Early Retirement, Capital Adjustment, and Technology Adoption

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Abstract

The literature frequently characterizes older workers as less productive and obstacles to innovation, implying that their exit should free firms to invest in new capital and adopt new technologies. I show instead that the sudden loss of older workers can reduce firms' capital accumulation and delay technology adoption. Exploiting a 2014 German pension reform that allowed workers to retire up to 29 months earlier than expected as a natural experiment, I use linked employer–employee data to estimate firm responses to unexpected exits of highly-tenured older workers. I find that exposed firms reduce capital persistently and are less likely to adopt new technologies, despite continuing to hire younger workers. Effects are strongest in small firms, in firms with outdated capital, and in firms with little in-house training. To explain these results, I develop a stylized model in which older workers train younger workers and transfer uncodified, firm-specific human capital necessary for integrating and customizing new capital and technologies into firms' operations. The unexpected loss of older workers both reduces the productivity of existing equipment and raises the cost of adopting new capital vintages.

Keywords: Early Retirement, Labor Supply Shock, Firm Performance, Capital Adjustment, Technology Adoption, Workforce Composition, Pension Reform

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1. Introduction

In 2024, The Wall Street Journal described Boeing's urgent struggle to train thousands of inexperienced hires after the retirement of veteran engineers. "*Legions of senior machinists retired when the pandemic hit*," the article observed, leaving "*factories populated by new employees ... with no experience related to building airplanes*" (WSJ 2024). The Financial Times later reported that the outflow of senior technical staff undermined Boeing's ability to develop its next aircraft (FT 2024).

This stands in contrast with the prevailing view in the literature, which often portrays older workers as 'deadwood labor' (Saez et al., 2024) or 'has-beens' (MacDonald and Weisbach, 2004): Older workers' knowledge tends to become obsolete over time (Rosen, 1975; Deming and Noray, 2020), their incentives for further human-capital investment diminish with age (Ben-Porath, 1967) and learning to work with new technologies is more costly for those who already have experience with previous technologies (Jovanovic and Nyarko, 1996; Kredler, 2014). Passing on their obsolete knowledge to younger workers is proposed to even be harmful to firm's technological progress (Jovanovic and Nyarko, 1995), suggesting that their departure should allow firms to redirect resources toward technological renewal and capital investment.

I offer a contrasting view: I find that when experienced workers retire earlier than expected, firms reduce capital accumulation and slow technology adoption. This pattern appears to reflect a loss of firm-specific human capital (Becker, 1962; Topel, 1991). Part of this knowledge concerns the operation and maintenance of legacy equipment, so its loss leads firms to let existing capital depreciate. My findings suggest, however, that a less recognized and more consequential component involves the know-how required to integrate new capital vintages into the production infrastructure, as this process depends on tacit organizational knowledge embedded in routines, informal adjustments, and experience rather than written manuals and acquired through learning by doing (Atkeson and Kehoe, 2005).[2] My results indicate that older workers play a central role in sustaining

[2] For example, when a firm upgrades to a new version of an enterprise resource planning (ERP) system, the software must be customized and integrated with legacy databases, production tracking tools, and workflow protocols. Such integration often depends on employees who understand the firm's prior

existing production technologies and enabling firms to adopt new ones.

I study this question in the context of the 2014 German pension reform, which introduced a new pathway to early retirement without financial deductions for workers with at least 45 years of pension contributions. The reform applied to cohorts reaching age 63 from July 2014 onward and, as a side effect, allowed all eligible workers who had already turned 63, but had not yet reached the normal retirement age of 65 years and five months, to retire immediately without associated financial penalties. Because the legislative details were announced only in January and the bill passed in May, this generated a large, unanticipated outflow of older workers, compressing two and a half years of expected retirements into a single, policy-induced wave. Firm-level exposure to the reform was predetermined by the age composition of the workforce at the start of the year and corresponds to the proportion of employees newly eligible for early retirement, providing sharp quasi-experimental variation in labor supply. This abrupt loss disrupted not only the quantity but also the timing and age composition of separations, creating a natural experiment for identifying how sudden retirements of experienced workers affect firm behavior.[3]

I estimate firm responses using a dynamic difference-in-differences framework, which allows me to trace the evolution of treatment effects before and after the reform. The analysis draws on linked employer–employee data covering German firms and their workforces. This dataset combines administrative employment records with several firm-level surveys conducted by the Institute for Employment Research (IAB), providing detailed information on investment, technology use, revenue, and value added. The

configurations, informal workarounds, and interdependencies between systems. Similarly, in manufacturing, adopting a new machine tool may require calibrating its operation speed and data interfaces to match existing production lines, tasks that rely on experiential knowledge accumulated through prior coordination among machines and personnel.

[3] In contrast, worker-death shocks studied in the literature (e.g. Jäger et al., 2022) represent idiosyncratic, individual-level events. While each death is unpredictable, the aggregate incidence follows a stable demographic distribution and therefore does not shift the expected pattern of workforce exits. The early-retirement reform instead compressed a predictable sequence of retirements into a concentrated episode, altering the distribution itself.

sources include worker histories enabling the construction of a continuous exposure measure that weights eligible employees by the number of months they could retire earlier under the reform. Using this measure of firm-level exposure, I examine how the sudden wave of retirements affects firms' labor input, capital accumulation, technology adoption, and output.

The reform constitutes a sizeable shock to firms' stocks of highly tenured workers. Exposed firms experience a sharp rise in retirements in 2014 and 2015, followed by fewer retirements in 2016 and 2017, when the affected cohorts would otherwise have exited. Overall employment declines gradually, as firms respond with increased turnover (both hiring and separations rise relative to pre-reform levels) but total labor input settles at a lower level and does not recover in subsequent years. The share of employees with more than fifteen years of tenure remains persistently depressed. This persistent loss of tenured workers, rather than a direct contraction in overall labor input, represents a shock to firms' accumulated knowledge, routines, and firm-specific human capital.

Exposed firms exhibit an immediate decline in capital accumulation and a delay in technology adoption. The negative effect on capital is already visible by the end of 2014 and strengthens over time. Exposed firms have a higher likelihood of cancelling or postponing investment projects related to product or process innovation in 2014. In addition, workers at exposed firms report lower probability of introduction of new technologies at their workplaces between 2016-2020. These aggregate effects mask strong heterogeneity by firm size: the negative responses are concentrated among small and medium-sized firms with up to 60 employees, while larger firms show little adjustment. This heterogeneity cannot be explained mechanically by the relative size of the shock to the workforce, since the shock is defined in relative rather than absolute terms and aggregate employment effects are similar across firm sizes.

Apart from the effects on input choices, labor and capital, the shock also translates into output losses. Exposed firms reported revenue and value-added declines after reform. These effects emerge with a delay, consistent with the gradual impact of reduced capital accumulation and slower technology adoption. Exposed firms with below-median capital growth post-reform also appear to compensate for the loss in labor and capital by increasingly outsourcing tasks or relying more on purchased intermediate inputs.

This pattern aligns with a firm-specific human-capital channel in which the loss of long-tenured workers prevents the transfer of uncodified knowledge to younger workers, raising the costs of both maintaining existing and adopting new capital. I also find little support for alternative mechanisms. A raw *labor-quantity channel* where capital and firms scale down because firms cannot rehire contradicts the data: Employment declines only gradually rather than showing a sharp drop and rebound; turnover rises in subsequent years as additional employees leave; and firms continue hiring well after the initial retirement wave, inconsistent with strong hiring frictions. A *financial channel*, in which a lower cash flow due to output losses post-reform restricts investment, is also implausible: the immediate decline in capital and technology adoption predates the reductions in revenue and value added.

Taken together, my results suggest that tenure-related, firm-specific knowledge plays an important role, potentially more so than age-related productivity differences. If older workers were mainly an impediment to new technologies, their exit should have accelerated adoption rather than delayed it. Instead, capital accumulation and technology adoption slow down, consistent with the idea that the departure of experienced workers leads to a loss of accumulated know-how and disrupts organizational knowledge. This complements recent evidence from Liscow et al. (2025) showing that early retirement of engineers slows the completion and increases the costs of public infrastructure projects in the US due to loss of expertise.

To formalize this mechanism, I develop a stylized model of firm investment and technology adoption with firm-specific knowledge transfer before retirement. Workers enter as entrants, become incumbents, then retire. Incumbents embody firm-specific human capital: they train entrants to reach full productivity and lower the costs of adopting new capital vintages by carrying tacit knowledge about phasing out old equipment and integrating new technologies into existing production processes. When incumbents retire earlier than expected, entrants remain less productive and the costs of adopting new capital rise. The marginal product of capital falls, replacement becomes unprofitable, and adoption is delayed until larger productivity shocks arrive. The model yields two predictions: (i) firms exposed to unexpected retirements allow capital stocks to depreciate, and (ii) they postpone adoption of new capital vintages. Both match the empirical evidence.

For tractability, the model abstracts from firm-level heterogeneity. Nevertheless, the mechanism it highlights provides a useful framework for interpreting additional patterns I observed in the data. In particular, smaller firms may be more sensitive because tacit knowledge is concentrated in fewer workers. Firms with older capital stock may experience larger effects because newly hired workers are less familiar with existing, outdated technologies and depend more on incumbents for training. Firms with limited internal training may face greater disruptions from knowledge loss.

The paper contributes to four strands of the literature. First, it complements work on ageing and technology adoption. Much of this literature stresses disadvantages of older workers. Aubert et al. (2006) show that French firms adopting new technologies employ relatively fewer older workers. Barth et al. (2020) find that U.S. software investment raises earnings for prime-age but not older workers, while equipment investment benefits older workers more. Aghion et al. (2022) emphasize that invention rents accrue mainly to workers closer to the human capital frontier, disadvantaging older workers without recent training. My findings add nuance to this consensus: even if older workers are slower to adapt to frontier technologies, they remain indispensable in sustaining firm-level investment and easing transitions to new technologies. While younger workers may be better suited to operate new technologies because of low training costs (Lipowski, 2025), older workers embody routines and tacit knowledge that are essential for retiring outdated equipment and embedding innovations within established production processes. This role is largely overlooked in standard narratives that present younger worker as the sole drivers of technological advancement.

Second, it adds to work on how firms capital and technology responds to negative shocks to labor input. Clemens and Lewis (2022) study U.S. firms' responses to exogenous restrictions on low-skill immigrant labor created by the randomized H-2B visa lottery, finding that restricted access reduces investment, production, and profits. San (2023) studies the termination of the Bracero program, which abruptly cut off Mexican guest workers from U.S. agriculture. He shows that the resulting labor scarcity spurred persistent directed innovation, particularly in labor-saving technologies. My paper adds to this literature by examining a negative labor supply shock of experienced domestic workers. Older workers are not directly comparable to immigrants or younger workers, and their loss can have distinct consequences for capital and technology.

