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

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

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Abstract

I study how delayed retirements reshape firms internal labor markets, leveraging a German reform that raised womens 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, with the greatest losses among middle-aged workers who were near to older workers on the career ladder. 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. Taken together, the evidence supports slot-constraint theoriesaugmented by firm-specific human-capital mechanisms.

Zusammenfassung

Ich untersuche, wie verzögerte Renteneintritte die internen Arbeitsmärkte von Unternehmen verändern, und nutze dazu eine deutsche Reform, die das frühestmögliche Renteneintrittsalter von Frauen um mindestens drei Jahre angehoben hat. Die Reform erhöhte die Beschäftigungsdauer älterer Frauen und führte zugleich zu einem Rückgang sowohl interner Beförderungen als auch externer Einstellungen jüngerer Beschäftigter. Die größten Verluste tragen dabei Arbeitnehmer mittleren Alters, die auf der Karriereleiter den verbleibenden älteren Beschäftigten am nächsten stehen. Die Spillover-Effekte sind klar strukturiert: Beförderungsverdrängung tritt vor allem in dichten internen Arbeitsmärkten mit intensiver Konkurrenz auf, während Rückgänge bei Neueinstellungen in dünnen externen Arbeitsmärkten mit hohen Fluktuationskosten am stärksten sind. Die Verdrängungseffekte konzentrieren sich innerhalb von Jobzellen, während Beschäftigte in anderen Jobzellen profitieren können, wenn verbleibende ältere Arbeitnehmer über firmenspezifisches Humankapital verfügen. Insgesamt stützen die Ergebnisse Theorien der Stellenrestriktionen (slot constraints), ergänzt um Mechanismen firmenspezifischen Humankapitals.

JEL

H55, J21, J23, J24, J26, J31, J63, M51

Keywords

aging, internal labor markets, human capital, worker substitutability

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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 womens 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 womens 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/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 (2025b) 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/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/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 similar firms across all characteristics, including the total number of focal workers, which differ in terms of the number of workers born just after the cutoff (1952-1953 cohorts, that is, *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 establishmentssingle 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 consists of the universe of private sector firms existing in 1998, the pre-reform year, and yields approximately 140,000 establishments in total with around 250,000 focal workers and ten million coworkers employed in these establishments in 1998. 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 coworkers and influencing external hiring practices. I do not find significant spillover effects on coworker promotions or on external hiring in the upstream period.

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

Because firms show virtually no adjustment in the upstream period, I re-sample the analysis in 2008before any treated workers reached pensionable ageswhich 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 20122017. 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/Oyer (2004) find that internal hiring (promotions) constitutes 44-88% of job postings, with the highest percentage occuring 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 workersespecially womenexperience 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 ladderso 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/van Ours, 2021). This mechanism predicts negative spillovers on younger workers, especially in hierarchical labor markets such as Germanys, where a large share of lifetime wage growth reflects career ladders (Bayer/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/Hackmann, 2021; Jäger/Heining, 2022; Jaravel/Petkova/Bell, 2018). When skills are highly specific, internal or external hires are imperfect substitutes (Chan, 1996;

[2] Employers replace workers who die suddenly by increasing hiring by about 0.4 workers (Jäger/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/Xiao (2023), who document gender-specific spillovers following paternity leave.

Herrmann/Rockoff, 2012; Waldman, 2003), making retention of older workers valuable to employers and potentially beneficial for coworkers through complementarities. Replacement hiring is costlyaround two months wages for high-skilled workers (Muehlemann/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 (2025b) shows that the 1999 reform disproportionately increased retention among older workers who are costly to replacemanagers, workers in highly specific occupations, and those in firms with few internal or external substitutesunderscoring 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/complementarity 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 firms 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/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 marketscommuting zones and industries in which suitable replacement workers are scarceshould 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 workersespecially in jobcells with many coworkers competing for the same rungs on the ladderwhile workers in other occupations may benefit from cross-occupation complementarities (Jäger/Heining, 2022). In establishments in which many coworkers share the older workers 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/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 jobcellswhere tasks and ladders overlapthe 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 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[4] to the smaller but growing body of work on intra-firm spillovers of rising

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

retirement ages, thereby linking the retirement and internal labor market literature (Doeringer/Piore, 1971; Lazear/Oyer, 2004).[5] Existing evidencemainly from Italy (Bianchi et al., 2023; Boeri/Garibaldi/Moen, 2022; Carta/DAmuri/Von Wachter, 2024), and the Netherlands (Hut, 2024; Ferrari/Kabátek/Morris, 2023)offers mixed findings on whether older worker retention crowds out or complements younger colleagues.[6] One explanation for these discrepancies lies in firm heterogeneity: positive effects in Carta/DAmuri/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 reconciliation of prior puzzles through incorporating internal and external labor markets into spillovers and novel mechanisms.

Unlike settings in which financial frictions shape workforce adjustments (Hut, 2024), the long pre-announcement horizon of the German reform makes liquidity constraints unlikely.[7] 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-outechoing evidence on high replacement costs and thin labor markets (Ginja/Karimi/Xiao, 2023; Jäger/Heining, 2022; Huebener et al., 2024; Schmutte/Skira, 2023). Inside a firm, promotion spillovers depend on internal structure and human-capital specificity: using establishmentoccupation identifiers, I show that within jobcellswhere tasks and ladders overlapretained 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 hierarchiesconsistent with human-capital and internal labor-market theories (Baker/Gibbs/Holmstrom, 1994; Huitfeldt et al., 2023). This unified view helps reconcile

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

[6] Further related work includes evidence from Portugal (Martins/Novo/Portugal, 2009), Norway (Hernæs et al., 2023), and the 1992 German reform (Berg et al., 2025). Mohnen (2025) studies spillovers in the US at a commuting zone level. Differences in institutional context, data coverage, and identification strategies make these studies informative but not fully comparable to the setting examined here.

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

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. Chapter 2 describes the institutional setting. Chapter 3 describes the data source, the sample construction, and the corresponding identification strategy. Chapter 4 shows the baseline results, followed by chapter 5, which outlines a simple model of firm decisions and quantifies the elasticity of substitution. Chapter 6 shows intra- and inter-jobcell spillovers. Finally, I conclude in chapter 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/Heining, 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/Pfeiffer, 2006).

Employment protection. Older workers are strongly protected under the Equal Treatment Act (AGG),[8], and severance pay rises steeply with tenure (Hut, 2024). Germany has relatively stable jobs and relatively high severance pay (Doeringer/Piore, 1971), 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 D1 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

[8] 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).

approximately 90% of the German workforce (Geyer/Welteke, 2021; Zwick et al., 2022). The public pension system consists of a pay-as-you-go scheme, where current taxpayers pay for the pensions of the old.

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 intended for vulnerable groupssuch as women, long-term insured workers, or unemployed workersconditional on meeting the specific eligibility criteria. Retiring between the ERA and NRA entails actuarial deductions of 0.3% per month.[9] 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/Schrader, 2021; Geyer/Welteke, 2021). Importantly, the 0.3% monthly deduction is low by international standards and not actuarially neutral (Queisser/Whitehouse, 2006), making ERA claims particularly attractive. Cross-country evidence shows that ERA are more successful in raising the effective age of retirement than changes in NRA (Boeri/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 womens 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/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 womenabout 90% of those previously eligible, who also met the 35-year contribution requirement for the long-insurance pathwaythe 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.[10] Further institutional details, such as the description of pathways, are provided in section A1.

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. It

[9] For a woman in the 1951 cohort, retiring at age 60 implies an 18% permanent reduction.

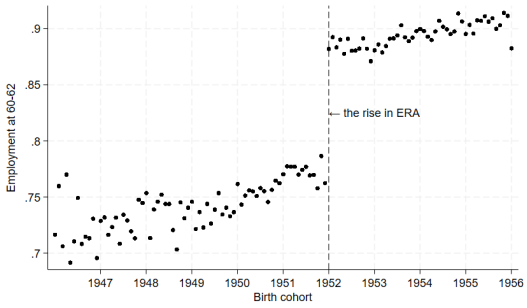
[10] The regular pathway retirement age also increased due to the 2007 reform, which raised the NRA in small increments; thus, the NRA rose from 65 for the 1951 cohort to 65.5 for the 1952 cohort.

generated substantial labor-supply responses: Geyer/Welteke (2021) document a 13.3 pp increase in employment at ages 6062 with no offsetting rise in disability, unemployment, or inactivity. Conditional on employment at ages 5859, the employment discontinuity at ages 6063 reaches 17.3 pp (Badalyan, 2025b). Consistent with these patterns, Figure C1 shows pronounced bunching at the relevant ERA (60 and 63) and NRA thresholds (65 and 65.5) for the 1951 and 1952 cohorts.

Figure 1: Discontinuity in birth cohorts

Panel A: Assignment rule

Panel B: Scatter plot (Badalyan, 2025b)



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

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 A1). 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 19992012, 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 6062), 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 are informative about anticipatory adjustments and post-shock recovery.

