillovers within jobcells but neutral or positive effects across jobcells—by showing that the sign of spillovers depends on whether workers compete on the same ladder or operate in complementary roles. Taken together, the evidence provides a richer account of how firms navigate workforce aging and underscores the central role of turnover frictions, match quality, and internal labor market structure in shaping the distributional effects of retirement reforms.

Future research could examine whether these within-firm adjustments propagate across firms and whether they generate general equilibrium responses in labor markets. Moreover, the limited ability to hire or promote younger workers may have consequences for productivity or service quality—for example, in health and care professions—where staffing shortages have been shown to harm patient outcomes (Friedrich and Hackmann, 2021). Finally, the mechanisms documented here—career-stage substitution, internal bottlenecks, and frictions in external hiring—are likely to be relevant in other settings where a component of the workforce is retained, and warrant further comparative study.

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Crowded Career Ladders?
Intra-Firm Spillovers of Raised Retirement Age

Sona Badalyan

June 17, 2026

Supplementary Online Appendix

Table of Contents

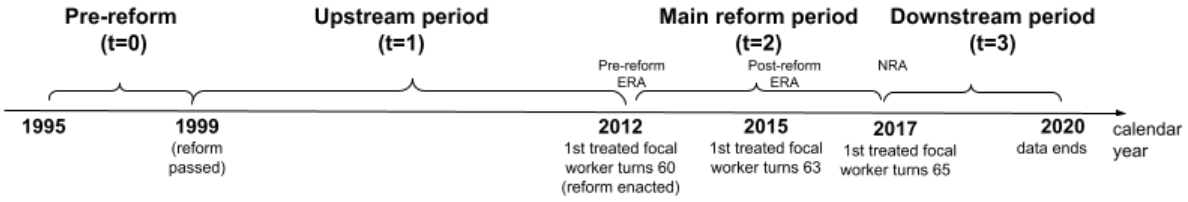
A The Public Pension System, Reform, and Identification Details

Pathways to retirement. There are several pathways to retirement in Germany, including regular, disability, long-term insurance, women's, and unemployment pathways. While the rules of some of these pathways changed or the pathways abolished altogether, the workers eligible for regular pathways to retirement were subject to a single statutory retirement age, because ERA and NRA are equivalent for them. ERA exists on pathways for more vulnerable groups, including women, the unemployed, and the long-insured workers with over 35 contribution years. More details can be found in Lorenz et al. (2018).

Birth cohorts affected by the 1999 reform. Panel A in Figure 1 shows how the retirement age increased discontinuously starting from the 1952 birth cohort. Although the reform also abolished pensions for the unemployed and persons on a progressive retirement plan (Lorenz et al., 2018), I focus primarily on the abolishing of women's pathways to early retirement because the other two categories are not recorded in the data.

Timing of the 1999 reform. The 1999 Reform (*"Rentenreformgesetz 1999"*) abolished the women's pathway to early retirement. The reform was drafted in October 1997, and the affected cohorts were announced on December 17, 1997, through publication in the Federal Law Gazette; however, according to Etgeton, Fischer, and Ye (2023), there was uncertainty regarding implementation due to the federal elections in 1998, as the reform was drafted under the old government which might not have remained in power the following year.

Figure A.1: The reform timeline



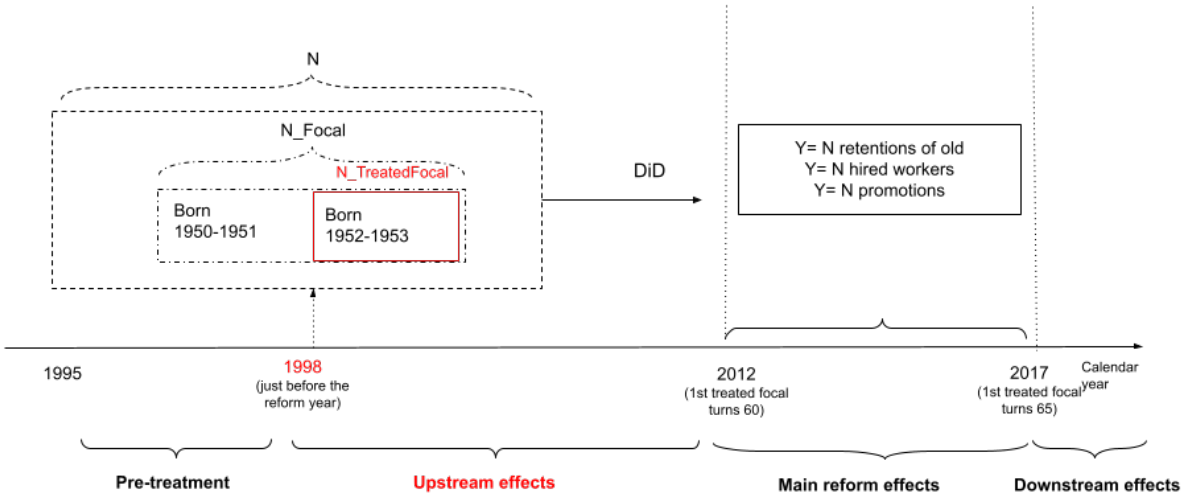
Notes: This figure represents the timeline of the reform. The upper section describes the periods, while the bottom part indicates the corresponding calendar years. For simplicity, the calendar years are written for the years of the ERA of the first affected cohort - the 1952 cohort (see Figure 1).

Illustration of empirical strategy. Below, I provide an illustration of the identification

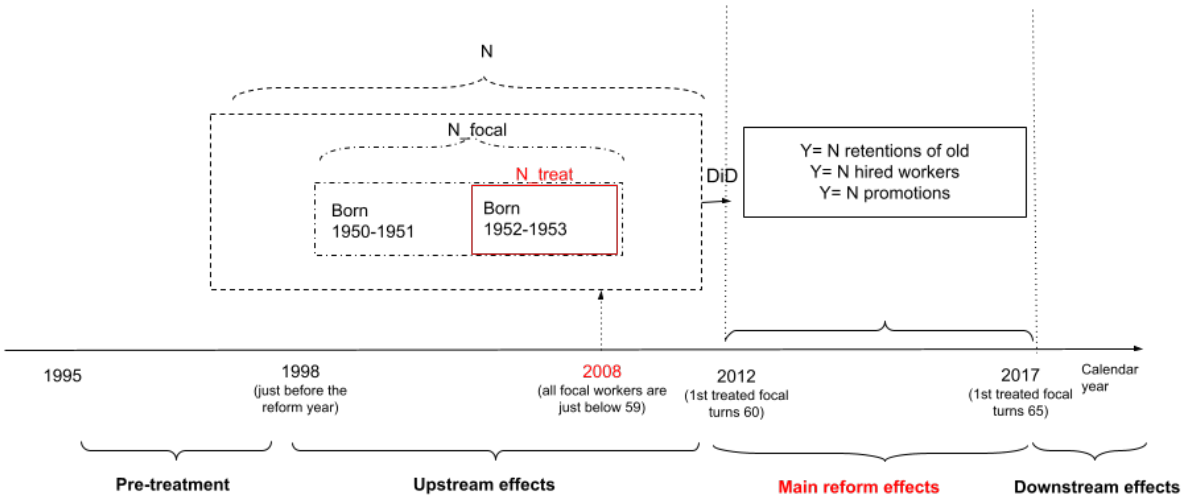
strategy in Equation 1 and Equation 2.

Figure A.2: Illustration of identification strategy: difference-in-differences

Panel A: Illustration for identification of upstream effects



Panel B: Illustration for identification of main reform effects



Notes: This graph illustrates the identification strategy for upstream effects (**Panel A**, displayed in Equation 1) and main reform effects (**Panel B**, displayed in Equation 2).

The Social Democratic Party and the Green Party coalition promised to change the established reform during the election campaign, but even two months after the elections and their victory in September 1998, there was still uncertainty about which parts of the reform would be changed (Bulmahn, 1998; Etgeton, Fischer, and Ye, 2023). In the end, the new government did not revoke the abolition of the women's pathway to early retirement. Due to this uncertainty, the news of the reform is unlikely to have changed worker or firm behavior in advance (Etgeton, Fischer, and Ye, 2023). The reform became effective on January 1, 1999. The previous literature studying this reform, such as

(Etgeton, Fischer, and Ye, 2023), uses 1998 as a pre-reform year. Given the uncertainty about implementation and use of the pre-reform period from 1998 in prior literature, I also use that year for the treatment construction to study the upstream period. Other papers (Geyer and Welteke (2021) and Badalyan (2025) among others) use the pre-reform enactment year, close to 60 years old, which I use for the main reform period.

The Figure A.1 shows the reform timeline. Because the first affected cohort (1952) was only 47 years old when the reform became effective in 1999 and would turn 60 only in 2012, there is a large upstream period.

B Additional and/or Alternative Sample Definitions

As discussed in section 3, the universe of comparable establishments is reserved for the main reform analyses, where precise measurement of firm-level spillovers, such as within and across workgroups, and subgroup analyses are essential for mechanisms. For robustness and falsification tests, I use the Sample of Integrated Employer–Employee Data (SIEED), a 1.5% random sample of German establishments with complete employment biographies of workers ever employed in them, because it focuses only on intra-firm spillovers and provides the necessary longitudinal detail while ensuring data parsimony and compliance with data security constraints.

I create four additional samples. The Samples B-D below follow all the data creation steps as in the main reform period, and alter one attribute described. Sample E follows a similar sample construction to that in the upstream period, except that I observe full biographies of focal workers regardless of the establishment at which they are employed.