Third, it relates to research on how the supply of older workers affects firm outcomes. Work that studies the effect of sudden worker deaths documents high replacement costs and negative effects on firms' labor input in small firms (Jäger et al, 2022; Schivardi and Sauvagnat, 2022). Another strand of literature exploits pension reforms which usually *increase* retirement ages. However, the result of such a positive labor supply shock or forced expansion does not need to be necessarily symmetric to an unexpected loss as different mechanisms might be at play. Carta et al. (2024) analyze the 2011 Italian Fornero reform, which delayed retirements up to six years, and find that retaining older workers negatively affects the wages and careers of younger workers due to slot constraints in manager positions at the firm level. Boeri et al. (2022) study the same reform on a different set of firms and find a crowding out of middle-aged rather than young workers. Their theoretical framework assumes capital to be fixed in the short run and attributes the effect to middle-aged workers being closer substitutes for older workers than younger workers are. Hut (2024) studies a similar pension reform in the Netherlands that unexpectedly raised the retirement age. He finds that cash-constrained firms who had to retain older workers reduced investment because they had expected to cut wage costs by replacing them with less costly younger workers. Although the mechanism and the direction of the labor-supply shock differ, both his results and mine indicate that firms adjust capital even in the short run. This suggests that the standard assumption of fixed capital may be too restrictive and that models should allow for short-run capital responses to shifts in workforce composition.

Lastly, it connects to the literature on pension systems and pension reform. Much of this work examines how reforms affect individual retirement behavior (Staubli and Zweimüller, 2013; Manoli and Weber 2016). Dolls and Krolage (2023) and Felder et al. (2023) analyze individual workers participation of the 2014 German pension reform, whereas evidence on firm-level effects of this reform has so far been lacking. My results show that changes to the pension system can have unintended consequences on firms and potentially impact aggregate capital accumulation and technology adoption.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on the German pension system and the reform. Section 3 describes the data and key variable construction. Section 4 describes the empirical framework. Section 5 presents the main empirical result. Section 6 discusses potential mechanisms and introduces the theoretical framework. Section 7 concludes.

2. Institutional Background and the 2014 German Pension Reform

2.1. The German Pension System

Germany operates the world's oldest public pension system. It remains the main source of retirement income and is organized as a pay-as-you-go scheme, with current workers' contributions financing current retirees. Benefits depend on the number of contribution years and the accumulation of so-called earnings points, which are credited through pensionable employment and certain non-employment periods such as military service, child-rearing, or caregiving. The standard net replacement rate, the ratio of the pension of an individual with forty-five contribution years to the average gross earnings, was 48.9 percent in 2013. This corresponded to an annual pension of €13,612, equivalent to approximately US $14,700 and corresponded to a monthly pension of €1136. (DRV 2024).[4]

Before the 2014 reform, the statutory system was institutionally complex, but retirement behavior was nonetheless concentrated at two focal ages.[5] Retirement could be claimed flexibly in monthly steps once the *Early Retirement Age (ERA)* and 35 contribution years were reached. ERA was set at 63 and involved permanent deductions of the pension payout of 0.3 percent per month claimed before the *Normal Retirement*

[4] These numbers correspond to workers who have contributed their whole working life in West Germany compared to the 2013 salary of an average German Worker (including East Germans). East German pensions are treated with special conversion factors for contribution times in the former German Democratic Republic, so the German pension system does not report a comparable number. In 2013, the average pension for East Germany was 91.5% of the level of West Germany (DRV, 2024).

[5] See Börsch-Supan, Rausch and Goll (2020) for a comprehensive overview of changes in German retirement pathways from 1980 to 2020.

Age (NRA). NRA was 65, rising gradually in two-month steps to 67 for cohorts born 1947–1964 and later. In 2014 the NRA was at 65 years and 5 months. Additional routes existed for disability pensions (from 5 contribution years) and, for cohorts up to 1951, unemployment-linked pensions (from age 60 with 15 years of contributions). Despite this institutional flexibility, workers overwhelmingly coordinated retirement at the months they reached the ERA and NRA thresholds. Figure 1 shows the bunching of retirements at the exact ages 63 and 65 in 2013, while fewer exited at intermediate ages. Seibold (2021) documents this sharp bunching in administrative records and explains it as a behavioral response to the framing of 63 and 65 as focal retirement ages.

2.2. The 2014 German Pension Reform

On July 1, 2014, the German government introduced an additional pathway, the *old-age pension for the especially long-term insured*. The reform granted workers with at least 45 years of contributions the right to claim a full pension at the Early Retirement Age (ERA) of 63 without deductions. This represented a fundamental change: before 2014, claiming at ERA always implied permanent benefit reductions of 0.3 percent per month claimed before the Normal Retirement Age (NRA).

The policy applied to cohorts born after March 1949, who reached age 63 mid 2014. For these workers, the reform enabled exits up to 29 months earlier than under the previous law. Eligibility was effectively limited to individuals with uninterrupted employment histories, as accumulating 45 contribution years by age 63 required starting work before or at age 18. University graduates and workers with substantial career breaks were therefore not eligible.[6] For eligible workers this generated a large financial incentive to retire earlier. For the monthly birth cohort that just reached age 63 and was eligible to retire 29 months earlier, the approximate average lifetime financial gain from the lifting of the associated penalties in pension payouts amounted to EUR 25,244 (≈ EUR 32,056

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[6] Unlike the general 35-year requirement for ERA with deductions the 45-year rule did not include university education. Credited periods were restricted to employment, unemployment insurance, child-rearing and military service.

in 2024 or USD 34,620). This corresponded to about 73 percent of the year's average gross income (see Appendix A1 for details on the calculation).

The reform had two main effects on retirement behavior, as illustrated in Figure 2 Panel A, which plots the distribution of retirement ages by calendar year. First, it generated a permanent shift in the distribution of retirement ages: after 2014, age 63 emerged as a new focal claiming age, replacing 65 as the dominant threshold. Dolls and Krolage (2023) and Felder et al. (2024) analyse the effect of the reform on the average retirement age at the individual level and find the reform reduced average retirement entry between six to seven months.

Second, and more important for this paper, the implementation created a large one-time outflow in 2014. When the law came into force, all workers who had already reached ERA but had not yet reached NRA suddenly became eligible to retire immediately without deductions. This resulted in a concentrated large wave of retirements, which I exploit as a natural experiment to identify the effect of retirement shocks.

The reform process created little scope for anticipation by firms. Although the measure was part of coalition negotiations in late 2013, concrete details were announced only in January 2014, legislation passed in May, and implementation began in July. Firms had very little time to adjust employment structures or retirement planning in advance. As a result, the exposure of each firm to the reform was predetermined by its workforce age composition in early 2014, providing a quasi-exogenous shock to labor supply at the firm level.

The mechanics of this one-time shock are illustrated in Figure 3. Under pre-reform rules, firms with workers who had passed age 63 expected a steady flow of retirements at the NRA of 65 and five months, with each monthly birth cohort retiring in sequence.[7] After the reform, all monthly birth cohorts who had already passed the age of 63 could suddenly bring retirement forward without pension deductions, but the shift varied by

[7] For illustrative purposes, Figure 3 assumes that workers who have passed the ERA at age 63 would not have retired early before the NRA, for example at age 64 and 2 months. As Seibold (2021) shows this is a reasonable approximation because of bunching at ERA and NRA. I discuss this in more detail in Section 4.

birth month: some gained one month, others two months, and so on. On July 1, 2014, all these cohorts effectively became eligible at once, producing a discrete spike in retirements. In subsequent months, later cohorts also shifted forward, but in a more gradual and predictable way. Thus, only the 2014 implementation produced a truly unanticipated, firm-level shock.

3. Data and Descriptive Statistics

3.1. Data Sources

The main analysis relies on the linked employer-employee dataset LIAB-LM 7521 (Graf et al., 2024), provided by the Institute for Employment Research (IAB).[8] The LIAB combines survey-based establishment data with administrative worker data (Panahian et al., 2024).

The firm-level component originates from the IAB Establishment Panel, an annual survey conducted since 1993 that samples approximately 15,000 establishments per year. The survey covers firm turnover, investment, business development, personnel policies, and industry characteristics. Many questions are repeated annually, enabling consistent panel construction. Some ad hoc questions appear only in specific years.

The LIAB longitudinal model (LM) restricts the sample to establishments observed more than once during the 2012–2019 window.[9] Establishments not interviewed during this period are excluded. For the selected firms, all historical interviews back to 1993 are available. A major advantage of the Establishment Panel is its coverage of firms across all sizes and industries, conditional on employing at least one worker subject to social security contributions (i.e., excluding firms with only civil servants).

The worker-level component is based on the administrative employment register

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

[9] Technically, it also includes establishments observed only once if they report bankruptcy in the following year and do not fill out the survey.

(BeH), derived from employer notifications to social insurance institutions. Notifications are filed at the start and end of each employment spell, and annually for ongoing contracts on 30 June. The data are structured in daily employment spells. In the LIAB longitudinal model, I observe the full employment biographies of all workers who were employed at any panel firm for at least one day between 2011 and 2020.[10] Workers employed at panel firms before 2011 but not afterwards are not included.

I observe complete labor market histories of included individuals—both employment and unemployment spells—from 1975 to 2021, including spells at non-panel firms. Worker-level variables include daily data on wages, gender, occupation, birth month and year, education, employment type (full-time/part-time), nationality, and reason for deregistration. This allows construction of exposure measures based on worker age and tenure. One limitation of the data is the absence of hours worked. Civil servants and the self-employed are also excluded. These two groups represented under 15% of the workforce in 2019 and are excluded from analysis as I focus on firm-level outcomes for establishments employing covered workers.

One additional dataset is used for extended analysis, the Linked Personnel Panel (Mackeben et al., 2023), short LPP. It is also provided by the IAB and designed for personnel economics research. The LPP surveys a subsample of firms from the Establishment Panel in 2012, 2014, 2016, 2018, and 2020 using two parallel survey instruments. The first instrument is a survey of a single HR manager or senior executive per firm, covering human resource policies, training investments, digital technologies, and strategic planning. I refer to this as the *LPP-Executive Survey*. The second instrument surveys multiple individual employees within the same firm on topics including work conditions, technological change, career development, and preferences. I refer to this as the *LPP-Employee Survey*. The LPP provides detailed micro-level insights unavailable in the Establishment Panel, particularly regarding within-firm heterogeneity. Limitations are a smaller sample size and biennial frequency.

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[10] This is preferable to the LIAB cross-sectional model, which only contains information on workers present on June 30 as formal deregistration due to the retirement reform might already happen a few days earlier than July 1, 2014.