The reform offers a useful case to study firm responses to workforce aging. The reform's 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 agecontracts end only at the NRAthe 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 womens early retirement age. I first describe the data source and sample constructionprivate-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 reforms 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).[11] 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 excludes 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 30th, 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 till 2021, with a starting date in 1975 for West Germany and 1992 for East Germany.[12]

The data includes precise day-to-day information on the start and end dates of employment spells and wages, which include overtime pay and bonus payments.[13] The data are rich in

[11] 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.

[12] 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.

[13] 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

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 includes 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 chapter 5.

The data consists of a universe of single locations of multi-site firms, that is, establishments.[14] 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 call use the terms establishment and firm interchangeably throughout this paper (Card/Heining/Kline, 2013; Dustmann/Ludsteck/Schönberg, 2009; Jäger/Heining, 2022), because single-establishment firms constitute the majority of the data (Jäger/Heining, 2022). More information about the data can be found in Jacobebbinghaus/Seth (2007).

3.1.1 Sample Construction for the Upstream Period

First, I construct a workeryear panel following Dauth/Eppelsheimer (2020), aggregating annual records as of June 30ththe date when employers submit annual workforce notifications and the reference point used in the IAB Establishment Panel.[15] I define *focal workers* as female employees born within two years of the 1952 cutoffthe first cohort affected by the abolition of the female earlyretirement pathwayexcluding miners and sailors, whose special retirement rules are not identifiable in the data (Lorenz et al., 2018).[16] 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 chapter 3).

wage correction would impact only a small fraction of workers in my sample (Drechsler/Ludsteck/Moczall, 2023); hence, I refrain from performing such imputations.

[14] 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.

[15] June 30th aligns reporting across datasets and captures the administrative employment stock.

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

I restrict the sample to private-sector establishments[17] with 5500 employees in 1998. I exclude the public sector because employment dynamics differ markedly (Oberfichtner/Schnabel, 2019) and substitutability responses may be muted by politically fixed budgets (Ginja/Karimi/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 98th percentile of the size distribution and is standard in spillover studies using similar identification strategies (Ginja/Karimi/Xiao, 2023). After defining the firm sample, I keep all workers employed in these establishments from 1995 to 2019four pre-announcement years for baseline trends and extending until all focal workers have reached ERA and NRA. These restrictions (sampling in 1998, at least one focal worker, private sector, and firm size 5500) and their impacts on sample size are summarized in Panel A of Table D2.

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 local workers. Panel B in Table D2 records the sample size after each sample restriction. The sample for 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.[18] Focal workers constitute approximately 10% of the total workforce on average (Panel A in Figure C7).

To assess how the selection criterion affects my sample, Table D3 and Table D4 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 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 chapter 2. As noted above, public sector establishments are absent in the data by construction.

[17] Public-sector industries are defined as 5-digit classifications beginning with 8485 and 99, based on 2008 classification of industries.
[18] Own calculations based on full employment biographies of focal workers.

3.2 Main outcome variables

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

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) the likelihood of an establishment has no employment without subsequent employment in future years, following (Huebener et al., 2024).

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 only aggregate them to years and firms.

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/Xiao (2023) and Bronson/Thoursie (2019), which rely on the relative real wage growth of individuals within a firm. [19] First, I deflate wages by consumer price indices in 2015, and, following the recommendation of Drechsler/Ludsteck/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. The difference between one's own and coworkers' wage growth shows the relative real wage growth. 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. I split these promotion variables by focal workers and coworkers (nonfocal workers). 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.

[19] 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 for these insights.

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 working hours are available only for certain years, and the only variable available throughout the years is the part-time versus full-time indicator. I control for part-time when accounting for promotions.[20] 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/Le Barbanchon, 2019); therefore, most of the wage effects captured likely reflect the promotions.

After defining the main treatment and outcome variables, such as individual-level hiring and promotion, among others, I aggregated them at the establishment level to observe the employment and wage dynamics of the entire workforce employed from 1995.

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 19521953 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 workeri.e., a woman whose retirement age was raisedon 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.

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

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 A2 for the graphical illustration of identification strategy). The resulting estimation strategy is generalized difference-in-differences.

I estimate a generalized Difference-in-Differences (DiD):[21]

$$y_{jt} = \alpha_j + \lambda_t + \sum_{\substack{t=1995 \\ t \neq 1998}}^{2019} \mathbb{1}\{year = t\} \left(\beta_t \cdot N\_TreatedFocal_j + \gamma_t \cdot N\_Focal_j + \zeta_t \cdot N_j\right) + \epsilon_{jt} \tag{1}$$

where y_{jt}- outcomes of interest (number of hired workers, number of promotions - of 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) 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_t, 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, 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, 19521953) 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 $\mathbb{1}\{year = t\}$ 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 1952 turns 60, till 2017 when she turns 65.

[21] 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.

Main reform and downstream effects. For the main reform period, I resample the firms with at least 1 focal worker in 2008. The challenge is that this design falls short of the ideal experiment for two reasons. First, firms with many affected workers may differ systematically from others in unobservable wayssuch as size, technology, or HR policiesthat are correlated with employment dynamics. Second, even conditional on observables, the composition of the workforce evolves endogenously over time. To restore the experimental ideal, I include firm fixed effects that absorb all time-invariant firm characteristics, and year fixed effects that capture macroeconomic shocks common to all firms. I additionally control for establishment age categories, East German location, broad industry groups, and baseline hiring levels in 2008. 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.

The corresponding identification resembles that of upstream effects:

$$y_{jt} = \nu_j + \mu_t + \sum_{\substack{t=2005 \\ t \neq 2008}}^{2020} \mathbb{1}\{year = t\} \left(\delta_t \cdot N\_TreatedFocal_j + \rho_t \cdot N\_Focal_j + \omega_t \cdot N_j + \xi_t \cdot X_j\right) + u_{jt} \tag{2}$$

This specification mimics the ideal randomized experiment: conditional on firm and year fixed effects and controls, the remaining variation stems from plausibly exogenous differences in exposure to the reform determined by historical workforce composition, not by contemporaneous decisions. In some parts of the paper, I run the same regressions at the firm-occupation level, adding the subscript c-occupations.

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 (i.e., different numbers of treated focal workers). While counterfactual post-treatment trends are unobservable, I assess this assumption by showing that event-study coefficients in pre-treatment years (19951997 for the upstream period and 20052007 for the main reform period) are statistically indistinguishable from zero.

I further support the identification strategy with falsification tests by estimating the results on placebo cohorts and placebo gender in chapter 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, I find crowd-out effects only within jobcells, while

spillovers across jobcells are absent (chapter 6), lending credibility to the causal interpretation.

Finally, Panel B of Figure C7 shows that, within the estimation sample, the share of workers born after the cutoff is close to 50 percent and varies smoothly across firms with different treatment intensities, consistent with quasi-random exposure to the reform.

Standard errors. I cluster the standard errors in equations 1 at the establishment-times-occupations level to address their potential correlation across workers in the same establishments.

Effect heterogeneity. The average effects that the Equation 1 estimates could hide substantial heterogeneity across different firm, industry, and/or labor market characteristics. I estimate the same regressions on the subsamples to study the effect of heterogeneity.

4 Intra-Firm Personnel Decisions

4.1 Upstream Effects

As discussed in chapter 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 A1. First, I present the upstream effects on focal worker retention, followed by spillovers on promotions, external hiring, and the wage bill.

Although Carta/De Philippis (2024) find significant upstream effects of employment, Rabaté/Jongen/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 C3 shows that establishments with more treated focal workers (19521953 birth cohorts) in 1998 retained more focal workers (19501953 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 C3) or in the number of external hires (Panel C in Figure C3). 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.

Firm closure. Panel D of Figure C3 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.