Sample B: placebo birth cutoff sample. These data are sampled analogously to the main period data, with the exception that I define focal workers as those women who were fully treated, that is, born in 1952-1955 cohorts.

Table B.1: Sample sizes in baseline and alternative samples for the main reform period

	No. of establishments	No. of jobcells	No. of workers	No. of focal workers
Panel A: baseline sample				
Sample A	160,667	1,234,969	8,029,046	414,209
Panel B: alternative samples				
Sample B	2,621	19,565	127,381	7,276
Sample C	2,706	22,295	135,580	7,774
Sample D	1,637	13,655	98,722	3,290

Notes: This table describes the number of establishments, jobcells, workers, and focal workers in the baseline (**Panel A**) and alternative samples (**Panel B**). For the details on baseline and alternative sample definitions, see section 3 and Appendix B.

Sample C: placebo gender sample. These data are also sampled analogously to the main period data, but I define focal workers as males rather than females.

Sample D: alternative window (1951-1952). In this section, I sample all the firms

that had focal workers born within a 1-year bandwidth around the cutoff.

The sample sizes, including the number of coworkers, peers (by treatment), and work-groups, are recorded in Table B.1.

Sample E: Individual-level focal worker biographies. I construct a complementary sample of focal workers born within a 2-year window of the January 1952 cutoff and follow their complete employment biographies, independent of the establishments in which they were employed in 1998 or 2008. This analysis is not feasible in the IEB data used for the baseline regressions, which restrict workers to their original establishments in order to identify intra-firm spillovers. Therefore, I rely on the SIEED 1975–2018, which provides complete employment histories for a random 1.5% sample of establishments. Unlike firm-level analysis, I do not condition on continued employment in a given establishment, since the objective here is to test whether treatment status affects workers' probabilities of employment, hiring, or separation. Treatment is defined by a dummy for being born after the cutoff. The resulting sample includes 14,707 focal workers observed across 23,264 establishments in which they were employed over their working lives.

C Promotion Measure: Details and Validation

Promotions are not directly observed in the administrative data. Following the empirical literature on internal labor markets, I therefore proxy promotions using relative wage growth within establishments (Bronson and Thoursie, 2019; Ginja, Karimi, and Xiao, 2023). In particular, I define promotion as a binary variable equal to one if the relative wage growth exceeds the threshold of 10 log points.

Subsection C.1 below assesses the validity of such promotion measure by comparing it to the alternative measures (individual wage growth, relative wage growth within jobcells, 5-digit occupation-based measure), and subsection C.2 discusses alternative thresholds (5 and 15 log point) for dichotomizing the relative wage growth for binary promotion definition.

C.1 Baseline and Alternative Promotion Measures

Baseline measure: **Relative wage growth within establishments.** I classify a worker as promoted if their real wage growth exceeds the average wage growth of coworkers in the same establishment by at least 10 log points. This measure captures movements within establishments' internal wage hierarchies while netting out common wage shocks, such as collective bargaining agreements, firm-wide wage adjustments, or changes in firm performance. The measure, therefore, identifies unusually large wage increases relative to coworkers rather than general wage growth. The threshold corresponds approximately to the upper decile of the distribution of relative wage growth.

Let worker-level log wage growth be defined as

$$\Delta \ln w_{ijt} = \ln(w_{ijt}) - \ln(w_{ijt-1}), \tag{14}$$

where w_{ijt} denotes real annual earnings of worker i employed in establishment j in year t. I compute a leave-one-out measure of coworker wage growth as

$$\overline{\Delta \ln w}_{-i,jt} = \frac{\sum_{k \in j} \Delta \ln w_{kjt} - \Delta \ln w_{ijt}}{N_{jt} - 1}, \tag{15}$$

where N_{jt} denotes the number of workers in establishment j in year t. The promotion indicator is then defined as

$$\text{Promotion}_{ijt} = \mathbb{1}\left(\Delta \ln w_{ijt} - \overline{\Delta \ln w}_{-i,jt} \geq 0.10\right) \times \mathbb{1}\left(\Delta \ln w_{ijt} > 0\right). \tag{16}$$

Thus, a worker is classified as promoted if their wage growth exceeds the average wage growth of coworkers in the same establishment by at least 10 log points and their own wage growth is positive. The use of logarithms is motivated by the fact that promotions are typically reflected in proportional rather than absolute wage increases. Log differences approximate percentage wage growth and are invariant to the level of wages. As a result, the promotion measure captures workers whose wage growth substantially exceeds that of their coworkers in percentage terms rather than in absolute monetary amounts.

To assess the validity of this proxy, I compare it to three alternative promotion measures.

Alternative measure 1: **Individual wage growth.** First, I construct a promotion measure based on individual wage growth, following the approach commonly used in the literature on intra-firm spillovers (Bianchi et al., 2023; Ferrari, Kabátek, and Morris, 2023). A worker is classified as promoted if annual earnings increase by at least 10% relative to the previous year and the increase persists over a two-year window. Formally, I define

$$\text{WageGrowth}_{ijt} = \frac{w_{ijt} - w_{ijt-1}}{w_{ijt-1}}, \tag{17}$$

$$\text{WageGrowth2}_{ijt} = \frac{w_{ijt+1} - w_{ijt-1}}{w_{ijt-1}}. \tag{18}$$

The promotion indicator is then defined as

$$\text{Promotion}^{\text{ind}}_{ijt} = \mathbb{1}\left(\text{WageGrowth}_{ijt} \geq 0.10\right) \times \mathbb{1}\left(\text{WageGrowth2}_{ijt} \geq 0.10\right). \tag{19}$$

The measure is computed only for workers who remain employed in the same establishment and retain the same part-time or full-time status. Unlike the baseline measure, this definition is based solely on individual wage growth and does not account for wage changes experienced by coworkers within the establishment.

Alternative measure 2: **Relative wage growth within jobcells.** Second, I construct an occupation-relative measure that compares a worker's wage growth only to coworkers in the same occupation (three-digit occupation code) and establishment. Specifically, a promotion occurs if

$$\text{Promotion}_{icjt} = \mathbb{1}\left(\Delta \ln w_{icjt} - \overline{\Delta \ln w}_{-i,cjt} \geq 0.10\right) \times \mathbb{1}\left(\Delta \ln w_{icjt} > 0\right). \tag{20}$$

where $\overline{\Delta w}_{-i,jct}$ denotes average wage growth among coworkers in the same occupation-establishment cell, i.e., jobcell c.

Alternative measure 3: **Occupational skill upgrading.** Third, I construct a promotion measure based on upward transitions in occupational skill levels using the German Classification of Occupations. Specifically, I identify workers who remain employed in the same establishment and within the same 3-digit occupation group but move to a higher occupational skill level (5^{th} digit) between consecutive years. By restricting attention to workers who remain within broad occupational categories, this measure captures upward career progression within occupations rather than occupational mobility across occupations. Unlike the wage-based measures, occupational skill upgrading reflects changes in formally classified job requirements and therefore identifies a narrower subset of promotion events. Moreover, there might be measurement error in the social security data, as the establishments may not always report the skill upgrades to the social security administration.

Table C.1: Agreement Between Alternative Promotion Measures

Correlation	$E(\text{base}\|alt=1)$	$E(\text{base}\|alt=0)$	Reg.Coef	R^2
Panel A: alternative measure 1: Individual wage growth				
0.714	0.795	0.042	0.753***	0.510
Panel B: alternative measure 2: Relative wage growth within jobcells				
0.724	0.672	0.020	0.651***	0.524
Panel C: alternative measure 3: 5-digit occupation-based measure				
0.014	0.213	0.123	0.090***	0.000

Notes: The baseline promotion measure classifies workers as promoted if their wage growth exceeds the average wage growth of coworkers in the same establishment by at least 10 log points. The table compares the baseline measure to three alternative promotion definitions. Column (1) reports pairwise correlations. Columns (2) and (3) report the mean of the baseline promotion indicator conditional on the alternative measure equaling one and zero, respectively. Columns (4) and (5) report coefficients and R^2 values from bivariate regressions of the baseline measure on the corresponding alternative measure.

The results are reported in Table C.1. The baseline measure is strongly correlated with a standard promotion proxy based on individual wage growth exceeding 10% ($\rho = 0.714$) and with an occupation-relative measure that compares workers only to coworkers in the same occupation and establishment ($\rho = 0.724$). Workers classified as promoted by these alternative measures are substantially more likely to be classified as promoted by

the baseline measure. Specifically, 79.5% of workers identified by the individual wage-growth measure and 67.2% of workers identified by the occupation-relative measure are also classified as promoted by the baseline measure, compared to only 4.2 and 2.0%, respectively, among workers not classified as promoted by the corresponding alternative measure.

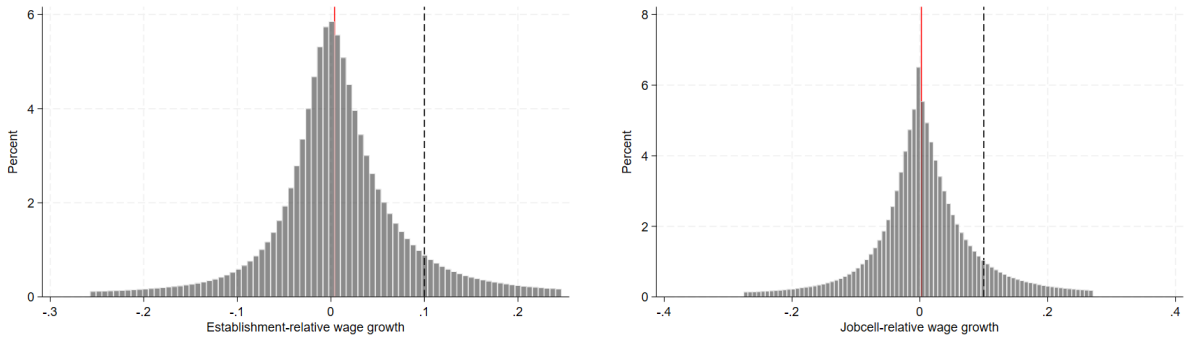
By contrast, occupational skill upgrading exhibits substantially weaker agreement with the wage-based measures. This is expected because occupational transitions capture only major changes in formally classified job requirements, whereas many promotions occur within occupations through increased responsibilities, supervisory tasks, or advancement along internal wage ladders. Consistent with this interpretation, occupational upgrading is rare (0.28% of observations) and therefore captures only a narrow subset of promotion events.