3.2. Key Variable Construction

3.2.1. Worker-level Eligibility and Firm-level Exposure

In order to quantify how much a firm is affected by the policy shock, I construct time-invariant exposure measures at the firm level. A potentially eligible worker is defined as a worker born between August 1949 and July 1951 (aged 63 to 64 and 11 months on July 1, 2014) who does not have a university degree. They are termed "potentially eligible" because I cannot observe full employment histories, particularly for East Germans, and the data exclude certain contribution sources that can count towards the 45 years threshold such as military service, child-care periods, or self-employment. University graduates are excluded because reaching 45 contribution years would require working from age 18, which is infeasible for those who attended university in Germany. It is important to note, that these workers should not be regarded as low-skilled. Within these cohorts, vocational pathways were the norm. Based on available census data for the cohorts 1948 and 1953, only 16% held a university degree, and approximately 9.5% obtained non-university advanced technical or master-craftsman qualifications in addition to their apprenticeships which provided established routes to managerial and specialized technical positions (Destatis, 2025).

I also compute an intensive margin of eligibility based on the number of months each worker could retire earlier on July 1, 2014. This is determined by birthdate and the difference between actual age and statutory retirement age. Grandfathering rules (e.g. for workers with partial retirement contracts before 2007) introduce minor uncertainty in the exact statutory retirement age. To ensure consistency, I adopt a conservative definition: I include the August 1949 to July 1951 cohorts and weight workers by their potential months of early eligibility. This captures substantial retirement shifts while excluding marginal cases where workers could retire only a few months earlier, which are unlikely to pose a significant operational shock to the firm.

In Figure 2 Panel B, I plot the median actual retirement age by monthly birth cohort for non-university graduates proxied by the last spell subject to full-social security contributions in the firm for workers that were age 62 to 66 at that time. The orange line shows the hypothetical age at retirement under full eligibility and full-compliance with the reform. Median retirement age, although noisily estimated, is relatively stable for cohorts before the reform. For the cohorts included in the exposure measure, median retirement age declines almost linearly in line with the predicted earlier eligibility. Starting with the January 1952 cohort, which could retire at 63, the median retirement age is flat at 63. In line with the stepwise two-month increase in the early retirement age for later cohorts, median retirement age rises again for the January 1953 and January 1954 cohorts.

Because the magnitude of early retirement eligibility matters for firms (losing a worker 12 months earlier differs substantially from losing one only 1 month earlier) I weight the exposure measure by months of early eligibility, assuming a linear effect.

The resulting potential exposure measure can be interpreted as an intention-to-treat measure: it reflects potential eligibility, not realized retirements. For each firm j, I count every worker i subject to full social security contributions on May 1, 2014 (two months before the reform), who is potentially eligible. I weight each eligible worker by their months of early eligibility. I then divide this sum by the total number of full social security employees in the firm on May 1, 2014, and rescale by 24. The division by 24 normalizes the exposure measure to a 0–1 scale, facilitating interpretation as a standardized share of the workforce potentially lost to early retirement.

$$E_j^{pot} = \frac{\sum 1\{Eligible_{ij}\} \cdot MonthsEarlier_i}{N_j^{total} \cdot 24}$$

where $1\{Eligible_{ij}\}$ is an indicator if worker i in firm j is born between August 1949 and July 1951 and has no university degree, $MonthsEarlier_i$ is the number of months that worker i can retire earlier. N_j^{total} is the total number of workers in firm j on May 1, 2014.

The resulting measure represents the share of the workforce that could exit unexpectedly within a two-year window due to the reform. The corresponding number is between 0 and 1 and represents the share of labor input measured in worker months in a two-year window that is at risk of loss to the firm as a result of the reform. Because retirement eligibility began uniformly on July 1, 2014, the exposure measure is fixed at baseline and does not vary over time. Actual retirements may occur later, but outcomes in the IAB Establishment Panel are observed at the earliest on 30 June each year. With exposure defined as of May 1, 2014, all outcomes are measured after treatment assignment, ensuring correct temporal alignment.

For robustness, I also define an actual exposure measure. This uses the same logic but counts only workers no longer observed in the firm one year later (May 1, 2015), still weighted by their potential months of early eligibility rather than actual retirement timing:

$$E_j^{act} = \frac{\sum 1\{Eligible_{ij}\} \cdot MonthsEarlier_i \cdot 1\{LeftFirm_{ij}\}}{N_j^{total} \cdot 24}$$

with $\{LeftFirm_{ij}\}$ being 1 if the worker is not observed in the firm as of May 1, 2015.

3.2.2. Estimation of Capital Stock

I construct firm-level capital stock using a modified perpetual inventory method (PIM), following Mueller (2008, 2017) and the updated 2024 Stata implementation provided by the IAB. The method combines firm-level investment reports from the IAB Establishment Panel with industry-specific depreciation parameters. Documentation and Stata routines are supplied by the IAB.

The Establishment Panel reports each year's total nominal value of gross investment (in Euros, or Deutsche Marks before 2002). It also includes indicators of whether investment occurred in four asset categories: buildings, IT and communication equipment, production machinery and other equipment, and transport equipment. These indicators are used in the PIM routine to approximate asset composition and calculate weighted average economic lifetimes. In the K3 version used here, only total investment enters the stock computation, with each firm assigned an industry-specific depreciation factor derived from the average lifetime of its capital stock based on its two-digit classification. Asset indicators improve precision in computing weighted lifetimes but are aggregated into a single capital stock measure. To initialize the stock, I follow Version II (K3) of the IAB do-file, which is designed for within-firm estimation. The initial stock is calculated as the average of three adjacent years of replacement investment, multiplied by the estimated economic lifetime. This yields consistent and stable capital stock series, particularly for short panels or firms with irregular investment histories. If investment data are missing in early years, initialization is delayed until a valid three-year window is available.

Although direct investment data are available, I do not use them as a main outcome. Investment is highly volatile and often zero in small and medium-sized firms. Capital stock smooths these fluctuations and better reflects durable adjustments in firms' input structures. As a robustness check, I also report results using the investment variable.

3.3. Sample Construction

I structure the data as a yearly panel of firm observations from 2010 to 2019. To construct the sample, I drop firms with implausible employment dynamics. I compute year-to-year percentage changes in the total number of employees, full-time employees, and marginal part-time employees relative to the prior year. If any of these changes exceed +1000% (a tenfold increase), the firm is dropped. In addition, I exclude firms whose total workforce declines by more than 90% in a single year, as such cases indicate near shutdown.[11] The sample is further restricted to firms with at least ten valid yearly observations for the capital stock variable within 2010–2019. Firms with fewer than 3 workers in 2013 are excluded, as the retirement of a single individual would constitute a disproportionately large shock, making comparisons with other firms difficult. Outliers in business volume and investment are removed by trimming the top 0.01% of the distribution. The effective target population is thus firms that remain in continuous operation, continuously invest, and for which a capital stock can be calculated.

My final sample consists of 654 firms with 131,616 workers in 2013, corresponding to about 0.3% of the total German workforce in that year. In total, I observe 5,886 firm-year observations from 2010 to 2018. Of these 654 firms, 374 have a potential exposure of zero and 280 have a potential exposure greater than zero, i.e. at least one worker identified as potentially eligible. Among firms with exposure greater than zero,

[11] Firms with workforce declines of more than 90% are excluded, as such drops are likely due to bankruptcy or liquidation. While it could theoretically be that a retirement shock affects a firm so severely that it triggers exit, this seems unlikely and is more plausibly driven by other factors that would confound the analysis.

the distribution of potential exposure ranges from 0.3% at the 10th percentile to 3.6% at the 90th percentile, with a median of 1.2%.

Table 1 reports descriptive statistics by exposure group. By construction, it is unlikely that very large firms have exactly zero exposure, since with a large number of workers it is almost certain that at least one is classified as potentially eligible. To account for this size effect, I also report a split based on very low but positive exposure (≤0.002).

Firms with exactly zero exposure are on average much smaller, with about 46 workers in 2013, while firms with any positive exposure average over 400 workers and show much larger capital stocks. Once zero-exposure firms are combined with firms with very low exposure, however, the differences become more moderate: firms with low exposure average about 193 workers and a log capital stock of 14.9, compared to 214 workers and a log capital stock of 16.3 for firms with higher exposure. The differences in capital levels primarily reflect differences in firm size. In the analysis, however, I consistently focus on percentage shocks to labor input (exposure) and corresponding relative changes in capital and other outcomes, so results are not driven by absolute firm size.

Exposed firms also have somewhat older workforces, with a higher share of employees aged 56–61 in 2013. This group is not directly affected by the reform that suddenly enabled early retirement from 2014, but they may still retire in the following years through regular or pre-existing early retirement channels. In contrast, workers aged 63–65 are those directly targeted by the reform. A second concern is that a high share of other older workers may in general affect firm outcomes, for example by slowing capital accumulation or technology adoption due to the age structure of the workforce. To address both issues, I explicitly control for the share of workers aged 56–61 in 2013 in my empirical specifications.

4. Empirical Framework

To estimate the causal effect of unexpected early retirements on firm outcomes, I implement an event-study difference-in-differences design exploiting firm-level variation in exposure to the 2014 pension reform. The unit of observation is the firm-year, with outcomes drawn from the IAB Establishment Panel between 2010 and 2018.[12]

The treatment variable is each firm's baseline exposure, defined as of May 2014. When using potential exposure (the share of workers potentially eligible to retire early), the estimand is an intention-to-treat (ITT) effect: the causal impact of facing higher eligibility risk, regardless of whether all eligible workers actually retired. This avoids bias from endogenous retirement decisions. As a robustness check, I also use actual exposure (the share of eligible workers who did retire by May 2015). Estimates with actual exposure are not a distinct causal parameter, since realized retirements are post-treatment, but they illustrate that results scale with retirement behavior.

Formally, the baseline specification is

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq 2013} \beta_k \cdot E_j + \sum_{k \neq 2013} \gamma_k \cdot Share56to61_i + \epsilon_{it} \qquad (1)$$

Where Y_{it} is an outcome for firm i in year t, α_i are firm fixed effects, λ_t are year fixed effects, and E_j is the continuous measure of potential or actual exposure defined prior to the reform. The coefficients of interest β_k trace the dynamic response of exposed relative to non-exposed firms, with 2013 as the omitted category. Control variables are the pre-reform share of workers aged 56-61 interacted with time.[13] Standard errors are clustered at the firm level.

Identification relies on the following conditions. (i) Exogeneity of reform timing: the pension reform was legislated and implemented within a six-month window and was

[12] Several Establishment Panel questions are asked retrospective for the previous year so including the 2019 survey wave yields information referring to 2018. Restricting to 2010–2018 ensures consistent timing.