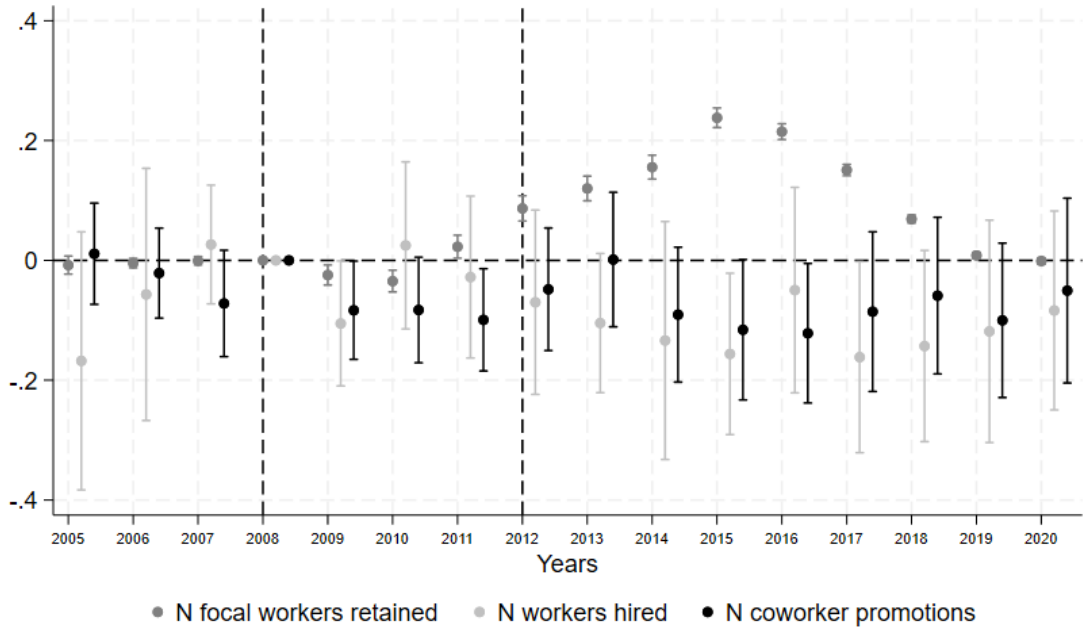
4.2 Main Reform Effects

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 during 2012–2017. When I aggregate over these main reform years, one additional treated focal worker in 2008 increases focal retention by $\Delta R = 0.163$ workers.[22]

[22] 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.

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 β_t 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 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.

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 retention 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.[23]

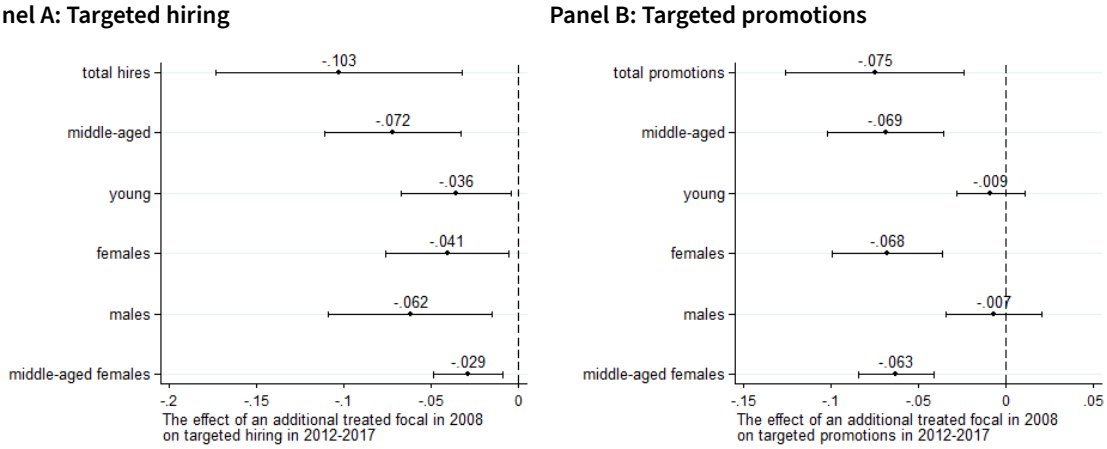
[23] Formally, this ratio is a reduced-form *substitution rate in headcounts* rather than a structural Hicksian elasticity of substitution, because both ΔY and ΔR are estimated in levels (numbers of workers). Under the approximation that the changes are small relative to baseline employment, this ratio 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.

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, capturing how promotion "slots are reallocated within a firm. Implied ratio 4.4 suggests that, for each additional promotion of an older focal worker, about 4.4 coworker promotions are forgone. This highlights that the reform does not simply keep older workers in place; it 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.

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 β_t in Equation 2 over 2012-2017, and the bars represent 95% confidence intervals. Standard errors are clustered at establishment level.

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 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 C6 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 womenmiddle-aged workersrather than those at the bottom of the age distribution.

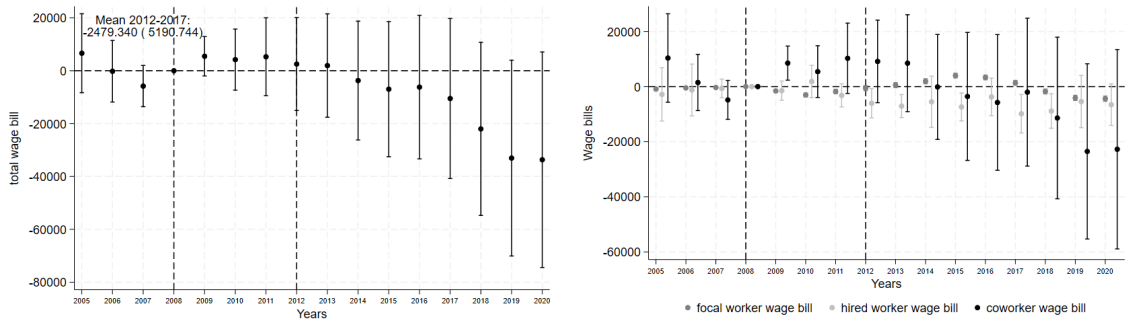
Panel B in Figure 3 reveals that crowd-out in promotions is concentrated among middle-aged and female coworkers, with much weaker responses for young and male coworkers. Combined with the hiring results, these findings point to compressed career ladders in the middle of the age distribution rather than 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.

Turning to firm closure, I estimate the impact of the reform on the probability of an establishment closure on Figure C4. 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.

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 β_t 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 establishment level.

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 womens 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 C5 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 section B1.

Falsification tests: placebo birth cutoff and gender. A concern is that contemporaneous macroeconomic shocks, such as the 20082009 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 Œ 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 C8 uses placebo birth cohorts (19521955), 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.

Robustness check: altering the estimation bandwidth. The baseline specification uses a two-year bandwidth around the reform cutoff. In Figure C9, I re-estimate the main results using a narrower one-year bandwidth. Focusing on cohorts born in 19511952 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 directlyindependent of firm samplingand 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 C2). The number of days worked (conditional on employment) is unaffected (Panel B of Figure C2), indicating that responses operate on the extensive margin. Panels C and D of Figure C2 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, 2025b; Geyer/Welteke, 2021). This supports the identifying assumption that sampling firms in 2008before any treated worker reaches pensionable ageis 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 interestmost 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/Xiao (2023) and Cortes/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 Kropp/Schwengler (2011).[24] 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 firm's ability to replace or expand its workforce and are therefore expected to amplify hiring crowd-out. Figure Figure C10 illustrates these patterns for motor vehicles and hospital activities.[25]

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

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

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

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

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

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

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

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 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, and the firm reallocates more strongly through promotions; in thick markets, adjustment occurs mainly through external hiring.

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 responses are muted ($dh/dr \approx 0$) and promotions account for most of the adjustment; in thick markets, hiring responses dominate.

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), then dh/dr is small and the observed crowd-out is attenuated relative to frictionless prediction. This understates true 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.

5.3 Results

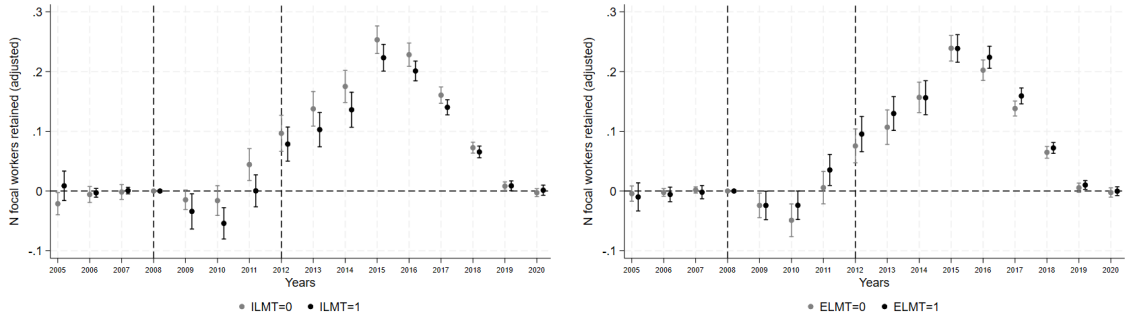
Focal worker retentions. Before analyzing the mechanisms behind crowd-out effects, it is important to understand how internal and external labor market thickness shapes the retention decisions of focal workers. Retention varies systematically with internal labor structure. Panel A in Figure 5 shows that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

These results align closely with Badalyan (2025b), which, using individual-level regressions, finds that internal substitutability strongly shapes labor supply responses to this reform, while industry-based ELMT generates little heterogeneity. That study also documents heterogeneity by occupation-based ELMT, showing that older workers in occupations with scarce external substitutes were more likely to be pushed into continued employment. Together with the firm-level evidence here, the pattern suggests 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 explain high retentions and low hiring rates for older workers found in the literature (Hutchens, 1986).

Hiring. The crowd-out of external hiring is highly sensitive to labor market thickness. In thin external markets, where firms face severe turnover frictions, hiring declines sharply (0.174). In thick external markets, the hiring response is much smaller (0.068). Because retention rates are nearly identical across ELMT groups, these differences imply that failing to account for external frictions would exaggerate the substitutability between older workers and external hires. What appears to be strong crowd-out is, in thin markets, largely the mechanical consequence of limited hiring possibilities rather than genuine technological substitution.