C.2 Baseline and Alternative Promotion Thresholds

Figure C.1: Distribution of Relative Wage Growth Within Establishments and Jobcells

Panel A: Distribution of establishment-relative wage growth

Panel B: Distribution of occupation-relative wage growth



Notes: **Panel A** shows the distribution of establishment-relative wage growth, defined as an employee's wage growth minus the average wage growth of coworkers in the same establishment. **Panel B** shows the distribution of occupation-relative wage growth, defined as an employee's wage growth minus the average wage growth of coworkers in the same jobcell (3-digit occupation within establishment). In both panels, observations below the 5th percentile and above the 95th percentile of the respective distribution are excluded for visual clarity. The vertical dashed line indicates the 10-log promotion threshold used to define promotions in the main analysis. The solid red line indicates the median value of the respective distribution.

Distribution of Relative Wage Growth within Establishments and Jobcells. The distributions shown in Figure C.1 support the choice of a 10 log-point threshold.

Relative wage growth is centered close to zero, with median values near zero in both specifications.

Table C.2 shows that the promotion threshold lies approximately in the upper decile of the distribution of relative wage growth within establishments, indicating that promotions correspond to unusually large wage increases relative to coworkers rather than routine wage adjustments. The threshold, therefore, identifies workers experiencing substantial upward movements within establishments' wage hierarchies while avoiding classification of common wage changes as promotions.

Table C.2: Position of Promotion Threshold in the Relative Wage-Growth Distribution

Percentile	Relative wage growth
50th	0.004
75th	0.046
90th	0.131
95th	0.247
Promotion threshold	0.100

Notes: The table reports selected percentiles of establishment-relative wage growth, defined as a worker's wage growth minus the average wage growth of coworkers in the same establishment.

Alternative thresholds. The results are robust to alternative promotion thresholds. Lowering the threshold from 10 to 5 log points increases the promotion rate from 29.1 to 36.6%, while raising it to 15 log points reduces the promotion rate to 25.8%. Despite these changes, the alternative definitions remain highly correlated with the baseline measure, suggesting that the results are not driven by the specific threshold chosen.

Table C.3: Sensitivity to Alternative Promotion Thresholds

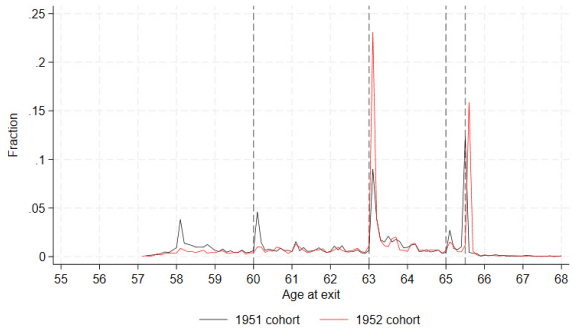
Threshold	Promotion rate (%)	Correlation with baseline
Panel A: baseline threshold (10 log points)		
	29.10	1.000
Panel B: alternative threshold (5 log points)		
	36.62	0.843
Panel C: alternative threshold (15 log points)		
15 log points	25.78	0.920

Notes: The baseline specification classifies workers as promoted when establishment-relative wage growth exceeds 10 log points. Correlations are computed relative to the baseline definition.

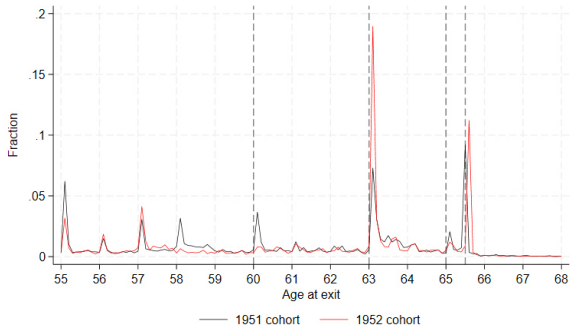
D Appendix Figures

Figure D.1: Retirement age distribution by birth cohorts

Panel A: Sample of workers employed at the age of 58



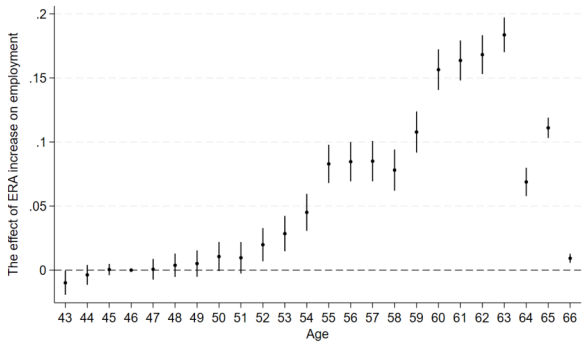
Panel B: Sample of workers employed in 1998



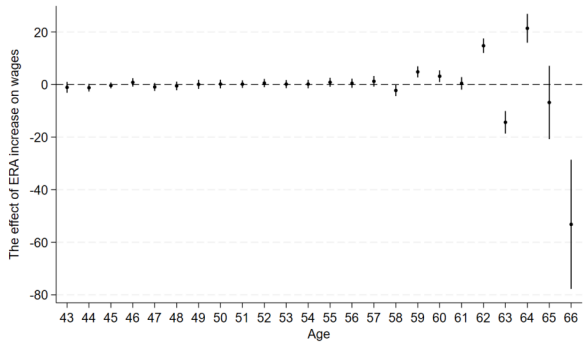
Notes: These graphs show the distribution of retirement ages. **Panel A** shows retirement ages based on focal workers employed at the age of 58. **Panel B** displays the distribution of retirement ages for workers employed in 1998. Both graphs are generated from the 2% random sample of IEB records.

Figure D.2: Direct effects of the rise in ERA on employment, probability to become a new hire, or to separate from an employer

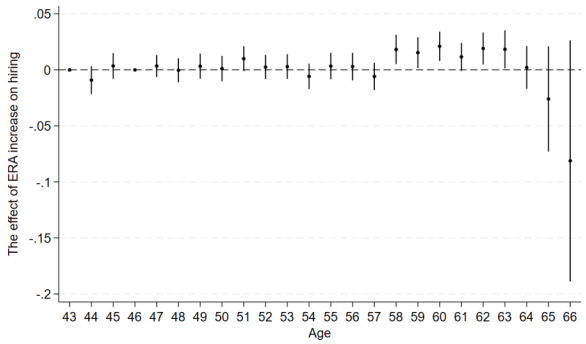
Panel A: Employment dummy



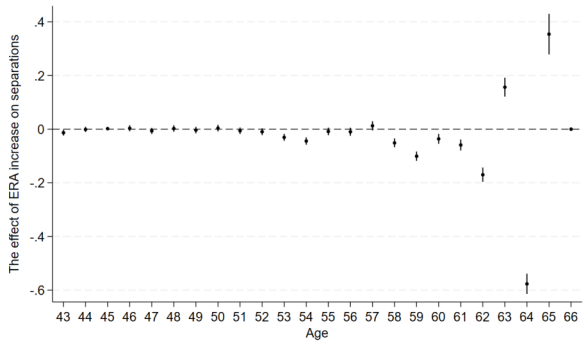
Panel B: No. of days employed at a given age



Panel C: Dummy for being a new hire



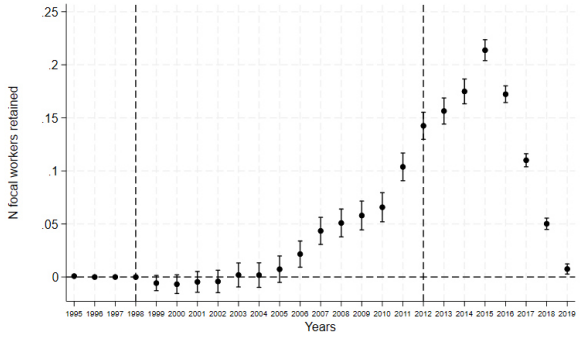
Panel D: Dummy for being separated



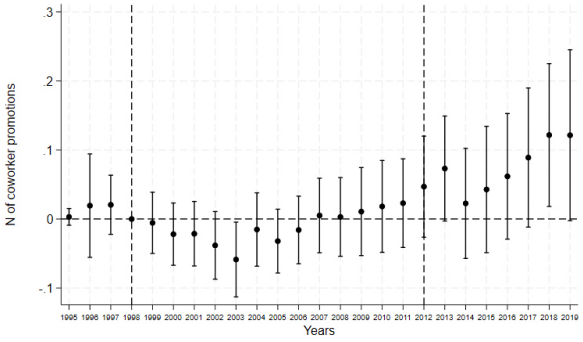
Notes: Coefficient plots. These graphs represent simple regressions of outcome variables on being born a year after the cutoff, separately for each age cohort. **Panel A**: employment dummy, **Panel B**: number of days employed at a given age, **Panel C**: dummy for being a new hire, **Panel D**: dummy for being separated. For sample construction details, see Appendix B.