[13] I control for the pre-reform share of workers aged 56–61 because exposed firms tend to have generally slightly older workforces, which could independently influence capital accumulation. This ensures that the estimated effects β_k capture the reform's impact rather than differences due to workforce age.

not anticipated by firms. (ii) Predetermined exposure: exposure is fixed as of May 2014 and thus orthogonal to subsequent firm decisions. (iii) No differential pre-trends: prior to 2014, high- and low-exposure firms evolved similarly across outcomes. (iv) No interfering policies: contemporaneous labor market reforms, such as the introduction of the federal minimum wage in 2015, did not differentially affect the close-to-retirement workforce targeted by the reform. The minimum wage primarily affected younger and low-tenure workers, whereas older full-career workers with 45 contribution years, who drive my exposure measure, were typically above the threshold. (v) Stable unit treatment value (SUTVA): retirements in one firm do not directly affect outcomes in other firms and there are no general-equilibrium spillovers. Older workers close to retirement rarely switch firms, and their human capital is highly firm-specific, which makes direct poaching across firms implausible. A potential concern, however, is that if many firms in the same local labor market simultaneously lose older workers, they may compete for a limited pool of younger workers. Such general-equilibrium effects could attenuate or amplify estimated responses. The design compares firms by exposure intensity, conditioning on firm and year fixed effects. This removes concerns about aggregate shocks to labor supply or investment. While potential spillovers across firms through elevated competition for younger workers in local labor markets increasing labor market tightness would not be absorbed by year fixed effects, I show in Section 5 that this does not seem to be a concern.

The primary outcomes are retirement shares, workforce age structure, capital stock, and indicators of technology adoption. Additional results cover employment levels, revenue, and organizational adjustments. To assess heterogeneity, I interact exposure with pre-reform firm characteristics such as size, capital vintage, and training intensity.

Because all firms faced the reform simultaneously, concerns about difference-in-difference designs with staggered treatment timing (Borusyak et al. 2024; Sun and Abraham 2021) do not apply. My event-study specification compares firms of different exposure levels before and after a common shock.

Treatment varies continuously across firms in a way that is predetermined and exogenous, as it arises mechanically from the pre-reform age distribution of employees. Recent work by Callaway et al. (2024) shows that difference-in-difference with continuous treatment is identified under a stricter, more generalized parallel-trends assumption. The strong parallel-trends assumption in a continuous treatment setting requires more than in the standard binary case. It does not only assume that treated and untreated firms would have had the same potential outcomes absent treatment. Instead, it requires that the average potential outcome for firms at dose d is the same as it would be for firms not assigned to d (but possibly assigned to another dose). This assumption cannot be tested directly and must be justified on theoretical grounds. In my context, violation would occur if a firm with a potential exposure of 2% reacted differently to a 2% dose than a firm that actually received 1% exposure. Put differently, violation would imply selection on gains into particular dose groups. Since my measure of potential exposure is an intention-to-treat and treatment intensity is driven by the random distribution of employees' birth months, there is no reason to expect such selection on gains, making the assumption plausible in this case. The coefficients should be read as reduced-form, linear approximations of the dose–response relationship between exposure and outcomes. It can be understood as the effect of the share of worker-years at risk of early retirement.[14]

Because the exposure measure captures potential rather than realized retirements, estimated effects represent lower bounds on the true firm-level impact of retirements. It assumes all workers with qualifying birth months have forty-five contribution years and retire earlier, so estimates capture an intention-to-treat effect: the impact of facing a higher share of potentially eligible workers. Before 2014, workers with forty-five contribution years could already retire at sixty-three with deductions; the reform removed these deductions. The reform thus altered the financial return to an existing option, affecting only compliers which are workers induced to retire earlier once deductions disappeared. Because these workers had already chosen to remain employed beyond sixty-three despite the availability of early retirement with deductions, it is unlikely that they would have retired earlier in the absence of the reform. As documented by Seibold (2021), there is

[14] As a robustness check, I also estimate an instrumental variables specification using the measure of potential exposure as an instrument for the realised retirement shares at the firm. The results are consistent with the reduced-form estimates presented.

strong bunching at the exact eligibility months for the early and normal retirement ages, in my case specifically at 63 years and 0 months, and at 65 years and 5 months, indicating that individuals typically retire precisely when they reach these thresholds. Thus, those already past 63 years and 0 months at the time of the reform are unlikely to have retired earlier without it, and the share of always-takers is negligible. The relevant behavioral margin lies among compliers, those induced by the removal of deductions to bring forward their retirement. Never-takers, those who remain employed despite being eligible, can reduce the estimated effect, potentially attenuating it toward zero.

The reform constitutes a *shock to the joint distribution of expected exits across age and time*. In a typical year, workforce separations occur gradually and are dominated by younger or mid-career turnover, while retirements among older workers follow a predictable, staggered schedule. The 2014 pension reform disrupted this pattern by suddenly increasing and concentrating exits among older employees who were already expected to retire in the future but not simultaneously and this early. Firms therefore faced an abrupt, policy-driven surge in separations at the upper end of the age distribution, a discrete shift in both the *timing* and *composition* of expected workforce outflows. This differs from shocks from unexpected worker-deaths studied in the literature (e.g. Jäger et al., 2022). Individual deaths are idiosyncratic and unpredictable, yet the *aggregate incidence* of deaths follows a stable, well-known demographic distribution. In that sense, worker deaths do not alter the expected pattern of separations: the firm knows that a (small) fraction of workers is expected to die each year, even if it does not know which worker. The early-retirement reform, by contrast, generated a systematic deviation from that expected distribution, transforming a gradual demographic process into a concentrated cohort-specific exit shock.

For some outcomes—both in the Linked Personnel Panel (LPP) and in selected questions of the IAB Establishment Panel—the survey design does not lend itself to a dynamic event-study specification. Many questions are asked retrospectively over a multi-year horizon or only appear in specific survey waves. For these outcomes I therefore estimate simpler regressions of the form

$$Y_{it} = \alpha_t + \beta_t \cdot E_i \quad + \gamma \cdot Share60plus2013_i + \sum_{t \neq 2013} \kappa_t (D_t \cdot Industry_i) \qquad (2)$$

Where Y_{it} is the firm outcome, E_i is the predetermined exposure measure, and controls include workforce age structure one year before the reform in 2013 and industry-by-year dummies. Standard errors are clustered at the firm level in the *LPP-Employee Survey* and are heteroskedasticity-robust in the *LPP-Executive Survey*.

These specifications identify reduced-form intention to treat effects of exposure on the relevant survey outcomes. While they do not trace dynamic adjustment paths as in the event-study design, the interpretation of the coefficients as causal effects under the same identification assumptions remains valid. The exposure variable is scaled between 0 and 1, so reported coefficients reflect the effect of a full-unit increase in exposure (from 0 to 1). To express the effect of a 1-percentage-point increase in exposure, the coefficient has to be multiplied by 0.01.

5. Empirical Results

This section presents the main results. I begin with labor input, where the reform initially exerts its effects, and then turn to capital and technology adoption, which are the central outcomes of interest. Finally, I document the effects on output measures, including revenue, value added, and profits.

5.1. Effect on Labor Input

Firms with higher exposure experienced a discrete, temporary surge in retirements immediately after the 2014 reform. Figure 4 Panel A presents the event-study of retirement share on potential exposure. Retirement rates increase sharply in 2014 and 2015, reflecting that workers who became newly eligible exited earlier than they otherwise would have. In 2016 and 2017 the retirement rate is correspondingly lower, as these workers would have retired in these years absent the reform. The magnitudes are economically large. The average retirement share in 2013 was about 1.1% of the workforce. The event-study estimates imply that a 1% increase in exposure raises the retirement rate in 2014 by 0.33 percentage points, about 30% relative to the baseline rate.

Moving from the 25th to the 75th percentile of exposure corresponds to an increase of roughly 1 percentage point in the full sample (or 1.6 percentage points among exposed firms). This raises retirements in 2014 by about 0.3–0.5 percentage points, equal to 30–45% of the baseline retirement rate.[15]

The reform-induced retirements also shifted the age composition of firms′ workforces. Figure 4 Panel B plots event-study coefficients from equation (1) with the average workforce age as the dependent variable. The estimates show stable pre-trends, followed by a discrete decline in average age starting in 2014. The effect reaches about –0.7 to –0.9 years at full exposure after 2015 and then stabilizes. At 1% exposure, this corresponds to a decline of roughly 0.007 to 0.009 years relative to the pre-reform mean of 44 years.

The reform primarily eroded the stock of long-tenured employees, revealing that its impact targeted the core holders of firm-specific knowledge rather than overall headcount. Figure 5 Panel A shows the effect on the stock of employees with more than 15 years of tenure. The effect is strongly negative and persistent, indicating that the reform caused a lasting reduction in long-tenured workers.[16] This result demonstrates that the shock affects not just total labor input, but a specific and potentially highly valuable component—workers with accumulated firm-specific human capital. Such workers embody intangible capital that cannot be replaced immediately, even in a frictionless labor market. Replacement of this form of labor occurs only gradually through experience accumulation, not instant hiring.

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[15] By construction, actual exposure (defined as the share of potential workers who retired by May 2015) is even more predictive of retirements than potential exposure (Appendix Figure A1, Panel A). I treat this as a robustness check, since actual exposure is post-treatment and therefore not my preferred causal measure. By contrast, the control variable—the pre-reform share of workers aged 56–61—only predicts gradual increases in retirements in 2015–2018, when these cohorts reached statutory retirement age (Appendix Figure A1, Panel B). The magnitudes are far smaller than the sharp, one-off spike in 2014–2015 driven by reform eligibility. This pattern shows that the exposure measure isolates the discrete, reform-induced retirement shock rather than simply capturing firms with older workforces.

[16] I obtain similar results for the stock of employees with more than 5 years of tenure (Appendix Figure A2 Panel A).