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 β_t in Equation 2 and the bars represent 95% confidence intervals. Standard errors are clustered at establishment level.

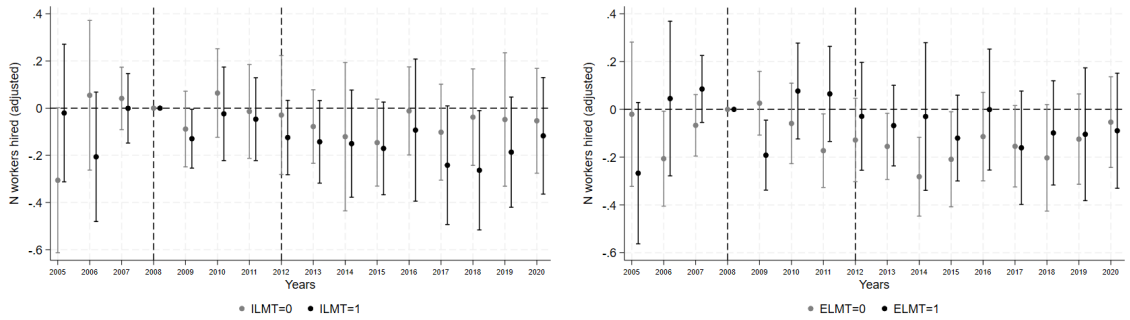
Internal labor market structure also shapes hiring responses, but in the opposite direction: hiring falls steeply when ILMTs are thick (0.923) and only modestly if ILMT is thin (0.081). When many workers share the same occupation, firms can adjust internally by diverting career progression rather than expanding the workforce. This yields very large hiring reductions relative to retention effects in thick ILMTs, producing an apparent substitution that is not purely technological but driven by internal hierarchy constraints.

Promotions. Promotion responses provide further evidence that internal bottlenecks mediate the impact of delayed retirement. In high-ILMT establishments, coworker promotions fall by 0.134more than five times the decline in thin ILMTs (0.026). Because focal-worker retention is similar across ILMT categories, this gap reflects tighter congestion along internal career ladders in thick markets. By contrast, promotion effects vary little across ELMTs (0.106 in thin vs. 0.057 in thick), reinforcing that promotion crowd-out is driven by internal rank constraints rather than external hiring frictions.

These patterns are consistent with Bertheau (2021), who shows that more than 60% 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. Without accounting for internal labor-market thickness, one would miss the fact that internal coworkers are closer substitutes for older women and therefore experience larger crowd-outs than external hires.

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

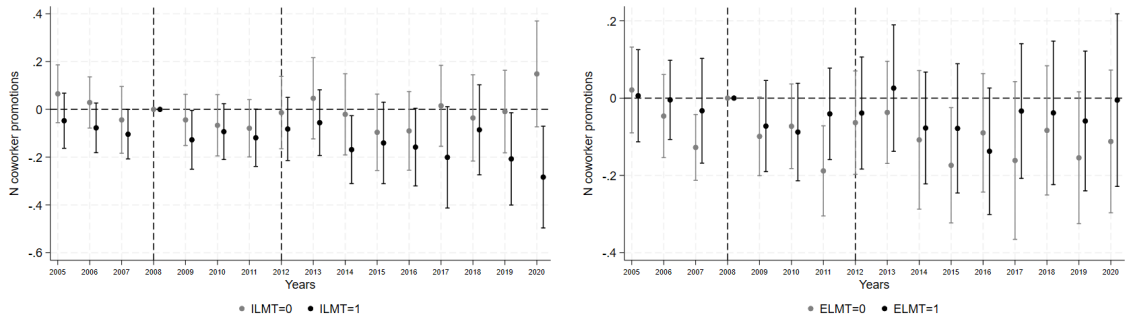
Panel A: Hiring by ILMT **Panel B: Hiring by ELMT**



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 β_t 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 establishment level.

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 β_t 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 establishment level.

Industry tradability. To further support the results on turnover frictions, I additionally analyze whether the firms in "more tradable industries" have smaller retentions of focal workers. Because production can be relocated across borders, tradable industries offer

greater scope for worker substitution through outsourcing than non-tradable sectors (Drenik et al., 2023). I classify the industries by tradability following Gregory/Salomons/Zierahn (2022).[26] Figure C11 shows that establishments in nontradable industries exhibit the largest increases in the retention of older workers, consistent with their more limited exposure to external competitive pressures. By contrast, the crowd-out of younger workers promotions and hiring is more pronounced in tradable industries. These patterns align with earlier evidence that external market conditions shape the adjustment margin: tradable industries rely more on external hiring and thus display stronger displacement when retention rises. At the same time, the heterogeneity across industries suggests that firm-level averages 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 retentions of focal workers are concentrated among the workers who are not substitutable internally, this could generate higher wage bills for firms, where ILMT is thin. The average effects of an aging workforce mask sharp heterogeneity in how firms adjust total labor costs. Panel A in Figure 8 shows that, in thick internal labor markets, the wage bill response is negative (though not significant): an additional treated focal worker reduces total wage bills by roughly EUR 11,415. This decline could reflect large reductions in coworker wage bills found in the previous section, consistent with compressed promotion opportunities and stalled wage growth in more competitive settings.[27]

Implications. Taken together, these results show that observed spillovers depend critically on market structureboth inside and outside the firm. This pattern maps directly into 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 follow.

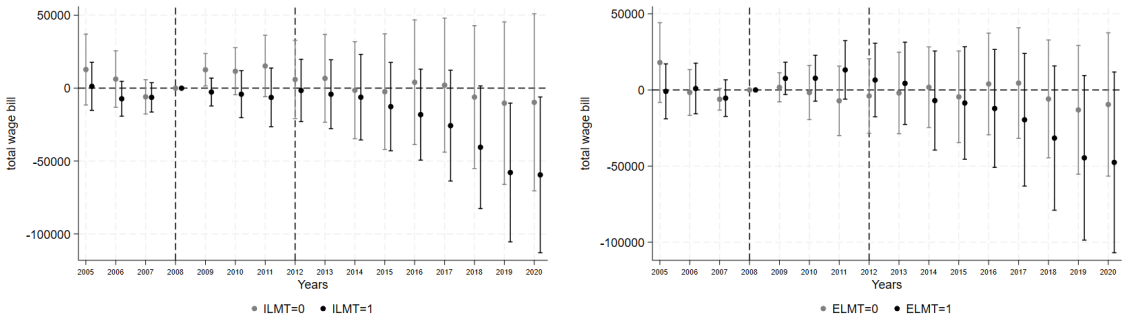
First, ignoring external labor market thickness overstates substitutability between older workers and young hires. In thin ELMTs, firms reduce hiring far more than in thick ELMTs despite similar increases in retention. This stronger hiring response reflects the limited availability of external candidates rather than a technological ability to substitute older workers for younger ones. Naive estimates that treat all hiring reductions as technological

[26] *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.

[27] 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), reflecting the limited pool of internal substitutes and the higher relative value of retaining experienced workers. External labor market thickness plays a smaller role: wage bill responses are near zero in thin external markets (EUR –35), but reach a sizable negative value (EUR –6,062) in thick external markets, where hiring adjustments are easier to implement, though they both are insignificant.

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 β_t 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 establishment level.

substitution, therefore bias the implied elasticity of substitution upward. In my data, ignoring ELMT inflates substitutability by a factor of roughly 2.8. This pattern corresponds to the model case in which turnover frictions suppress the hiring margin, mechanically increasing the share of adjustment allocated to other channels.

Second, ignoring internal labor market thickness understates the crowd-out of promotions. In thick ILMTs, delayed retirements substantially compress internal career progression (0.134), whereas the same reform generates almost no promotion effect in thin ILMTs (0.026). Averaging across firms masks the environments in which internal competition is most intense and promotion ladders are most congested. Analyses that do not condition on ILMT, therefore, understate the true career costs borne by workers in occupations with dense hierarchies. In terms of the earlier conceptual framework, thick ILMTs correspond to settings with high internal substitutability: when older workers stay longer, promotions become the primary margin of adjustment.

The evidence shows that the spillovers from raising retirement ages arise from 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 in one margin (hiring or promotion) reshapes the other: when external hiring is constrained (thin ELMT), promotions absorb more of the shock; when internal ladders are congested (thick ILMT), external hiring declines even when labor supply is plentiful. These interactions highlight that reduced-form crowd-out coefficients combine technological

substitution with adjustment constraints, and must be interpreted through the lens 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 establishmentoccupation 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).[28]

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/Morris, 2023), while others, using slightly larger firms, find positive impacts (Carta/DAmuri/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/Rosenqvist, 2019; Jäger/Heining, 2022), for example, due to human resource management systems (Holzer (1987) as cited in Schmutte/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

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

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/DAmuri/Von Wachter, 2024; Bianchi et al., 2023).