Figure D.3: The effect of an additional treated focal worker employed in 1998 on the probability of a firm closure, number of focal worker retentions, coworker promotions, and external hires

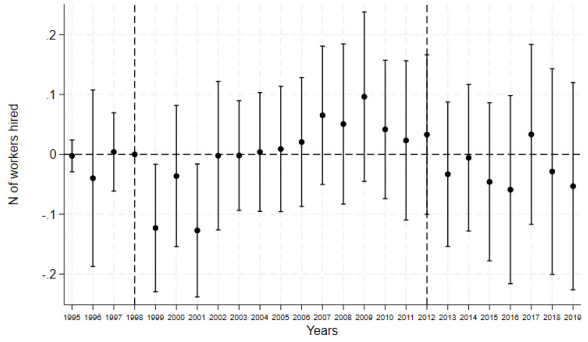
Panel A: No. of focal worker retentions



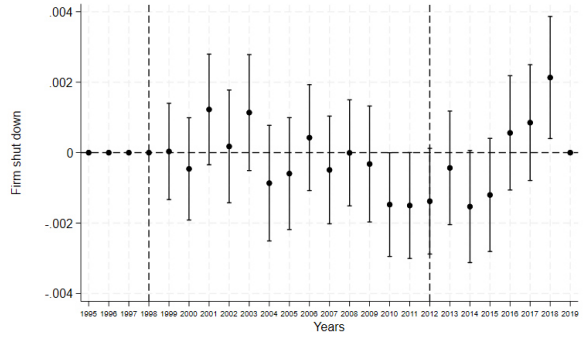
Panel B: No. of coworker promotions



Panel C: No. of hires

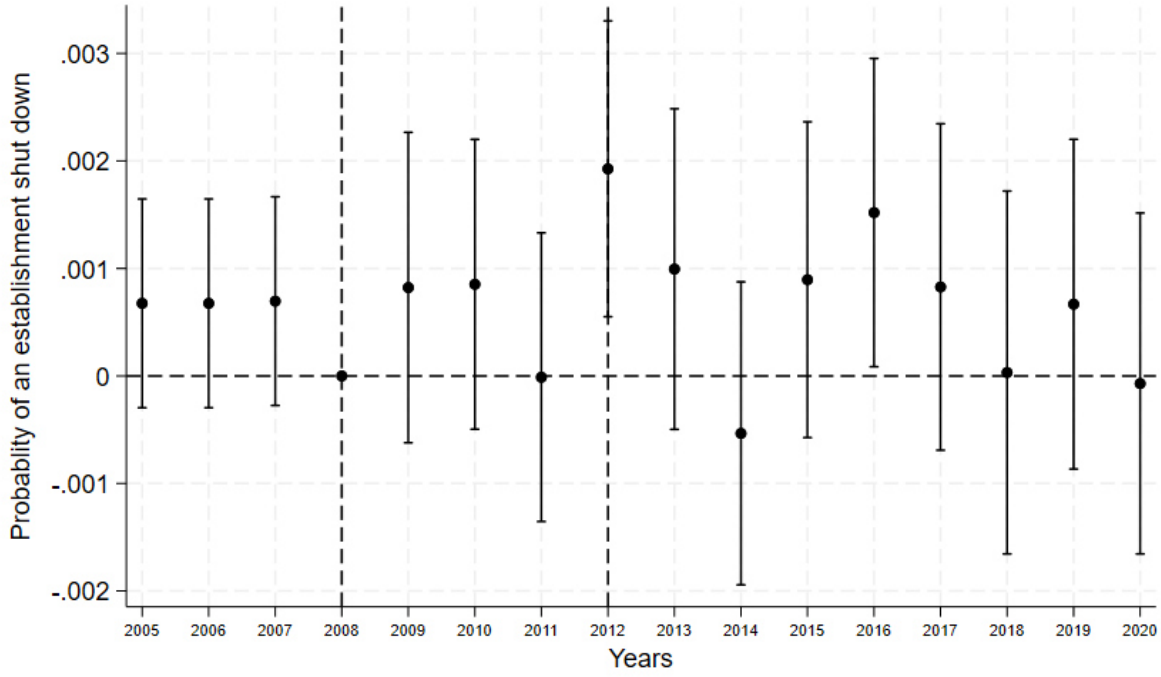


Panel D: Probability of a firm closure



Notes: This figure represents the effect of having one additional treated worker (1952–1953 birth cohorts) in 1998 on number of focal worker retentions (**Panel A**); coworker promotions (**Panel B**); external hires (**Panel C**); and firm closure (**Panel D**). The points represent the estimated coefficients β_τ in Equation 1 and the vertical bars represent 95% confidence intervals. The dashed vertical line in 1998 represents the year before the reform passed, while the second dashed line represents when the first affected cohort (1952 birth cohort) reached the age of 60. Standard errors are clustered at the establishment level.

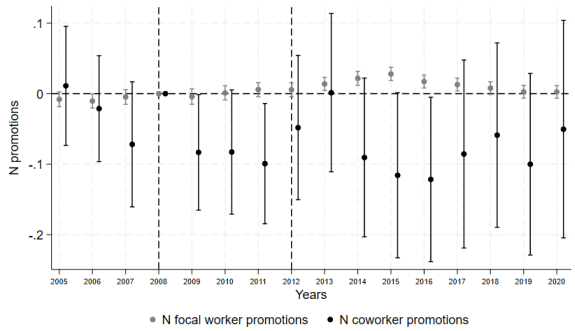
Figure D.4: The effect of an additional treated focal worker employed in 2008 on the probability of an establishment closure



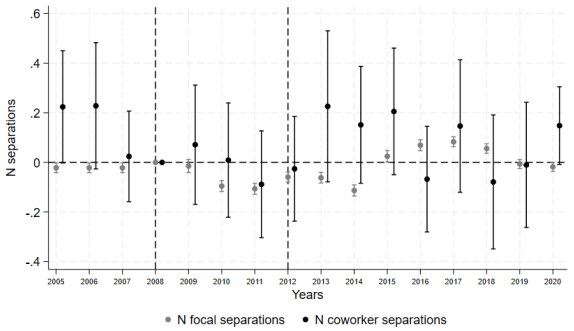
Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the probability of a firm closure each year. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Figure D.5: The effect of an additional treated focal worker employed in 2008 on promotions and separations of focal workers and coworkers

Panel A: No. of promotions



Panel B: No. of separations

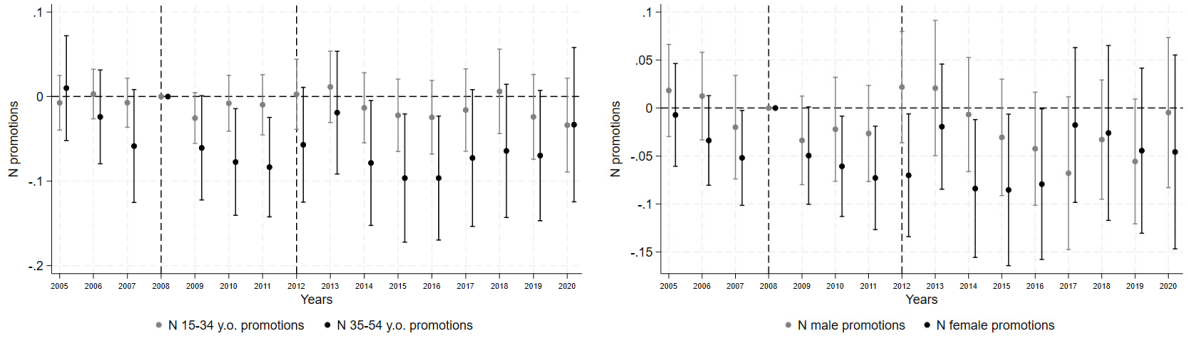


Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of promotions (**Panel A**) and separations (**Panel B**) of focal workers (cohorts 1950-1953, in gray) and coworkers (in black) in each year. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Figure D.6: The effect of an additional treated focal worker employed in 2008 on coworker promotions, separations, and external hiring by age groups and gender

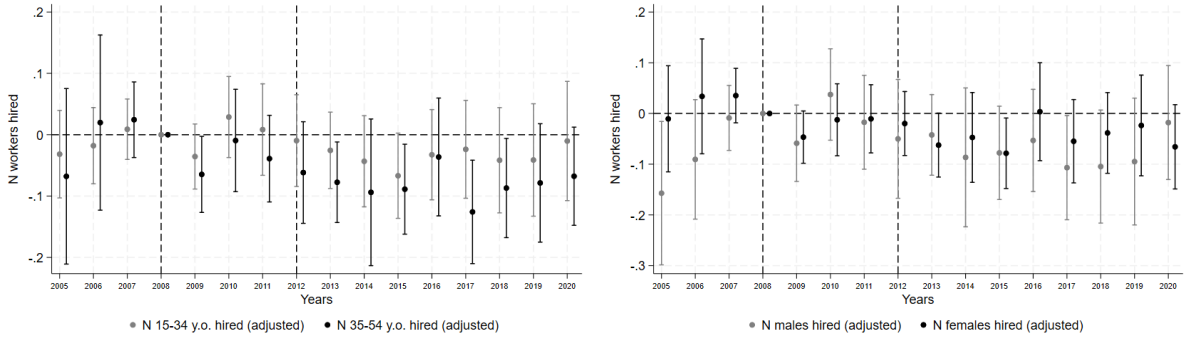
Panel A: No. of coworker promotions by age