The reform's impact on total employment operates through gradual workforce reallocation rather than an immediate contraction, revealing adjustment frictions instead of pure labor shortages. Figure 5 Panel B shows the effect on total employment. The pattern does not follow a sharp downward adjustment followed by recovery as it could be expected from such a shock. Instead, employment remains stable at first and then declines gradually over time. This result is also robust to just using the number of full-time workers (Appendix Figure A2 Panel B). This indicates that the reform did not trigger an immediate labor contraction offset by subsequent hiring, but a slow, persistent reduction in total employment. Figure A3 in the Appendix shows the effect of exposure on annual hiring and separation shares. First, although the reform-induced retirement shock is economically meaningful, it remains small relative to normal workforce turnover. In the average firm in this sample, around 10% of employees enter and leave within a given year. By contrast, the exposure measure here corresponds to roughly a 1% potential reduction in labor input, so the direct quantitative effect is limited compared to ordinary churn. However, standard turnover mostly involves younger and more mobile workers, while mobility among older workers is much lower. Therefore, the nature of this shock differs qualitatively from normal turnover. If this were merely a raw labor-input shock, we would expect a clearer increase in leavers combined with a muted response in joiners if firms face hiring frictions. Instead, the data show that both leaver and joiner shares rise, albeit imprecisely estimated. Moreover, the increase is especially visible in 2016, after the immediate reform period. This pattern implies two things. First, firms do not appear to be completely constrained in rehiring and are able to attract new workers. Second, the simultaneous increase in both separations and entries suggests a form of organizational adjustment: firms may be reshuffling their workforce or replacing lost senior workers with new hires that do not fully substitute in productivity, prompting further turnover. This points to reallocation and replacement frictions, not a simple quantitative shortage of labor.[17]

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[17] This also speaks against the potential concern mentioned in Section 4 of a violation of the SUTVA assumption if spillovers from other firms in local labor market increased labor market tightness and prevented firms from rehiring.

5.2. Effect on Capital Stock, Investment and Technology Adoption

The retirement shock triggered a sharp and persistent slowdown in firms' capital accumulation, followed by weaker innovation and technology adoption. Figure 6 plots event-study coefficients from Equation (1) with log capital stock as the dependent variable. Pre-trends are flat up to 2013. Starting in 2014, exposed firms diverge downward, with the gap widening through 2016 and persisting thereafter. A 1% increase in exposure is associated with about 0.8% lower capital by 2016. To interpret this magnitude, consider the interquartile range (IQR) of exposure—that is, the difference between the 75th and 25th percentiles. In the full sample, the IQR is 0.93 percentage points, implying that moving from a low-exposure (25th percentile) to a high-exposure (75th percentile) firm is associated with about -0.8 x 0.93 = -0.7% lower capital in the medium run.

The decline in capital is concentrated among firms that experienced a greater share of retirements, consistent with the mechanism implied by their exposure. I estimate the effect on the change in log capital from 2013 to 2015 using two-stage least squares, instrumenting the change in the retirement share, measured as the difference between the average over 2014–2015 and the average over 2012–2013, with firms' scaled potential exposure.[18] The IV estimate (Appendix Table A1) implies a reduction of about 1.8 log points in capital per one-percentage-point increase in exposure which is somewhat larger than the baseline effect. Direct investment measures provide complementary evidence. I construct indicators for whether post-reform investment fell by more than 30% relative to pre-reform averages over a three-year window.[19] Regressions using actual exposure show that more exposed firms were significantly more likely to experience large investment cuts after 2014. Results based on potential exposure show a similar direction of effect, with a higher likelihood of investment cuts among more exposed firms, but the estimates are not statistically significant.

Taken together, the results provide two insights. First, they reject the standard

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[18] See Appendix A2 for the detailed empirical specification.

[19] Appendix A2 describes the construction of this variable and the regression specification.

assumption that capital is fixed in the short run. Capital adjusts immediately, while employment responds gradually.[20] Second, the direction of adjustment indicates complementarity between capital and highly-tenured labor. Firms do not replace retiring workers with capital; they reduce capital instead. The loss of incumbents lowers the effective return to capital, revealing that physical investment depends on firm-specific human capital embodied in long-tenured workers.

The aggregate pattern hides strong heterogeneity by firm size. Figure 7 shows that capital accumulation of small firms below 60 employees in 2013 contracts, while large firms' does not. In fact, for larger firms the coefficient even turns positive in later years despite not being significant. This suggests that adjustment capacity differs sharply by firm size. Smaller firms reduce capital when highly-tenured older workers unexpectedly retire, whereas larger firms seem to be not affected or may reoptimize and expand. Importantly this is not driven by a different response of the raw labor input measured as the total number of workers as shown in Appendix Figure A4.

The reform also slowed firms' adoption of new technologies. Table 4 shows that the unexpected retirements significantly increased the probability that planned innovations were not implemented. In 2014, a one–percentage–point increase in potential exposure raised the likelihood of canceling a planned product or process innovation by about 1.7 percentage points. As this effect is visible already in the reform year it indicates that already ongoing innovation activity was disrupted. These results suggests that the reform affected not only labor inputs and capital accumulation but also firms' ability to execute technological upgrades, consistent with the view that long-tenured workers carry tacit knowledge essential for the adoption and integration of new technologies.[21]

The reform also affected firms' medium-run technology adoption. Evidence from

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[20] In practice, some forms of capital can be bought or sold faster than labor can be hired or dismissed, implying that adjustment frictions are stronger on the labor margin.

[21] An event-study version of this specification (Appendix Figure A5) confirms the absence of pre-trends and shows a similar pattern around the reform year. Given that the outcome is a binary indicator observed biennially and refers to a one-time cancellation event, the linear probability specification is more suitable for summarizing the effect, while the event study serves as a complementary robustness check.

the LPP-Employee Survey shows that exposure reduced the likelihood that workers experienced the introduction of new technologies at their workplace. The survey asks whether a new technology had been introduced within the previous two years. This question appears only in the 2018 and 2020 waves, covering the periods 2016–2018 and 2018–2020. I pool both survey rounds and estimate Equation (2) with standard errors clustered at the firm level.

Table 5 reports the results. A one–percentage–point increase in potential exposure lowers the probability that a worker reports a new technology introduction by 4.2 percentage points; using realized retirements, the estimated effect rises to 6.5 percentage points. The effect emerges several years after the reform and persists through 2020, indicating a medium-run delay in technology adoption. The timing and persistence of this response are consistent with the sustained decline in the share of long-tenured workers documented above. Firms that lost experienced employees following the reform appear less able to implement new technologies, suggesting that tenure-related firm-specific knowledge remains a critical input for technological upgrading.

Additional evidence on input composition supports this interpretation. Among exposed firms with below-median capital growth post-reform, the share of externally sourced intermediate inputs rises between 2013 and 2018 (Appendix Table A2). This shift suggests that affected firms that slowed down capital accumulation increasingly relied on outsourcing rather than in-house production—consistent with the view that the loss of experienced workers led firms to abandon tasks requiring firm-specific know-how and to purchase them from outside suppliers.

5.3. Revenue, Value Added and Profits

The retirement shock also affected broader firm performance. Figure 8 Panel A shows that revenue declined with a lag, becoming pronounced only two years after the reform in 2016–2017. Value added, measured as the difference between revenue and intermediate consumption, follows a similar pattern, declining from 2017 onward (Figure 8 Panel B). Profitability, measured using a binary indicator equal to one if the firm reports a profit and zero otherwise, declines after the reform (Table 6). For each year from 2014 to 2017, I estimate linear probability models of reporting a profit on exposure, including a control for prior profitability in 2013.[22] The results show a short-run deterioration: exposure has no effect in 2014, turns sharply negative and statistically significant in 2015, and remains negative thereafter. Because the outcome is a binary indicator and the estimates are year-specific, the magnitude and precise timing should be interpreted with caution. Nonetheless, the pattern indicates a clear weakening in firms' profitability following the unexpected exit of workers.

6. Mechanism and Theoretical Framework

The key empirical result is that firms reduced capital accumulation and delayed technology adoption immediately after the unexpected wave of retirements. The timing of this response indicates that the reform altered not only the scale of production but also the process through which firms renew and upgrade their productive assets. Understanding this requires identifying the mechanism that links the loss of older workers to firms' investment and innovation behavior.

The results point to a mechanism operating through the destruction of firm-specific human capital rather than a loss of raw labor input. Following the reform, exposed firms experienced a sharp increase in retirements among long-tenured employees and a persistent decline in the stock of workers with more than fifteen years of tenure. Capital accumulation fell in the same year, while total employment declined only gradually. The slow adjustment was accompanied by higher hiring and separations over several years after the reform, indicating continued labor turnover rather than an immediate inability to rehire. Declines in revenue and profits appeared only later. The sequence and persistence of these adjustments imply that the shock affected the composition of labor rather than its aggregate quantity.

One mechanism consistent with these patterns is that long-tenured workers embody tacit organizational knowledge essential for sustaining existing capital and integrating new vintages of technology. Their abrupt exit likely disrupted internal knowledge transfer, reduced the effective productivity of replacement workers, and increased the cost of maintaining or upgrading capital. As a result, firms postponed or

[22] See Appendix A2 for details.

canceled investment projects, delayed technology adoption, and relied more heavily on externally sourced intermediate inputs. Taken together, these patterns suggest a process of organizational knowledge loss through which the sudden retirement of experienced workers lowered the effective return to capital and raised the cost of technological renewal.

The next section formalizes this mechanism in a model of firm investment and technology adoption under the unexpected loss of incumbents.

6.1. A Model of Firm Investment and Technology Adoption with Knowledge Transfer

I develop a stylized two-period model of the firm to illustrate how unexpected retirements can affect firm investment and technology adoption. It builds on the basic intuition of capital vintage models of Chari and Hopenhayn (1991) and extensions by Jovanovic and Yatsenko (2012) and Kredler (2014). In my framework old workers embody firm-specific human capital and transfer this knowledge to young workers before retirement. The key novel assumption is that this firm-specific human capital does not only enable young workers to better operate older machines (old capital vintages) but also lowers the cost of adopting new technologies (new capital vintages).

Environment. Time is discrete. A firm consist of N production units. Each unit requires one worker and one unit of capital.[23] Each worker lives for two periods. In the first period she is an entrant, in the second she is an incumbent and then she retires. After she retired a new entrant replaces her at her production unit. The firm operates N units and output is produced as

[23] By production unit I mean the smallest indivisible worker–capital combination that produces output. In manufacturing, this could be a machine operated by a single worker; in retail, a staffed cash register; in services, a workstation with specialized equipment.

$$Y_t = \sum_{l=1}^{N} Y_{lt} \qquad Y_{lt} = A_{vlt}(K_{vlt})^{\alpha}(L_{lt})^{1-\alpha},$$

where K_{vlt} is capital of vintage v, A_{vlt} its productivity, and L_{lt} the efficiency units of the assigned worker. Final output Y_t is a linear aggregator of unit input Y_{lt}. In this model total factor productivity A_{vlt} is vintage-embedded: it captures the performance of the specific generation of capital equipment such as a software version, machine tool series, or vehicle model year, and the production routines associated with it, rather than a disembodied technology shifter.