Firm size heterogeneity. A firms 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 reallocationvia promotions or transfers across jobcellsrather 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 adjustment margins are driven by differences in organizational depth or by within-occupation constraints operating inside jobcells.

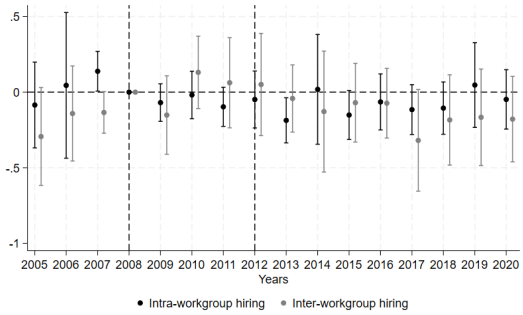
Figure C12 shows that establishment size also moderates firms adjustments to delayed retirements. Smaller firms exhibit larger increases in the retention of older incumbents, consistent with their more limited internal substitution possibilities. By contrast, crowd-out coefficients are noisier but tend 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.

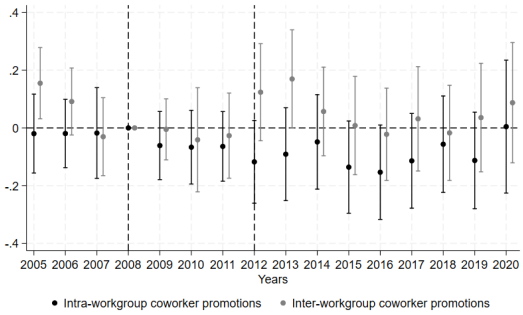
The results reveal that crowd-out operates primarily within the focal workers own jobcell. Panel A in Figure 9 shows that an additional treated older worker retained in 2008 reduces *intra-jobcell* hiring by approximately 0.086 workers, nearly twice the magnitude of the reduction in *inter-jobcell* hiring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

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 β_t 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 establishment level.

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/Heining, 2022; Schmutte/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.

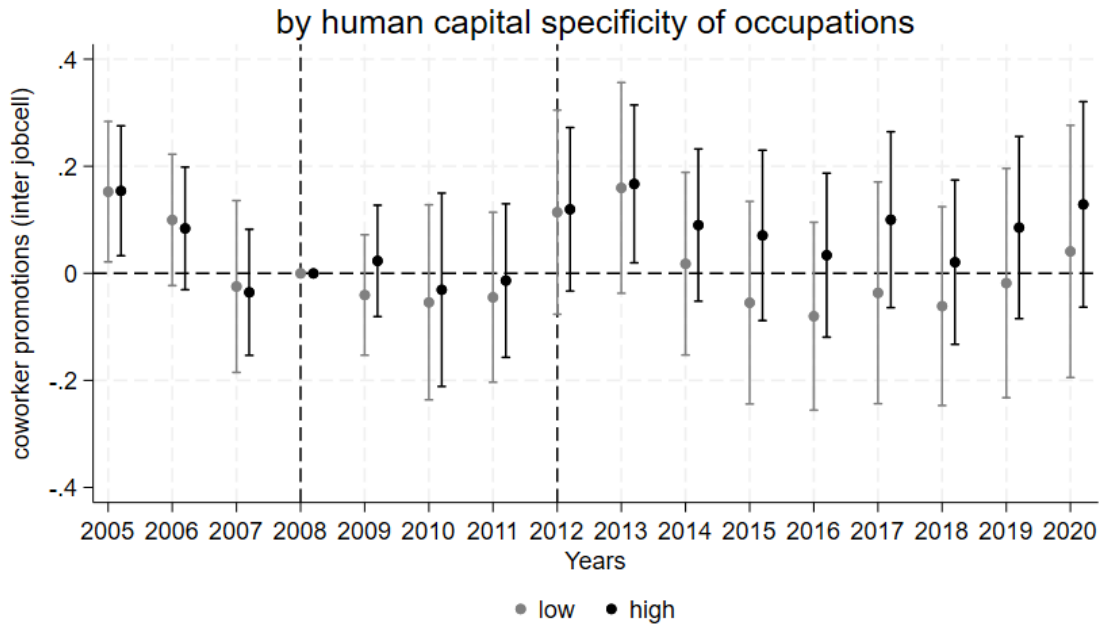
These patterns indicate that adjustment frictions are highly localized. Firms substitute most strongly among workers who share tasks, supervisors, or career ladders, foreshadowing the heterogeneities to come: the importance of internal labor market structure, firm-specific human capital, and bottleneck 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 replacesuch as managers and workers in occupations with strong job-specific skill requirementshighlighting the importance of firm-specific human capital and turnover frictions (Badalyan, 2025b). 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.

Figure 10: 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 β_t 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 establishment level.

Human capital specificity of occupations. Occupations differ by how much of a workers 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/Heining (2022) and Bleakley/Lin (2012) and estimate occupation-level Mincer regressions for each 3-digit occupation.[29] I use

[29] I run these regressions on a random sample of all the workers to classify occupations, and merge these classifications with my analysis data.

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 10 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.[30]

7 Conclusion

This paper exploits a large retirement reformabolition of the female pathway to early retirement in Germanyto 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 turnoverlosses 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

[30] 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 findingsnegative spillovers within jobcells but neutral or positive effects across jobcellsby 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 qualityfor example, in health and care professionswhere staffing shortages have been shown to harm patient outcomes (Friedrich/Hackmann, 2021). Finally, the mechanisms documented herecareer-stage substitution, internal bottlenecks, and frictions in external hiringare likely to be relevant in other settings where a component of the workforce is retained, and warrant further comparative study.

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Appendix

A1 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.

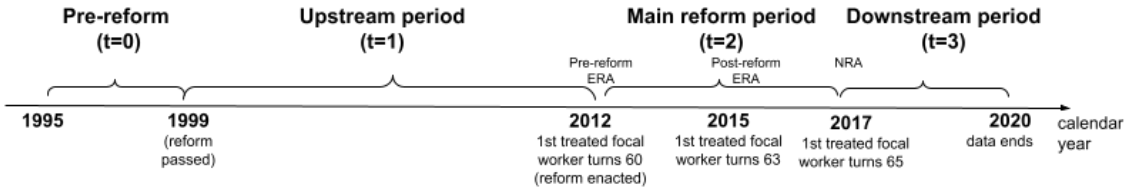
Timing of the 1999 reform. The 1999 Reform (*"Rentenreformgesetz 1999"*) abolished the women's pathway to early retirement. The reform was drafted in October 1997, and the affected cohorts were announced on December 17, 1997, through publication in the Federal Law Gazette; however, according to Etgeton/Fischer/Ye (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.

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/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/Ye, 2023). The reform became effective on January 1, 1999. The previous literature studying this reform, such as (Etgeton/Fischer/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/Welteke (2021) and

Badalyan (2025b) among others) use the pre-reform enactment year, close to 60 years old, which I use for the main reform period.

The Figure A1 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.

Figure A1: The reform timeline

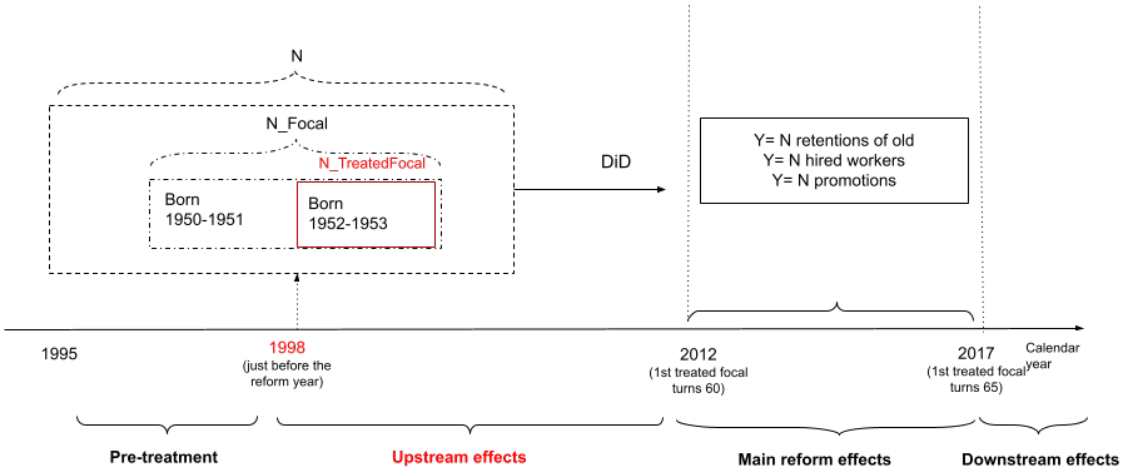


Notes: This figure represents the timeline of the reform. The upper section describes the periods, while the bottom part indicates the corresponding calendar years. For simplicity, the calendar years are written for the years of the ERA of the first affected cohort - the 1952 cohort (see Figure 1).