Panel B: No. of coworker promotions by gender



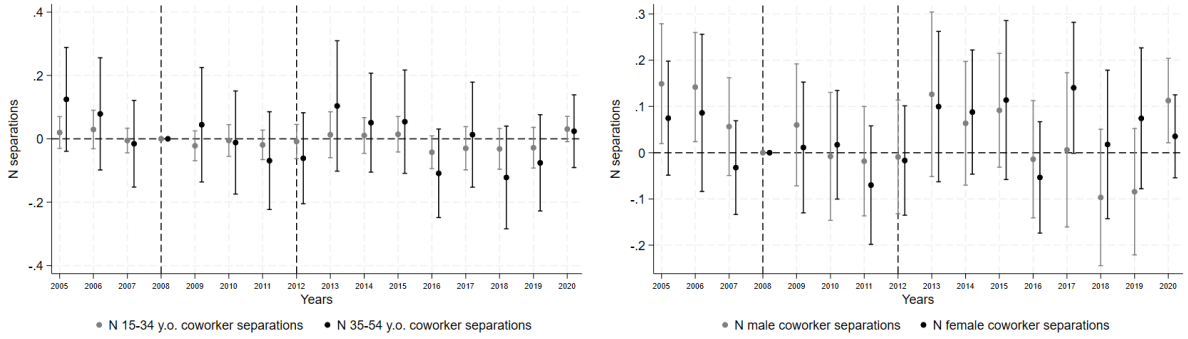
Panel C: No. of hires by age

Panel D: No. of hires by gender



Panel E: No. of coworker separations by age

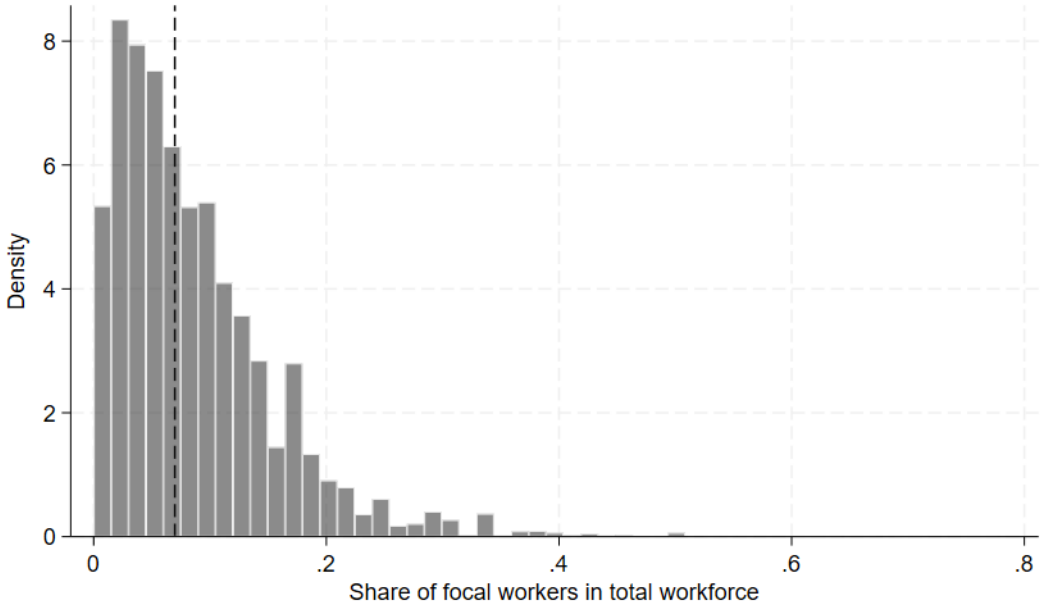
Panel F: No. of coworker separations by gender



Notes: This figure represents the effect of having one additional treated worker (1952-1953 cohorts) in 2008 on the number of coworker promotions by age (**Panel A**) and gender (**Panel B**); the number of hires by age (**Panel C**) and gender (**Panel D**); the number of coworker separations by age (**Panel E**) and gender (**Panel F**) in each year. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, specifically when all the focal workers (1950-1953 cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Figure D.7: Firm-level treatment variables

Panel A: Distribution of share of treated focal workers in total workforce



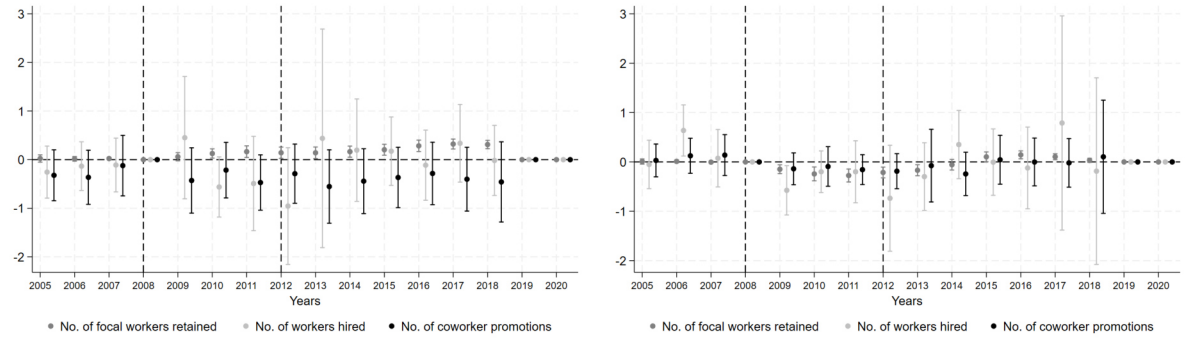
Panel B: Main treatment variables in 2008

Notes: **Panel A** displays the distribution of the share of focal workers in the total workforce in 2008. The vertical dashed line displays the median value in the distribution, which stands for almost 10% of the total workforce and is almost equivalent to the distribution mean. In both panels, the part-time workers are counted as 0.5 workers (the headcounts are adjusted for working hours). **Panel B** displays the share of 2008 workers (black circles show the share of treated focal workers (birth cohorts 1952-1953)) in focal workers (birth cohorts 1950-1953), medium-gray diamonds show the share of treated focal workers in the total workforce, and the light-gray triangles show the share of focal workers in the total workforce) over different establishment sizes. The horizontal dashed line at 0.5 indicates a point of no excess mass of share of treated focal workers among the total focal workers.

Figure D.8: Falsification tests by birth cohorts and gender

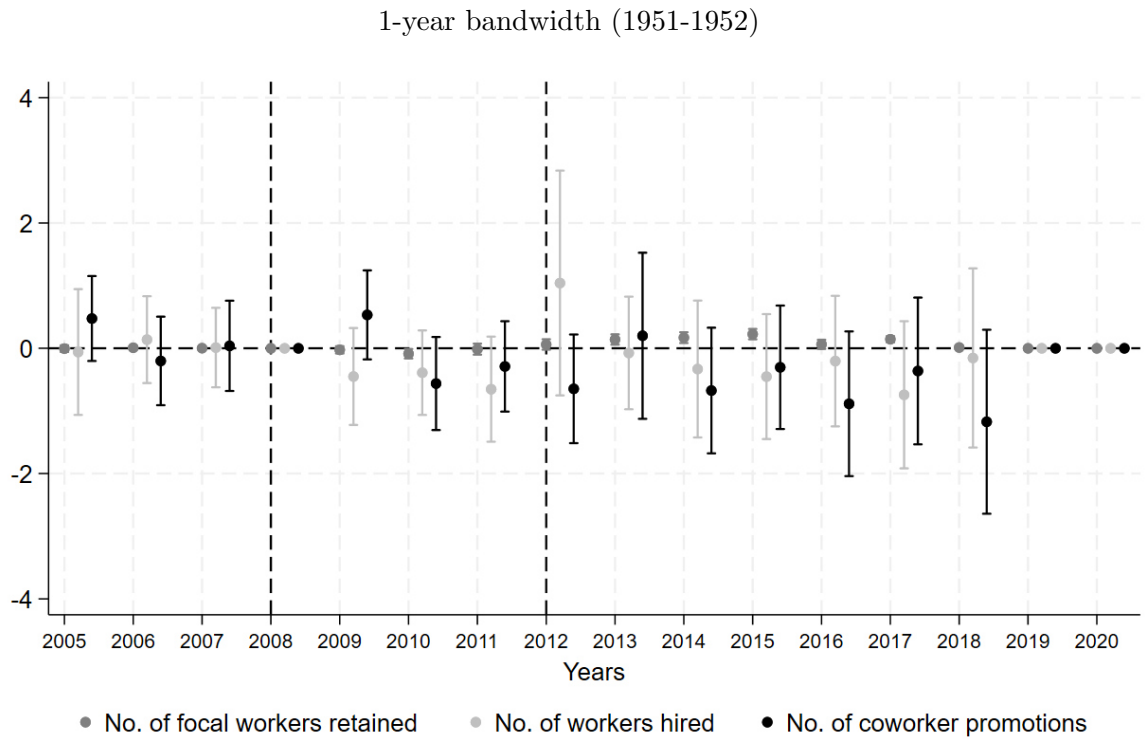
Panel A: Placebo birth cohorts (1952-1955, all treated)

Panel B: Placebo gender (males)



Notes: This figure represents the effect of having one additional placebo-treated worker in 2008 on the number of focal worker retentions, number of external hires, and number of internal promotions. I perform falsification tests by redefining the focal workers as women born between 1952 and 1955, that is, all treated (**Panel A**) and as males born between 1950 and 1953 (**Panel B**). The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. Standard errors are clustered at the establishment level. For sample construction details, see Appendix B.