Training

Entrants can be trained without costs by incumbents present in the firm between the period the entrant enters and the incumbent leaves. If entrants are trained, they produce with full efficiency $L = 1$. If no incumbent is present, then they produce with lower efficiency $L = \theta < 1$ for the two periods. Training can be interpreted as the transfer of firm-specific human capital: knowledge of the firm´s production routines, workflows, and how capital vintages interact with them.[24]

Capital vintages

Each period, a new capital vintage that embodies a higher total factor productivity becomes available on the market $A_{v+1} > A_v$. To adopt a new vintage, the firm must pay a fixed cost for deinstalling the old equipment, installing the new one, and integrating it

[24] This mechanism mirrors the anecdotal evidence from Boeing mentioned earlier, where managers acknowledged that the firm's traditional system of peer-based learning collapsed once experienced employees became a minority. As Boeing's quality chief Elizabeth Lund explained, 'Traditionally we counted on our on-the-job training and this peer mentorship. But with this lower percentage ratio of experienced employees, it really made that more difficult for employees.' Executives later admitted that they 'didn't realize the extent of the knowledge loss until after the accident,' and the company ultimately sought 'to hold on to experienced workers and continue using their expertise even after they can no longer tolerate the physical demands of the job' (WSJ 2024).

into ongoing production. This cost depends on the presence of incumbents. Incumbents hold firm-specific knowledge not only about the outgoing vintage but also about the operating procedures and interdependencies that link machines, software, and workflows within the production process. Much of this knowledge is non-codified—embedded in practical routines, informal adjustments, and experience rather than written manuals. Because capital equipment rarely operates "off the shelf," it must be adapted to a firm's established processes, layouts, and control systems. Incumbents lower adoption costs by knowing how to phase out the old system, calibrate and test the new one, and align it with existing routines. When these workers retire without passing on their tacit knowledge to younger employees, the firm must rediscover or outsource these integration steps, raising the cost from f to $f' > f$.[25]

Profit and firm decisions

Per unit profit under vintage v is:

$$\pi_v = A_v K_v^{\alpha} L^{1-\alpha} - (r+\delta) K_v - w$$

where r is the rental cost of capital, δ the depreciation rate and w the wage.

Replacement rule

If the firm keeps vintage v, it maintains its capital stock only if the marginal product of capital covers the rental rate and the depreciation:

$$MPK \quad = \alpha A_v K_v^{\alpha-1} L^{1-\alpha} \geq r + \delta$$

If this condition fails, the firm finds it unprofitable to replace depreciated units, so capital gradually declines at rate δ. Assume that $\delta \ll \theta$.

[25] Formally, one can model vintage productivity as a stochastic process where a new vintage arrives each period but the size of the productivity gain is random. For instance, let $A_{v+1} > A_v(1+\epsilon_t)$ with $\epsilon_t \geq 0$ drawn from i.i.d. from a distribution $F(\cdot)$. Note that this means that the firm might not introduce a new vintage every period because new vintages might only be slightly more effective than the old one but over time as the productivity gains accumulate and the likelihood of a large increase in productivity increases, adoption becomes more likely.

Adoption rule

Adoption of vintage $v + 1$ is profitable if the productivity gain exceeds installation cost:

$$\pi_{v+1} - \pi_v = (A_{v+1} - A_v)K^\alpha L^{1-\alpha} - f(o) \geq 0$$

where $f(o) = f$ if incumbents are present and $f(o) = f'$ otherwise.

If the firm adopts the new vintage $v + 1$, per-production unit profit is

$$\pi_{v+1} = A_{v+1}K^\alpha L^{1-\alpha} - (r + \delta)K - w - f(o),$$

Reform shock

In steady state, entrants are trained ($L = 1$,), so adoption requires $(A_{v+1} - A_v)K^\alpha \geq f$. Assume now that due to an unexpected retirement reform, some incumbents retire earlier than expected by the firm.[26] Their premature exit interrupts the transfer of firm-specific knowledge that normally occurs before retirement: (a) entrants on their production units are not fully trained, and (b) the tacit know-how required to phase out old capital vintages and integrate new ones is not yet passed on. In normal circumstances, younger workers could perform these tasks once trained; with early retirements, this preparatory transfer is cut short. As a result, entrants operate at lower efficiency $L = \theta$ and adoption cost rise to f'.

Proposition 1 (Capital Replacement Margin)

Unexpected retirement lowers the effective labor units of entrants from 1 to θ. The marginal product of capital falls to $MPK = \alpha A_v K^{\alpha-1}\theta^{1-\alpha}$. If this falls below the user cost $r + \delta$, the firm stops replacing depreciated capital, and the capital stock declines gradually at rate δ.

Proposition 2 (Technology Adoption margin)

Adoption of new vintages occurs when the productivity gain $(A_{v+1} - A_v)$ exceeds installation cost. With incumbents present, adoption requires $(A_{v+1} - A_v)K^\alpha \cdot 1 \geq f$. With incumbents absent, the condition becomes $(A_{v+1} - A_v)K^\alpha \theta^{1-\alpha} \geq f'$. Because

[26] We can think of the earlier retirement taking place between the periods, reflecting the relatively short nature of their earlier retirement of in some cases a few months.

$\theta^{1-\alpha} < 1$ and f' the adoption threshold is strictly higher. Adoption is therefore more likely to be delayed until a large enough vintage productivity shock arrives.

6.2. Heterogeneity in Firm Responses

These two margins of adjustment generate intuitive implications for heterogeneity across firms. Firms differ in their capacity to buffer the loss of incumbents depending on (i) how firm-specific knowledge is distributed across workers, (ii) how easily it can be transmitted, and (iii) at what point in the employment cycle that transmission occurs.

The first dimension concerns **firm size**, which shapes the internal distribution and redundancy of knowledge. Smaller firms depend more heavily on individual workers whose tacit know-how is not easily substitutable. When such firms lose a given share of their workforce, a larger fraction of firm-specific human capital disappears because knowledge is concentrated in fewer individuals. Larger firms, by contrast, might distribute similar shares of expertise across more employees and rely on more standardized procedures, which limits the relative impact of an equivalent proportional loss. As seen in Figure 7, smaller firms experienced a pronounced and persistent contraction in capital following the reform, whereas larger firms displayed no comparable decline. This empirical pattern is consistent with the interpretation that the concentration of firm-specific knowledge in few individuals amplifies the effect of unexpected retirements in smaller organizations.

The second dimension relates to the **technological age of the firm's capital stock**. Firms operating older or less modern equipment before the reform faced greater potential for disruption. In such firms, production relied on idiosyncratic, experience-based routines that were difficult to codify, and close-to-retirement workers typically possessed essential knowledge about operating and maintaining these legacy systems. When these workers retired early, the cost of upgrading to newer vintages increased sharply because younger workers lacked familiarity with how old and new technologies interact. Firms that had already modernized their capital depended less on this tacit expertise and were therefore less exposed to disruption.

As illustrated in Figure 9 Panel A, firms reporting outdated capital in 2013 show larger post-reform declines in capital stock than firms with more modern equipment.

Although the estimates are imprecise, the direction of the effect is consistent with the model's prediction that the loss of incumbent-specific know-how raises the effective installation cost f' for firms relying on older vintages.

The third dimension concerns **training intensity**, which determines how effectively knowledge is transmitted before incumbents retire. Firms that provide extensive in-company training establish systematic channels for transferring skills and routines from experienced to younger workers, reducing the productivity gap between entrants and incumbents. Where training activity was limited prior to the reform, this transmission was weaker, leaving firms more exposed when older workers exited unexpectedly. In the model, stronger training mitigates the decline in effective labor efficiency θ following retirements.

As shown in Figure 9 Panel B, firms with low training intensity in 2013 experienced a more pronounced and persistent contraction in capital after the reform, whereas those with higher training intensity adjusted far less. Although these estimates are again imprecise, their pattern is consistent with the interpretation that active training attenuates the productivity loss associated with unexpected retirements.

6.3. Alternative Channels

The interpretation advanced so far emphasizes the loss of firm-specific human capital as the main mechanism through which unexpected retirements reduced capital accumulation and slowed technology adoption. Two alternative explanations, a pure labor-quantity adjustment or financial constraints, could in principle generate similar aggregate patterns but are inconsistent with the observed dynamics.

A purely raw labor quantity mechanism would imply that firms scaled down capital mechanically after losing workers and failing to rehire. Figure 5 Panel B shows that total employment does not fall sharply but instead declines gradually over several years, while Figure A3 in the Appendix documents simultaneous increases in both hiring and separations. These offsetting flows indicate that firms remained active in labor-market adjustment rather than being unable to recruit replacements.

Additional evidence supports this interpretation. Small exposed firms exhibit higher subsequent turnover and notably higher dismissal rates among newly hired workers in their six-month probationary period.[27] This pattern could suggest that replacement workers were hired but did not match the productivity or knowledge of retirees and were consequently dismissed, reflecting a qualitative rather than quantitative labor shortage. The persistence of capital and technology effects alongside continuing hiring thus rules out a frictionless labor-supply explanation.

A second possibility is a financial channel, implying that the decline in capital accumulation reflects financing constraints triggered by reduced internal liquidity.[28]

In principle, such a mechanism would require a fall in revenues or profits that restricted investment funding. Yet in the data, this timing does not align. Figure 8 shows that revenue and value-added decrease only with delay, and profitability declines briefly in 2015 but then recovers, while capital accumulation contracts already in 2014. Moreover, most investment is financed either externally through debt or with retained earnings (that would in this case have been accumulated before the reform). This implies that a contemporaneous profitability shock would affect future investment only with a lag. In addition, such a binding financial constraint would also require a sudden tightening of access to external finance, but the reform was unrelated to general credit conditions. The financial channel is therefore best viewed as a secondary amplifier, which could have potentially modestly reinforced adjustment once profits weakened, but not as a primary cause of the immediate response.

Taken together, the sequence and timing of employment, capital, and profitability adjustments indicate that the reform's effects did not mainly operate through labor-supply shortages or financing frictions. The evidence instead points to a qualitative mechanism: the loss of firm-specific human capital that lowered the effective productivity of capital and raised the cost of technological renewal.

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[27] For German firms, firing a worker with more than 6 months of firm tenure becomes considerably more complicated and costly as dismissal protection laws apply. Firms therefore have strong incentive to end bad matches in the first 6 months.

[28] Hut (2024) discusses a related channel in the context of a Dutch pension reform that increased pension ages and forced firms to keep paying high wages to incumbents they expected to replace with less costly entrants, which lead cash constrained firms to delay investment.

7. Conclusion

This paper studies how the sudden retirement of experienced workers affects firms' capital and technology adoption. Exploiting the 2014 German pension reform as a natural experiment, I show that firms exposed to unexpected retirements reduce their capital stock and delay adoption of new technologies. These effects are strongest in small firms, in firms with outdated capital, and in firms with little in-house training.