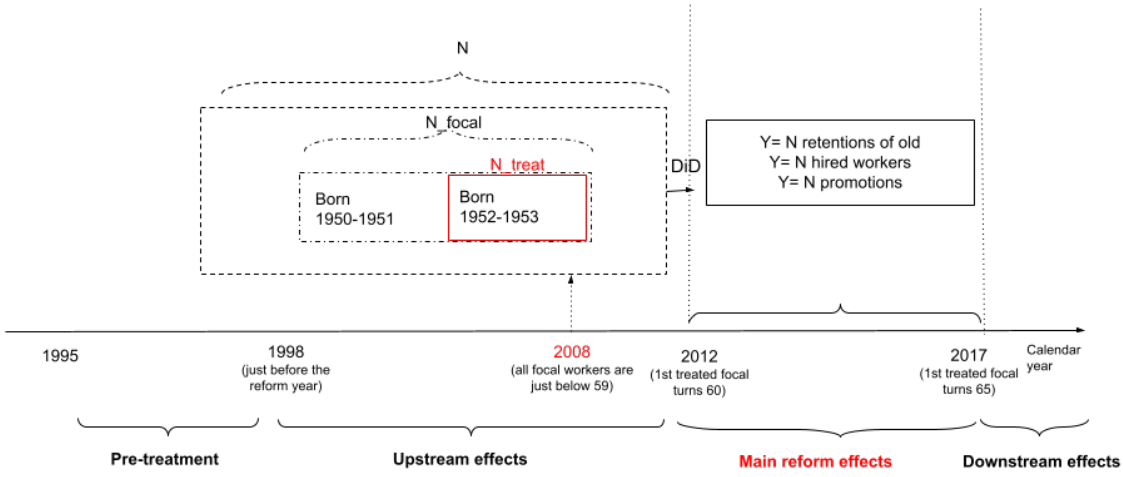
Illustration of empirical strategy. Below, I provide an illustration of the identification strategy in Equation 1 and Equation 2.

Figure A2: 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).

B1 Additional and/or Alternative Sample Definitions

As discussed in chapter 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 EmployerEmployee 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.

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 workgroups, are recorded in Table B1.

Table B1: 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

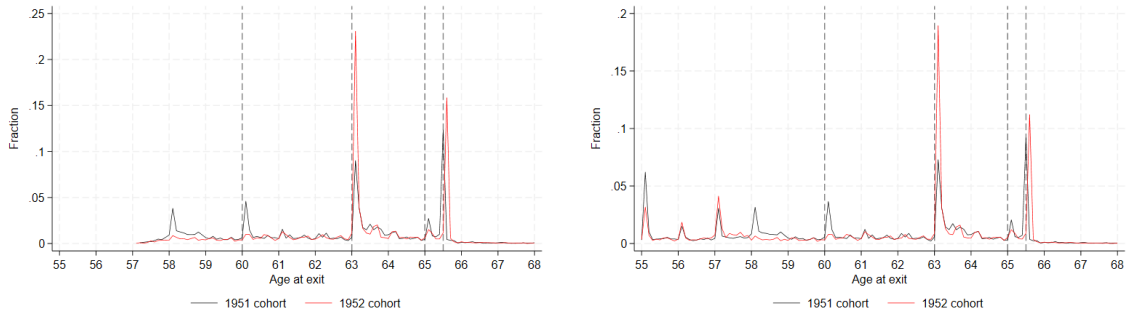
Notes: This table describes the number of establishments, jobcells, workers, and focal workers in the baseline (**Panel A**) and alternative samples (**Panel B**). For the details on baseline and alternative sample definitions, see chapter 3 and section B1.

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 19752018, 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.

C1 Appendix Figures

Figure C1: Retirement age distribution by birth cohorts

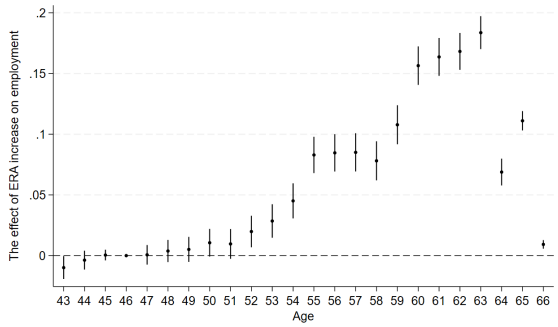
Panel A: Sample of workers employed at the age of 58 **Panel B: Sample of workers employed in 1998**



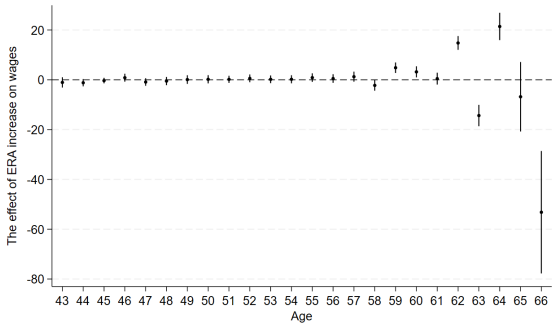
Notes: These graphs show the distribution of retirement ages. **Panel A** shows retirement ages based on focal workers employed at the age of 58. **Panel B** displays the distribution of retirement ages for workers employed in 1998. Both graphs are generated from the 2% random sample of IEB records.

Figure C2: Direct effects of the rise in ERA on employment, probability to become a new hire, or to separate from an employer

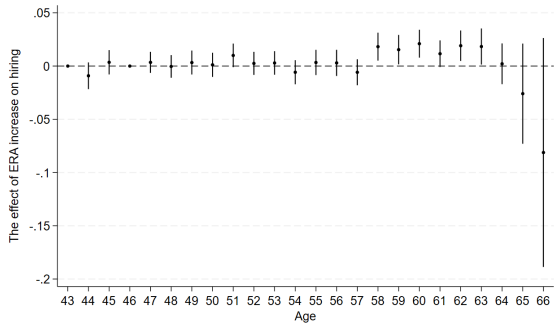
Panel A: Employment dummy



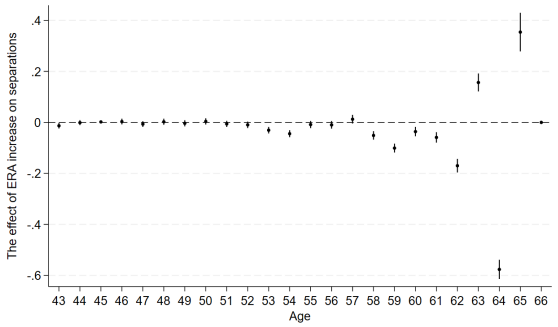
Panel B: No. of days employed at a given age



Panel C: Dummy for being a new hire



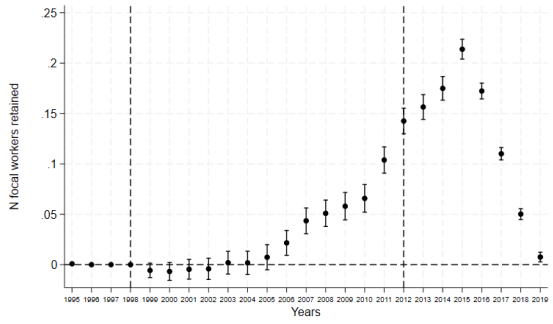
Panel D: Dummy for being separated



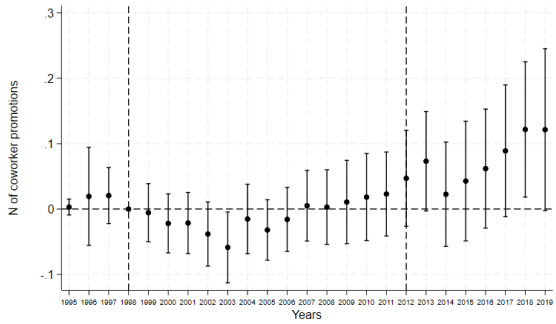
Notes: Coefficient plots. These graphs represent simple regressions of outcome variables on being born a year after the cutoff, separately for each age cohort. **Panel A**: employment dummy, **Panel B**: number of days employed at a given age, **Panel C**: dummy for being a new hire, **Panel D**: dummy for being separated. For sample construction details, see section B1.