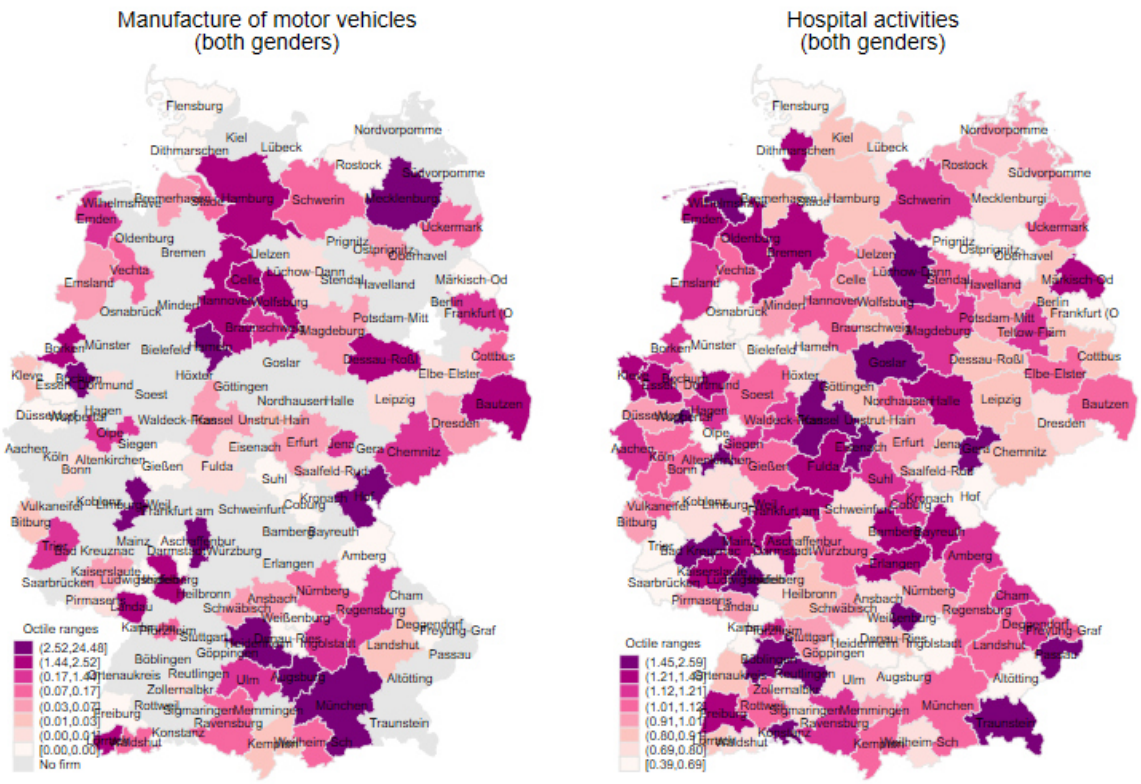
Figure D.9: Robustness check: altering the estimation bandwidth around the 1952 cutoff



Notes: This figure represents the effect of having one additional treated worker (1952 cohorts) in 2008 on the number of focal worker retentions, number of external hires, and number of internal promotions. I perform a robustness test by redefining the window of focal workers as women born in 1951-1952, that is, a 1-year bandwidth. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. Standard errors are clustered at the establishment level. For sample construction details, see Appendix B.

Figure D.10: Example of external labor market thickness in 2007

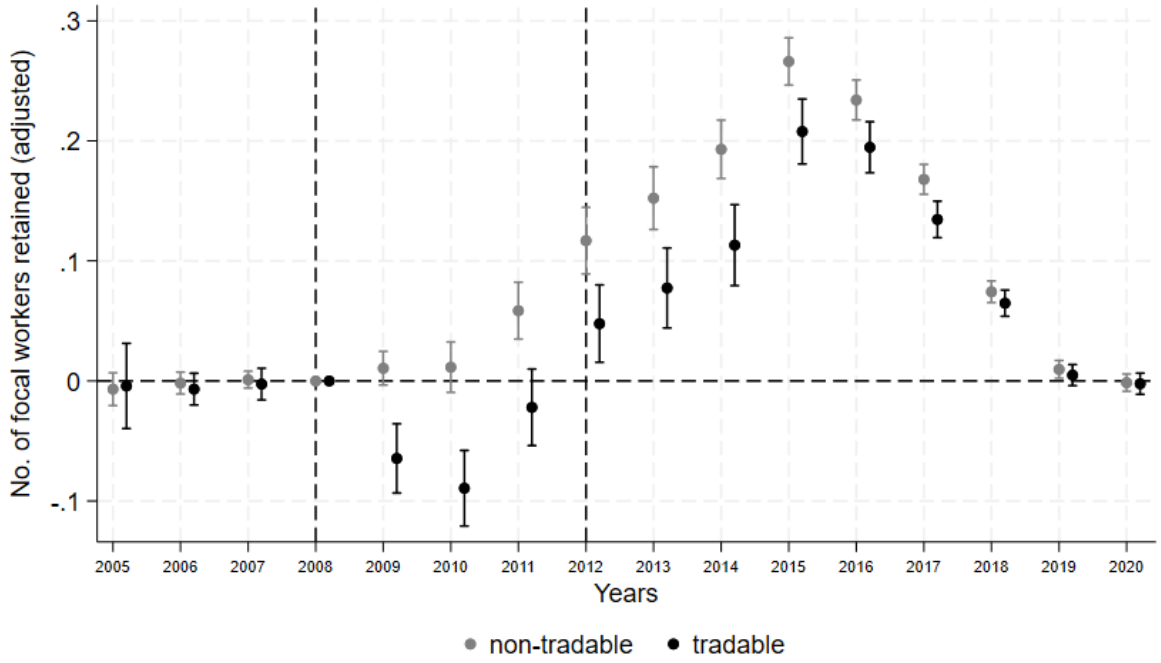
Panel A **Panel B**



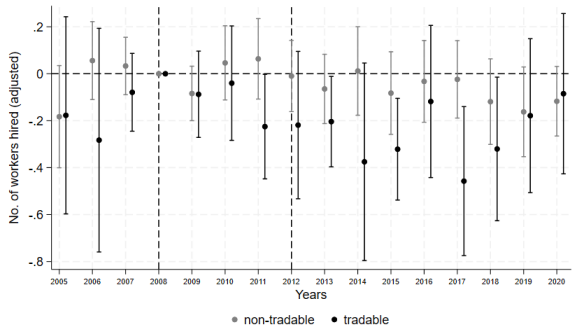
Notes: This map shows the computed external labor market thicknesses (ELMT) for each of the 141 German labor market regions based on the classification of Kosfeld and Werner (2012), based on high within-region commuting and low between-region commuting. I compute ELMT based on Equation 4 for the two large German industries: "manufacture of motor vehicles" (**Panel A**) and "hospital activities" (**Panel B**). I plot the ELMT indices on the map using the eight quantile ranges (octiles) shown in the left corner of each graph.

Figure D.11: The effect of an additional treated focal worker employed in 2008 on focal worker retentions, coworker promotions, and external hiring by industry tradability