I develop a stylized model to rationalize these findings. In the model, incumbents raise the productivity of entrants through training and facilitate technology adoption by assisting with deinstallation and integration of new vintages. Their unexpected loss lowers the marginal product of capital and raises adjustment costs. This mechanism explains the observed decline in capital and the delay in technology adoption.

The findings overturn the view of older workers as "deadwood labor" for technology adoption. Instead, they show that older, high-tenured workers are a key productive resource needed for firm-level investment and technological upgrading. The results also indicate that capital adjusts quickly to labor shocks, implying that short-run interactions between labor and capital are stronger than typically modeled.

These results have several implications for research and policy. Standard models of labor supply shocks abstract from short-run capital responses and may thus mischaracterize firm dynamics. Future work should incorporate the joint determination of age, tenure, and capital structure to capture the mechanisms revealed here. At the same time, large firms' apparent resilience suggests heterogeneity in adjustment capacity that warrants further study. For firms, the results highlight the cost of losing tenured workers and the importance of safeguarding against sudden knowledge loss. Continuous training and early technology upgrading can strengthen resilience to demographic shocks. For policymakers, the findings show that pension reforms and flexible early-retirement schemes can have unintended consequences for firms by increasing uncertainty and eroding firm-specific knowledge. Incentives that allow or encourage continued employment after pension eligibility could help preserve these knowledge stocks and mitigate the adjustment burden on firms. Overall, the evidence shows that sudden retirements are not neutral: they can slow the diffusion of technology by removing the workers who carry the organizational knowledge needed to adopt it.

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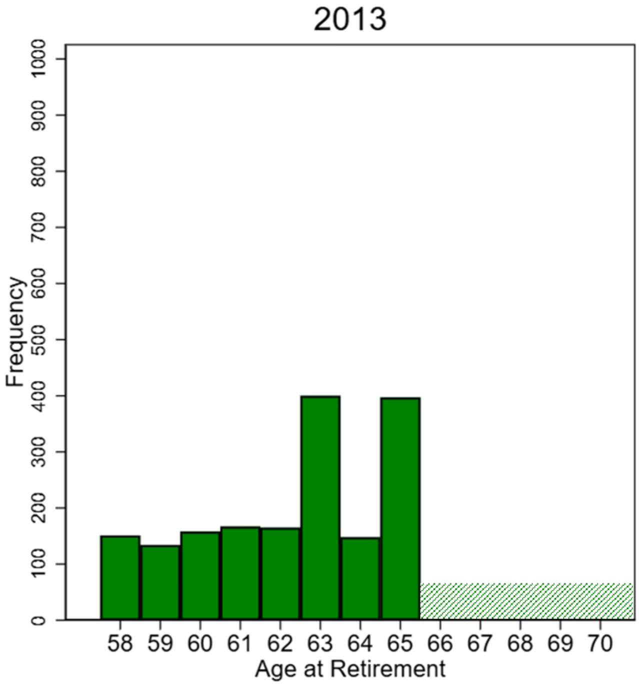
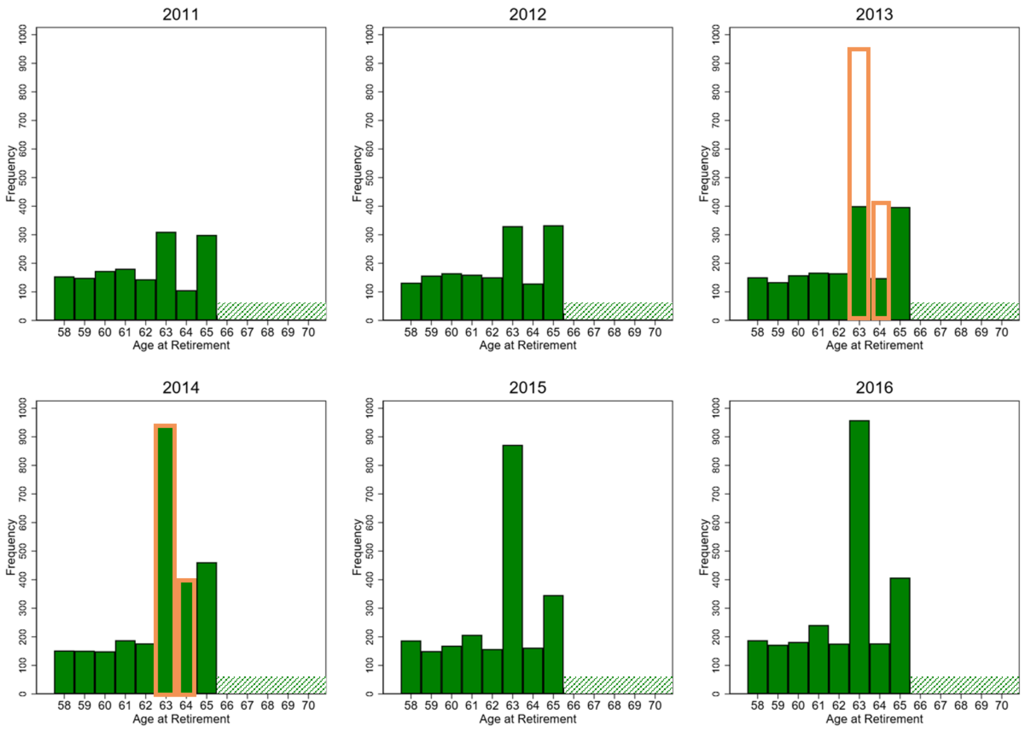
Figures

Figure 1 - Distribution of Retirement Age in Year 2013



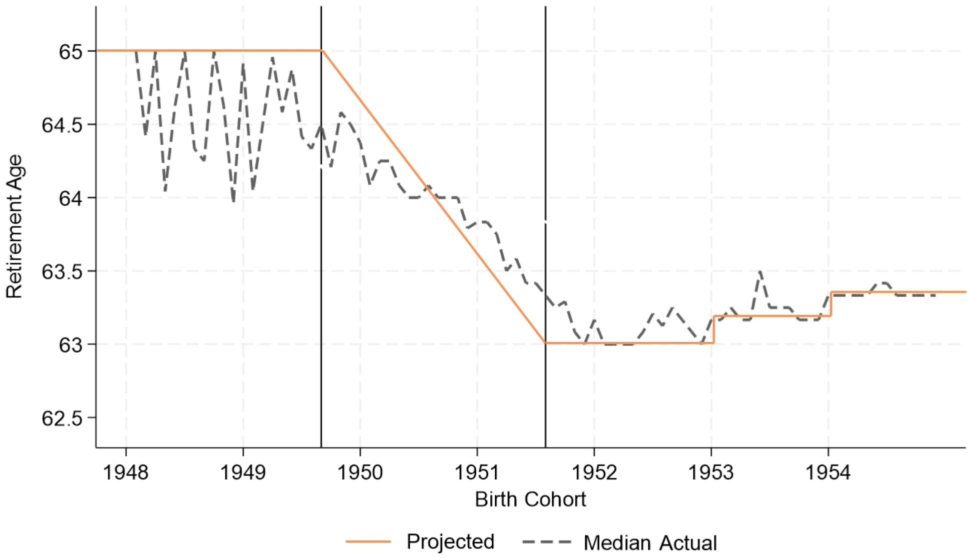
Note: This figure shows the distribution of entry into retirement by age at year 2013.
Source: IAB staff calculations

Figure 2 - Age at Retirement



(a)

Age at retirement per calendar year

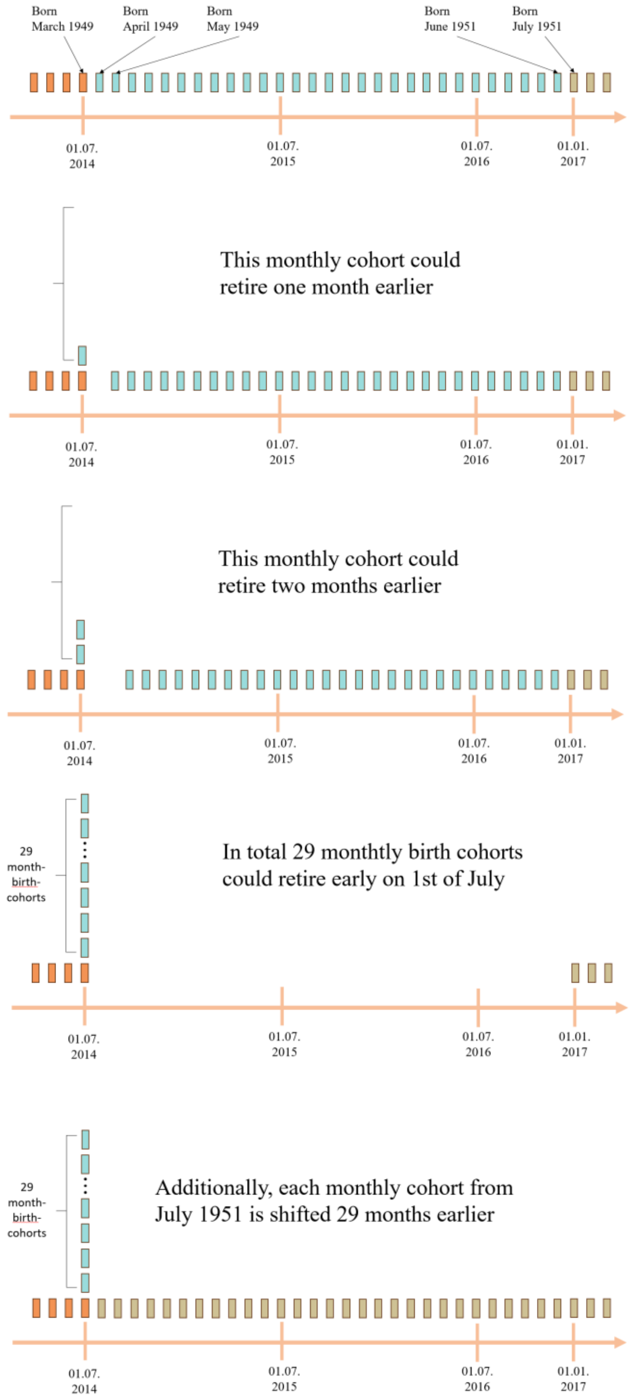


(b)