Figure C3: 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

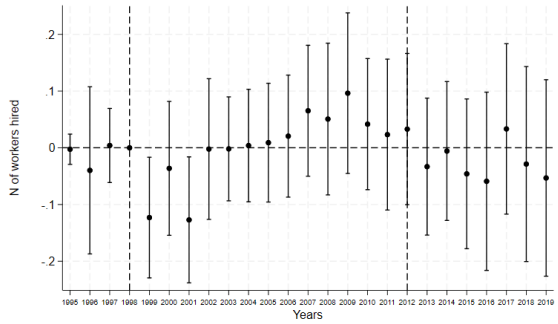
Panel A: No. of focal worker retentions



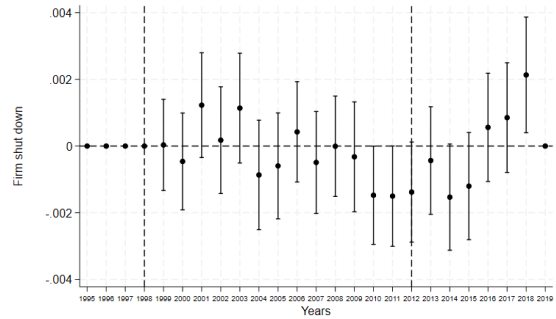
Panel B: No. of coworker promotions



Panel C: No. of hires

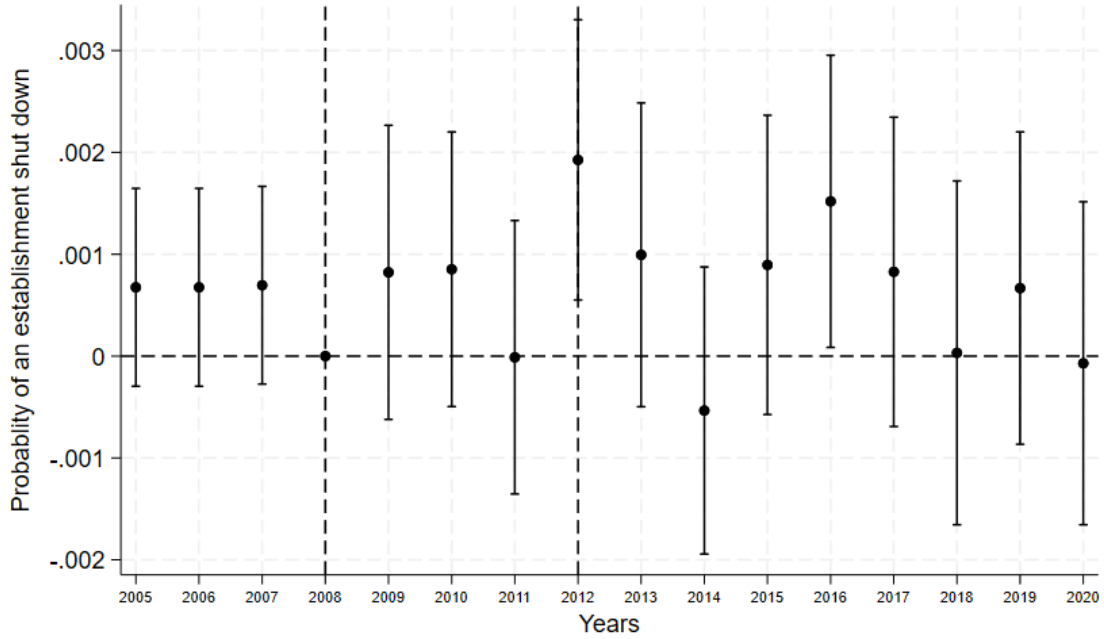


Panel D: Probability of a firm closure



Notes: This figure represents the effect of having one additional treated worker (19521953 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_t 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 establishment level.

Figure C4: The effect of an additional treated focal worker employed in 2008 on the probability of an establishment closure

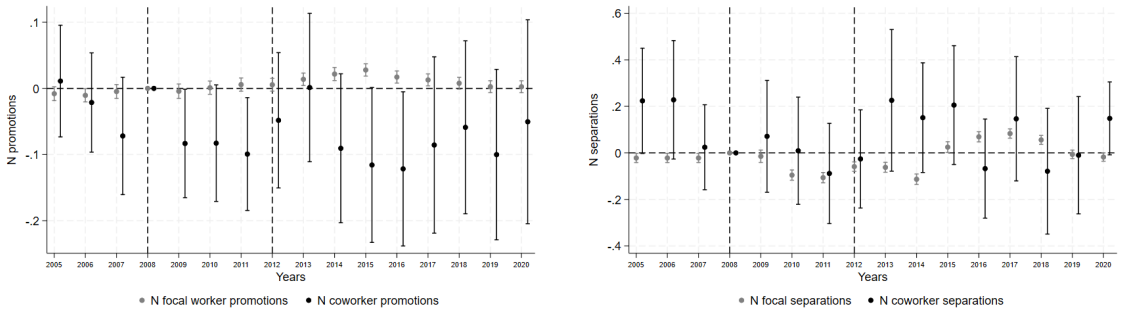


Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the probability of a firm closure each year. The points represent the estimated coefficients β_t in Equation 2 and the vertical bars represent 95% confidence intervals. The dashed vertical 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 establishment level.

Figure C5: 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

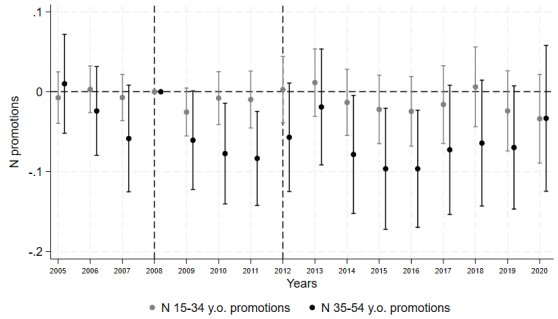
Panel B: No. of separations



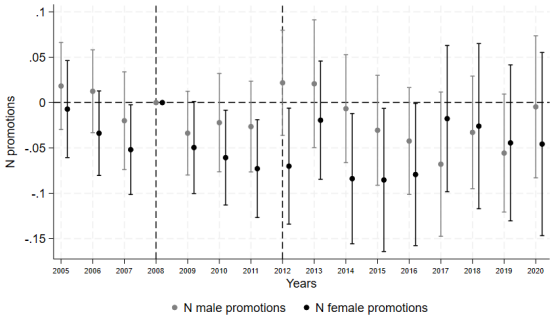
Notes: This figure represents the effect of having one additional treated worker (1952-1953 birth cohorts) in 2008 on the number of promotions (**Panel A**) and separations (**Panel B**) of focal workers (cohorts 1950-1953, in gray) and coworkers (in black) in each year. The points represent the estimated coefficients β_t 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 establishment level.

Figure C6: The effect of an additional treated focal worker employed in 2008 on coworker promotions, separations, and external hiring by age groups and gender

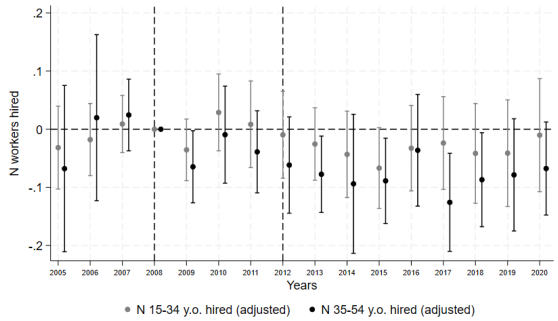
Panel A: No. of coworker promotions by age



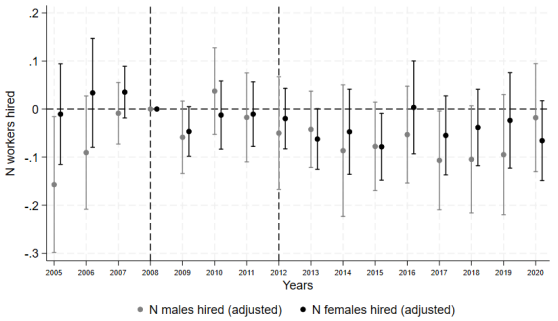
Panel B: No. of coworker promotions by gender