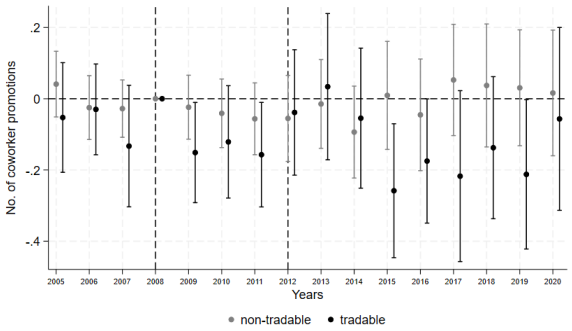
Panel A: No. of focal worker retentions



Panel B: No. of hires



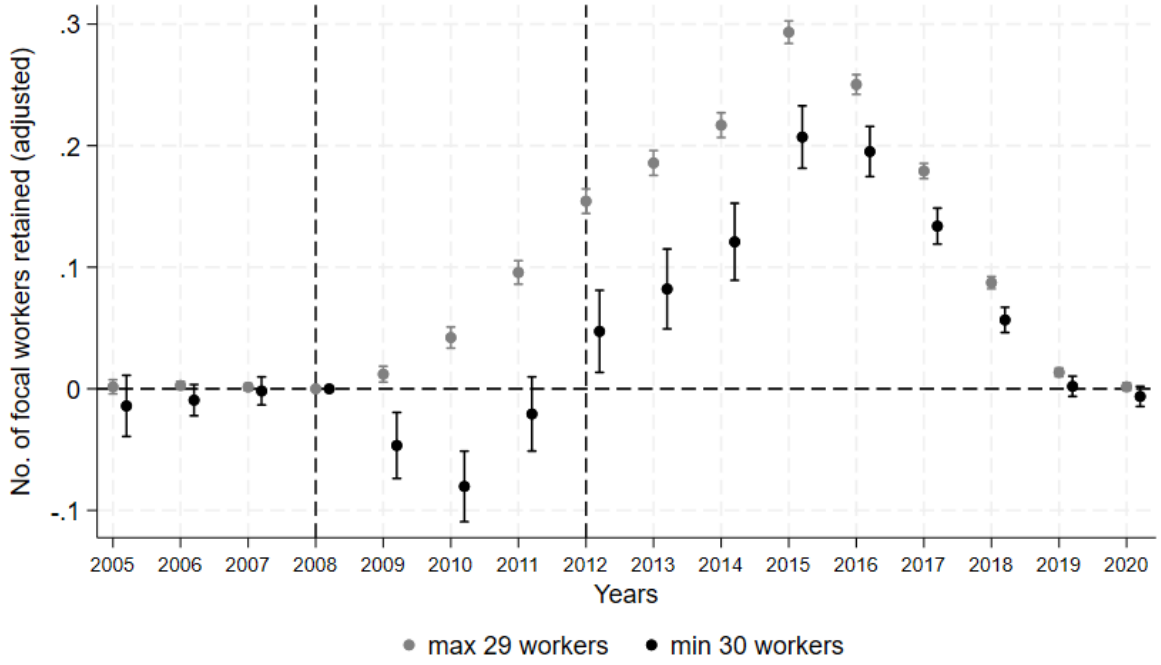
Panel C: No. of promotions



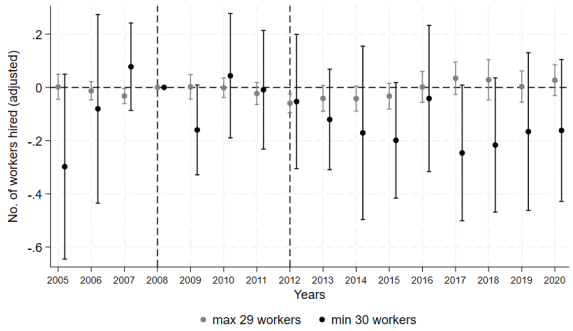
Notes: This figure shows the effects of an additional treated focal worker employed in 2008 on the number of retentions (**Panel A**), the number of external hires (**Panel B**), and the number of promotions (**Panel C**) by industry tradability. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all the focal workers (1950-1953 cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Figure D.12: The effect of an additional treated focal worker employed in 2008 on focal worker retentions, coworker promotions, and external hiring by establishment size

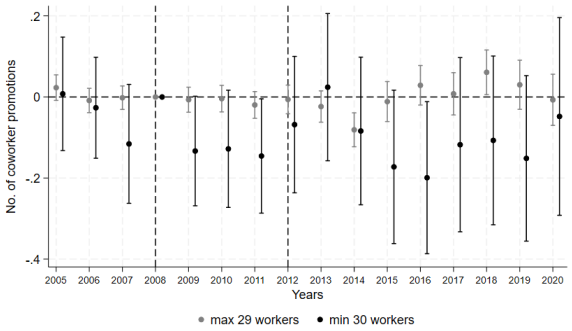
Panel A: No. of focal worker retentions



Panel B: No. of hires

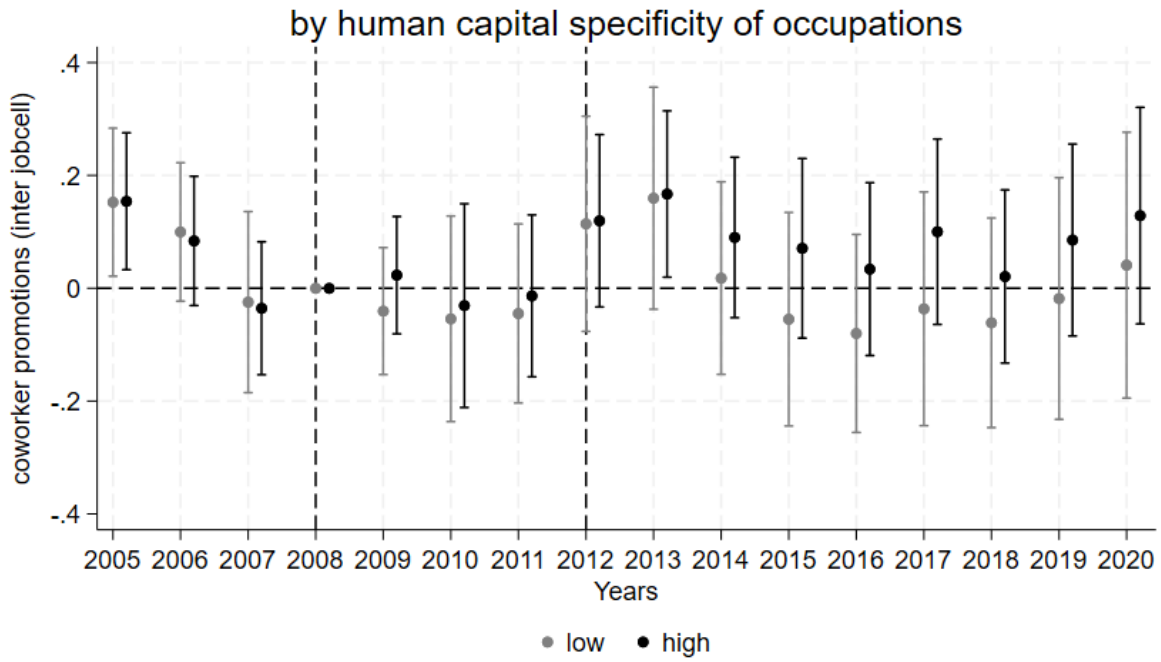


Panel C: No. of promotions



Notes: This figure shows the effects of an additional treated focal worker employed in 2008 on the number of retentions (**Panel A**), the number of external hires (**Panel B**), and the number of promotions (**Panel C**) by establishment size categories. I split the establishments by those below 30 (gray color) and above 30 (black color) workers. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all the focal workers (1950-1953 cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

Figure D.13: Inter-jobcell effects on coworker promotions by human capital specificity



Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on inter-jobcell promotions in each year by low (in gray) and high (in black) human capital specificity of occupations. The points represent the estimated coefficients δ_τ in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical line represents the year before policy enactment, when all focal workers (1950-1953 birth cohorts) were under the age of 60. Standard errors are clustered at the establishment level.

E Appendix Tables

Table E.1: Share of industry employment by gender in 1998 and 2007

Industry	**Panel A: 1998**		**Panel B: 2007**	
	share women	share men	share women	share men
Agriculture, forestry, and fishing	0.32	0.68	0.32	0.68
Mining and quarrying	0.09	0.91	0.09	0.91
Manufacturing	0.26	0.74	0.25	0.75
Electricity, gas, steam, air conditioning supply	0.21	0.79	0.23	0.77
Water supply; sewerage, waste management and remediation activities	0.18	0.82	0.18	0.82
Construction	0.12	0.88	0.12	0.88
Wholesale and retail trade; repair of motor vehicles and motorcycles	0.50	0.50	0.50	0.50
Transportation and storage	0.27	0.73	0.25	0.75
Accommodation and food service activities	0.58	0.42	0.57	0.43
Information and communication	0.38	0.62	0.36	0.64
Financial and insurance activities	0.54	0.46	0.55	0.45
Real estate activities	0.49	0.51	0.49	0.51
Professional, scientific, and technical activities	0.53	0.47	0.53	0.47
Administrative and support service activities	0.42	0.58	0.40	0.60
Public administration and defense; compulsory social security	0.59	0.41	0.61	0.40
Education	0.67	0.33	0.67	0.33
Human health and social work activities	0.80	0.20	0.80	0.20
Arts, entertainment, and recreation	0.48	0.52	0.50	0.50
Other service activities	0.66	0.34	0.66	0.34
Activities of household as employers; undifferentiated goods and services-producing activities of households for their own use	0.86	0.14	0.88	0.12
Activities of extraterritorial organizations and bodies	0.33	0.67	0.36	0.64

Notes: This table shows the female and male employment share in each of 22 industries. I aggregate 3-digit industries (based on the 2008 classification) into 22 groups following suggestions by Statistisches Bundesamt. The numbers are generated from the universe of full-time employed workers aged 18-64 employed in jobs subject to social security or vocational training as of June 30th, 1998 (**Panel A**) or 2007 (**Panel B**).

Table E.2: Sample restrictions to obtain the original data extract for upstream and main reform periods

Restriction	No. of establishments	No. of workers
Panel A: upstream period		
Universe of establishment and workers in 1995-2019	8,611,676	69,208,790
+ observed in 1998	2,044,663	
+ employed at least 1 focal worker in 1998	413,995	
+ private sector	382,007	
+ at least 5 employees in 1998	221,853	
+ at most 500 employees in 1998	218,588	32,506,683
+ Workforce with positive wages and employment subject to social security in 1998, and redefined focal group (cohorts 1950-1953)	140,222	21,774,237
+ 5-199 workers	131,592	15,625,535
Panel B: main reform period		
Universe of establishments and workers in 1995-2020	8,241,529	69,296,143
+ observed in 2008	1,958,754	23,798,218
+ employed at least 1 focal worker in 2008	352,836	15,364,408
+ private sector	317,912	13,388,108
+ at least 5 employees in 2008	193,612	13,059,745
+ at most 500 employees in 2008	190,228	9,117,917
Universe of affected establishments, and their employed workers in 1995-2020	190,228	26,593,003

Notes: This table shows the number of establishments and workers after each restriction in the data extract requested, separately for the upstream period (**Panel A**) and main reform period (**Panel B**).

Table E.3: Comparison of characteristics in analysis sample with a random sample of German establishments

	Panel A: upstream period		**Panel B: main reform period**	
	Random sample	Sampled establishments	Random sample	Sampled establishments
located in East Germany	0.234 (0.424)	0.264 (0.441)	0.209 (0.407)	0.221 (0.415)
No. of non-German workers	11.784 (84.822)	2.434 (6.693)	1.154 (19.372)	3.216 (10.174)
No. of female workers	5.322 (35.329)	18.755 (23.43)	6.677 (33.933)	24.335 (37.797)
No. of workers with university degree	1.519 (21.804)	3.835 (9.01)	2.516 (40.415)	6.643 (18.181)
No. of workers 15-34 y.o.	4.486 (36.634)	12.631 (15.084)	4.064 (31.398)	11.971 (20.64)
No. of workers 35-54 y.o.	6.722 (52.281)	21.375 (22.962)	9.583 (78.63)	30.381 (46.063)
No. of workers 55+ y.o.	1.475 (10.704)	4.864 (6.865)	1.991 (11.456)	7.622 (10.742)
No. of full-time workers	10.81 (87.239)	33.435 (36.106)	12.765 (107.217)	39.396 (62.526)
No. of part-time workers	1.842 (17.585)	5.085 (12.21)	2.867 (19.896)	10.562 (24.56)
Observations	30,296	131,592	21,581	160,667

Notes: This table shows the characteristics of a random sample of establishments (1.5% random sample based on SIEED7518 data and the sampled establishments (universe of affected firms sampled from IEB). IEB data are described in section 3, while the sampling is described in section 3. The comparison is performed separately for the upstream period (**Panel A**, variables measured in sampling year 1998), and main reform period (**Panel B**, variables measured in sampling year 2008).

Table E.4: Comparison of the industry composition in the analysis sample with a random sample of German establishments.

	Panel A: upstream period		**Panel B: main reform period**	
	Random sample	Sampled establishments	Random sample	Sampled establishments
Agriculture, forestry, and fishing	0.017 (0.128)	0.016 (0.125)	0.016 (0.127)	0
Mining and quarrying	0.002 (0.043)	0.002 (0.048)	0.002 (0.049)	0
Manufacturing	0.111 (0.314)	0.232 (0.422)	0.109 (0.311)	0.199 (0.399)
Electricity, gas, steam and air conditioning supply	0.002 (0.044)	0.004 (0.066)	0.003 (0.051)	0.005 (0.067)
Water supply; sewerage, waste management, and remediation activities	0.004 (0.064)	0.007 (0.085)	0.004 (0.067)	0.008 (0.087)
Construction	0.121 (0.326)	0.078 (0.269)	0.107 (0.31)	0.049 (0.215)
Wholesale and retail trade; repair of motor vehicles and motorcycles	0.229 (0.42)	0.231 (0.421)	0.211 (0.408)	0.247 (0.431)
Transportation and storage	0.043 (0.204)	0.036 (0.185)	0.041 (0.199)	0.039 (0.195)
Accommodation and food service activities	0.07 (0.255)	0.042 (0.201)	0.056 (0.23)	0.043 (0.203)
Information and communication	0.015 (0.123)	0.022 (0.145)	0.021 (0.144)	0.02 (0.14)
Financial and insurance activities	0.024 (0.154)	0.038 (0.191)	0.027 (0.162)	0.034 (0.182)
Real estate activities	0.024 (0.152)	0.013 (0.113)	0.021 (0.142)	0.012 (0.107)
Professional, scientific, and technical activities	0.082 (0.275)	0.231 (0.421)	0.089 (0.285)	0.075 (0.263)
Administrative and support service activities	0.036 (0.186)	0.036 (0.185)	0.042 (0.201)	0.047 (0.211)
Public administration and defense; compulsory social security	0.018 (0.131)	0.042 (0.201)	0.019 (0.137)	0
Education	0.024 (0.154)	0.013 (0.115)	0.031 (0.173)	0
Human health and social work activities	0.093 (0.29)	0.082 (0.275)	0.118 (0.323)	0.15 (0.357)
Arts, entertainment, and recreation	0.013 (0.113)	0.008 (0.088)	0.012 (0.108)	0.01 (0.101)
Other service activities	0.056 (0.23)	0.043 (0.203)	0.059 (0.236)	0.049 (0.216)
Activities of the household as employers; undifferentiated goods and services-producing activities of households for their own use	0.014 (0.119)	0 (0.021)	0.009 (0.097)	0 (0.011)
Activities of extraterritorial organizations and bodies	0.002 (0.047)	0 (0.011)	0.001 (0.031)	0.014 (0.118)
Observations	30296	131592	21,581	160,667

Notes: This table shows the characteristics of a random sample of all the establishments in Germany (1.5% random sample based on SIEED7518 data and the sampled establishments (universe of affected firms sampled from IEB). IEB data are described in section 3, and the sampling is described in section 3. I aggregate 3-digit industries (based on the 2008 classification) into 22 groups following suggestions by Statistisches Bundesamt. The comparison is performed separately for the upstream period (**Panel A**, variables measured in sampling year 1998), and the main reform period (**Panel B**, variables measured in sampling year 2008).

Table E.5: Balance test: quantiles of "share of treated focal workers in total workforce"

Variable	Means				Differences			t-stat		
	Q1	Q2	Q3	Q4	Q2	Q3	Q4	Q2	Q3	Q4
located in East Germany	0.22	0.18	0.23	0.25	-0.04***	0.02***	0.04***	-13.29	6.70	12.69
No. of non-German	1.85	8.82	2.87	0.67	6.97***	1.03***	-1.18***	78.84	20.79	-33.89
No. of female workers	11.65	46.71	34.12	11.37	35.06***	22.47***	-0.28**	150.09	96.02	-2.70
No. of University degree	3.61	17.91	6.27	1.52	14.30***	2.66***	-2.08***	91.72	30.84	-35.98
No. of workers 15-34 y.o.	7.66	31.10	10.76	2.84	23.44***	3.11***	-4.82***	140.53	31.98	-64.21
No. of workers 35-54 y.o.	16.83	74.40	32.48	8.76	57.56***	15.65***	-8.07***	176.13	66.84	-58.01
No. of workers 55+ y.o.	4.31	15.57	9.03	3.71	11.26***	4.72***	-0.60***	150.56	78.44	-16.49
No. of full-time workers	24.62	102.95	35.13	9.92	78.33***	10.52***	-14.70***	162.90	36.73	-70.87
No. of part-time workers	4.17	18.09	17.14	5.39	13.92***	12.97***	1.22***	90.89	80.45	17.43
Manufacturing	0.19	0.32	0.18	0.13	0.12***	-0.01***	-0.07***	39.81	-5.10	-27.21
Electricity, gas, steam, and air conditioning supply	0.00	0.01	0.00	0.00	0.01***	0.00**	-0.00***	14.21	2.62	-6.64
Water supply; sewerage, waste management and remediation activities	0.01	0.01	0.01	0.00	0.00***	-0.00**	-0.00***	6.53	-3.07	-8.45
Construction	0.07	0.04	0.04	0.04	-0.03***	-0.04***	-0.03***	-19.99	-23.50	-16.68
Wholesale and retail trade; repair of motor vehicles and motorcycles	0.27	0.18	0.23	0.29	-0.08***	-0.03***	0.02***	-27.55	-11.38	5.92
Transportation and storage	0.04	0.05	0.04	0.03	0.01***	-0.00	-0.01***	9.76	-1.26	-4.92
Accommodation and food service activities	0.05	0.02	0.04	0.05	-0.03***	-0.01***	0.00**	-20.93	-7.44	3.09
Information and communication	0.02	0.03	0.02	0.01	0.01***	0.00	-0.01***	9.69	0.89	-5.87
Financial and insurance activities	0.02	0.05	0.04	0.03	0.03***	0.02***	0.01***	23.33	14.06	4.98
Real estate activities	0.01	0.01	0.01	0.02	-0.01***	-0.00	0.00***	-7.74	-1.32	4.44
Professional, scientific and technical activities	0.09	0.05	0.06	0.10	-0.04***	-0.03***	0.01***	-19.72	-14.71	4.89
Administrative and support service activities	0.04	0.07	0.05	0.03	0.02***	0.00**	-0.01***	13.81	2.78	-7.60
Human health and social work activities	0.11	0.11	0.21	0.17	0.00	0.10***	0.06***	0.91	41.05	27.48
Arts, entertainment and recreation	0.01	0.01	0.01	0.01	-0.00	-0.00	-0.00	-0.48	-1.34	-0.11
Other service activities	0.05	0.03	0.05	0.07	-0.02***	0.00**	0.02***	-13.94	2.58	13.69
Activities of household as employers; undifferentiated goods and services	0.00	0.00	0.00	0.00	-0.00*	-0.00	0.00	-2.44	-1.45	0.46
Activities of extraterritorial organizations and bodies	0.02	0.01	0.02	0.01	-0.01***	-0.00	-0.00***	-7.82	-0.54	-4.51
firm age below 5 years	0.07	0.06	0.07	0.07	-0.01***	-0.01**	0.00	-3.83	-3.09	1.15
firm age 6-15 years	0.29	0.25	0.27	0.30	-0.03***	-0.01***	0.01**	-10.51	-4.47	2.88
firm age 16-30 years	0.33	0.26	0.31	0.34	-0.07***	-0.02***	0.02***	-20.80	-6.62	5.38
firm age over 30 years	0.31	0.42	0.35	0.29	0.11***	0.04***	-0.03***	31.80	12.47	-9.03
No. of hires	3.84	14.09	4.04	1.08	10.25***	0.21***	-2.76***	95.17	3.60	-54.62
No. of focal workers (adjusted)	1.01	2.64	3.05	1.83	1.63***	2.03***	0.81***	131.72	109.97	80.62
No. of total workers (adjusted)	26.72	112.02	43.71	12.62	85.31***	16.99***	-14.10***	173.78	53.52	-65.75
Share of treated focal workers in focal workers	0.00	0.69	0.77	0.88	0.69***	0.77***	0.88***	514.98	660.86	965.85
Observations	80586	89490	87495							

Notes: This table shows the summary statistics (means, rows 2-5) and balance tests (differences in rows 6-8, t-statistics in rows 9-11) of quartiles ($Q1$, $Q2$, $Q3$, $Q4$) of the share of treated focal workers within firms $\frac{N\_TreatedFocal_j}{N_j}$. All the variables are measured in 2008. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.