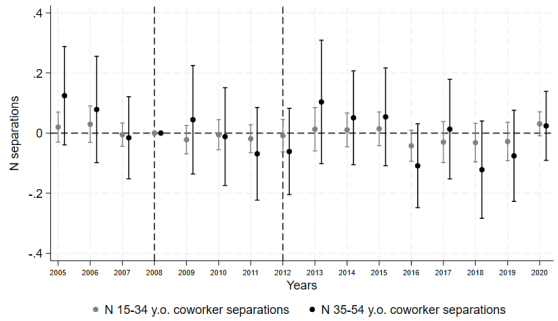
Panel C: No. of hires by age



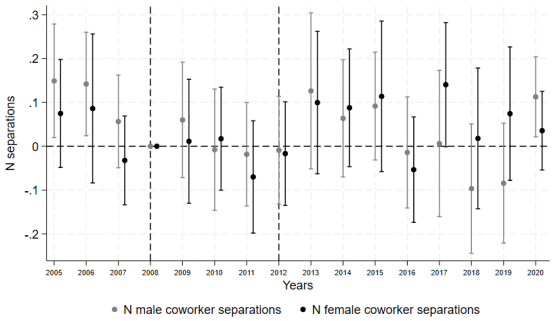
Panel D: No. of hires by gender



Panel E: No. of coworker separations by age



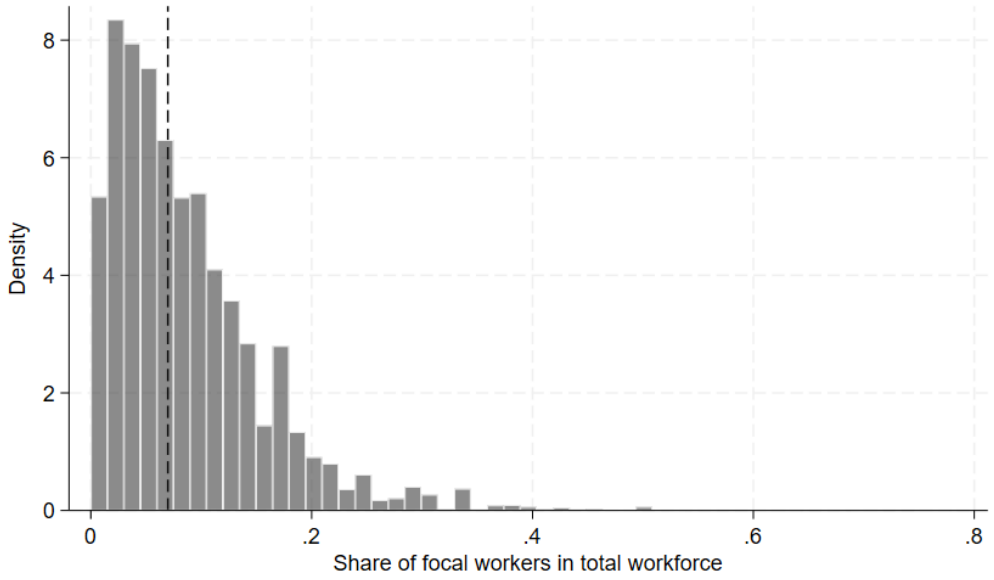
Panel F: No. of coworker separations by gender



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 β_t 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 establishment level.

Figure C7: Firm-level treatment variables

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

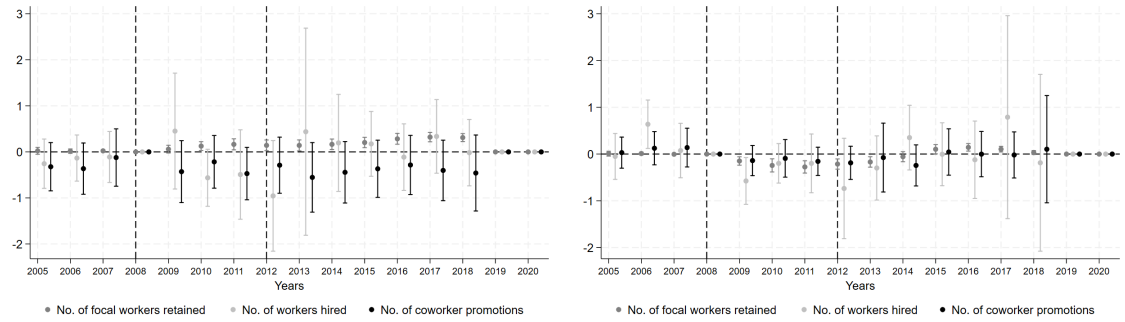


Panel B: Main treatment variables in 2008

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 C8: Falsification tests by birth cohorts and gender

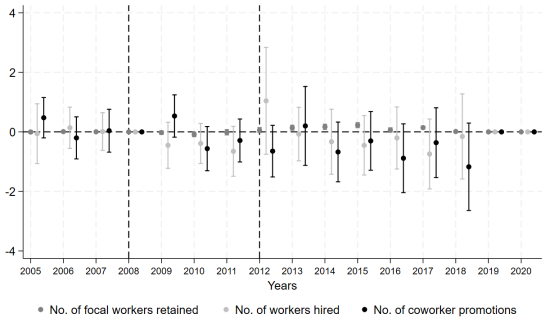
Panel A: Placebo birth cohorts (1952-1955, all treated)Panel B: Placebo gender (males)



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 β_t in Equation 2 and the vertical bars represent 95% confidence intervals. Standard errors are clustered at establishment level. For sample construction details, see section B1.

Figure C9: Robustness check: altering the estimation bandwidth around the 1952 cutoff

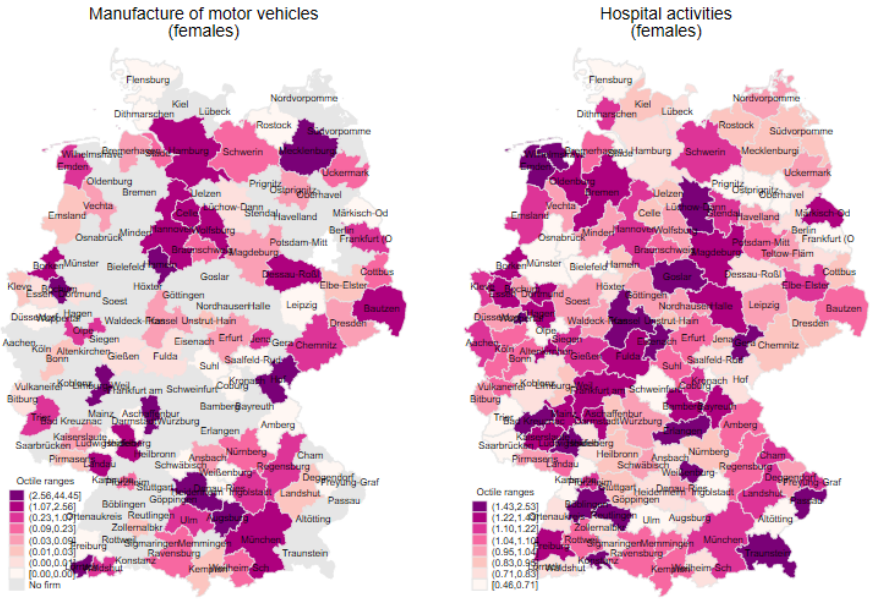
1-year bandwidth (1951-1952)



Notes: This figure represents the effect of having one additional treated worker (1952 birth cohort) 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 β_t in Equation 2 and the vertical bars represent 95% confidence intervals. Standard errors are clustered at establishment level. For sample construction details, see section B1.

Figure C10: Example of external labor market thickness in 2007

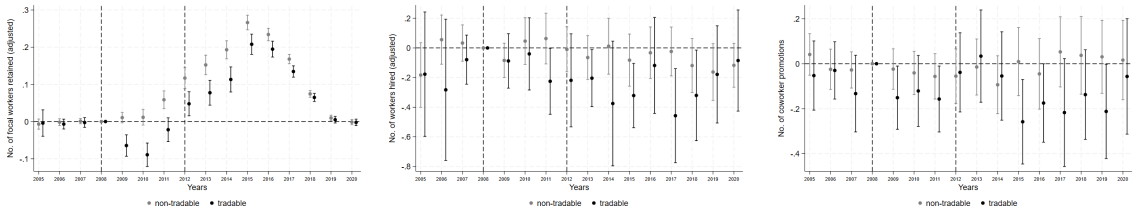
Panel A **Panel B**



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 Kropp/Schwengler (2011), 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 indexes on the map based on the eight quantile ranges (octiles) presented in the left corner of each graph.

Figure C11: The effect of an additional treated focal worker employed in 2008 on focal worker retentions, coworker promotions, and external hiring by industry tradability

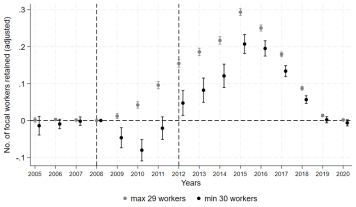
Panel A: No. of focal worker retentions **Panel B: No. of hires** **Panel C: No. of promotions**



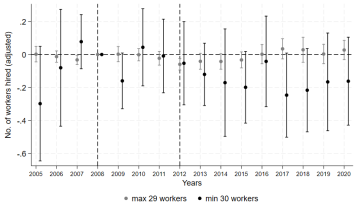
Notes: This figure shows the effects of an additional treated focal worker employed in 2008 on the number of retentions (**Panel A**), the number of external hires (**Panel B**), number of promotions (**Panel C**) by industry tradability. The points represent the estimated coefficients β_t in Equation 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 establishment level.

Figure C12: The effect of an additional treated focal worker employed in 2008 on focal worker retentions, coworker promotions, and external hiring by establishment size

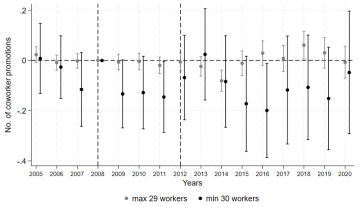
Panel A: No. of focal worker retentions



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**), 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 β_t 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 establishment level.

D1 Appendix Tables

Table D1: 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 D2: 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 D3: 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 chapter 3, while the sampling is described in chapter 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 D4: 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	30,296	131,592	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 chapter 3, and the sampling is described in chapter 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).

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