Median age at retirement by monthly birth cohort

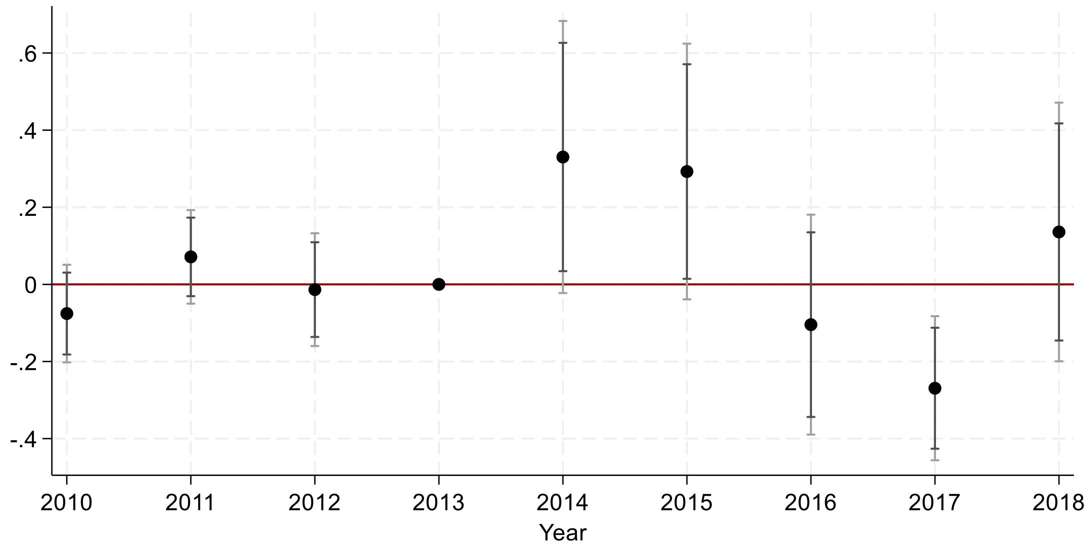
Note: Panel (a) shows the distribution of age at retirement per calendar year. Orange borders in 2013 mark the bars for ages 63 and 64 in 2014. Panel (b) shows the median actual retirement age by last spell subject to full-social security contributions at the firm for workers retiring between 62 and 66. The orange line shows the projected age at retirement under full eligibility and compliance with the pension reform.
Source: IAB staff calculations. Own calculations based on IAB data.

Figure 3 - Potential effect of the reform on timing of retirements



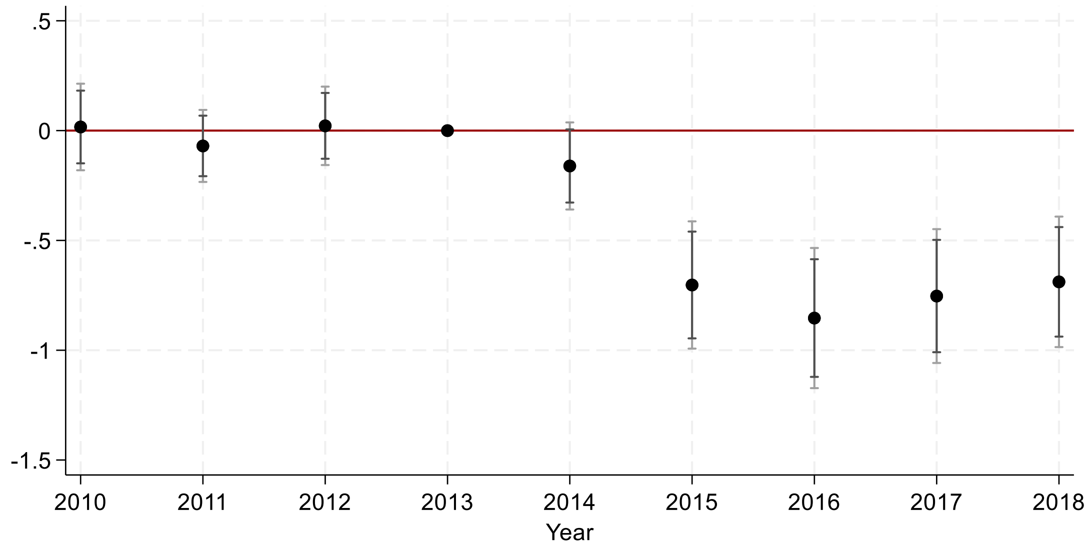
Note: This panel shows the potential change in eligibility to retire by monthly birth cohort due to the 2014 reform. Each block represents one monthly birth cohort and the x-axis represents the expected and later actual realized date of retirement under full compliance.
Source: Own illustration

Figure 4 - Event-Studies on Retirement Shares and Log Average Age



(a)

Event-study coefficients on retirement share, relative to 2013.

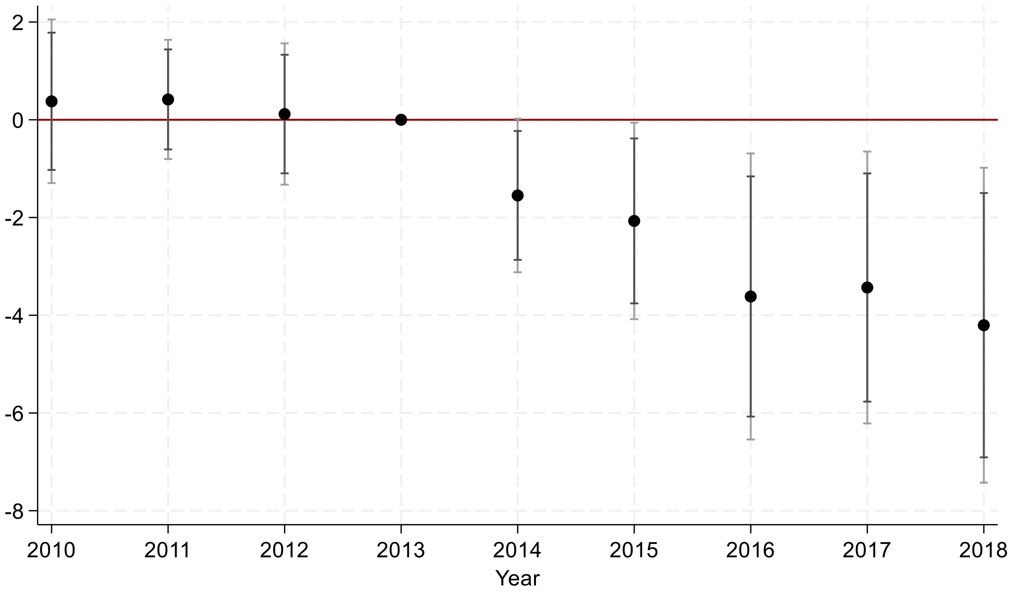


(b)

Event-study coefficients on log average workforce age, relative to 2013.

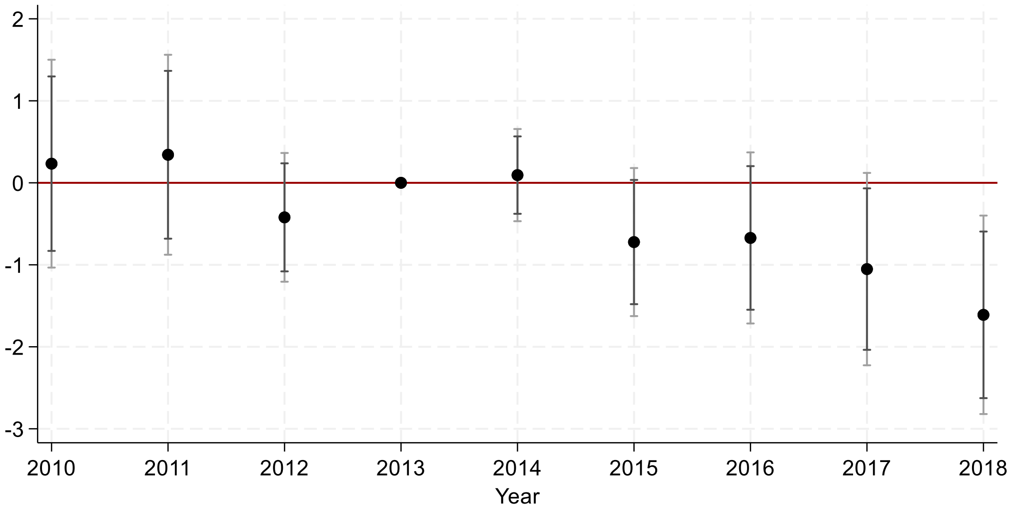
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Panel (a) plots coefficients with the retirement share as the dependent variable; Panel (b) plots coefficients with the log of the average workforce age as the dependent variable. Retirement shares are calculated as the number of workers leaving the firm aged 62–65 who have their last spell at the firm in the calendar year divided by the total number of employees subject to full social security contributions on June 30. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure 5 - Event-Studies on Stock of Employees with more than 15 years of firm tenure and Log Number of Total Workers



(a)

Event-study coefficients on the inverse hyperbolic sine (IHS) of the number of workers with more than 15 years of firm tenure, relative to 2013.

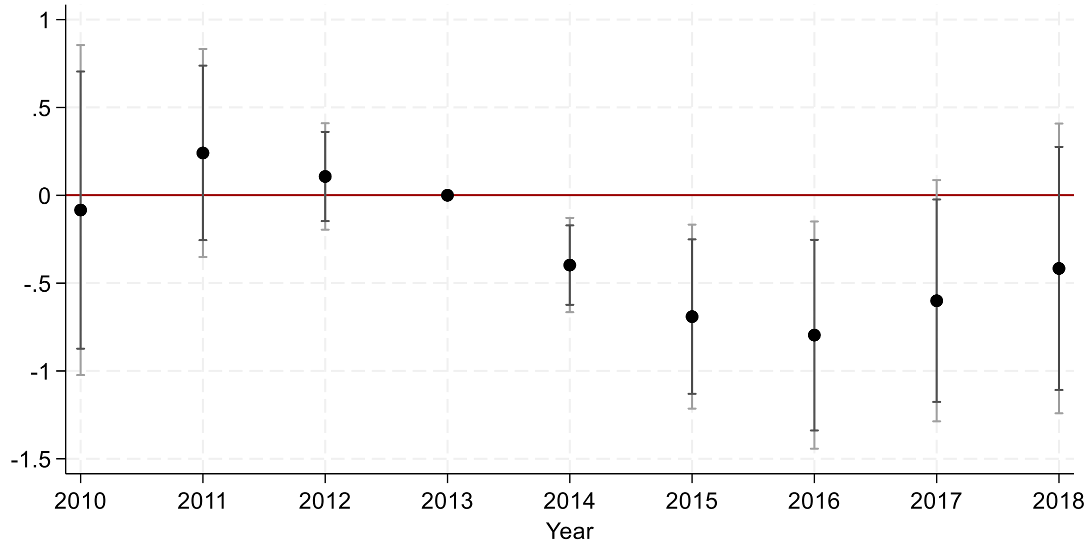


(b)

Event-study coefficients on log total number of workers, relative to 2013.

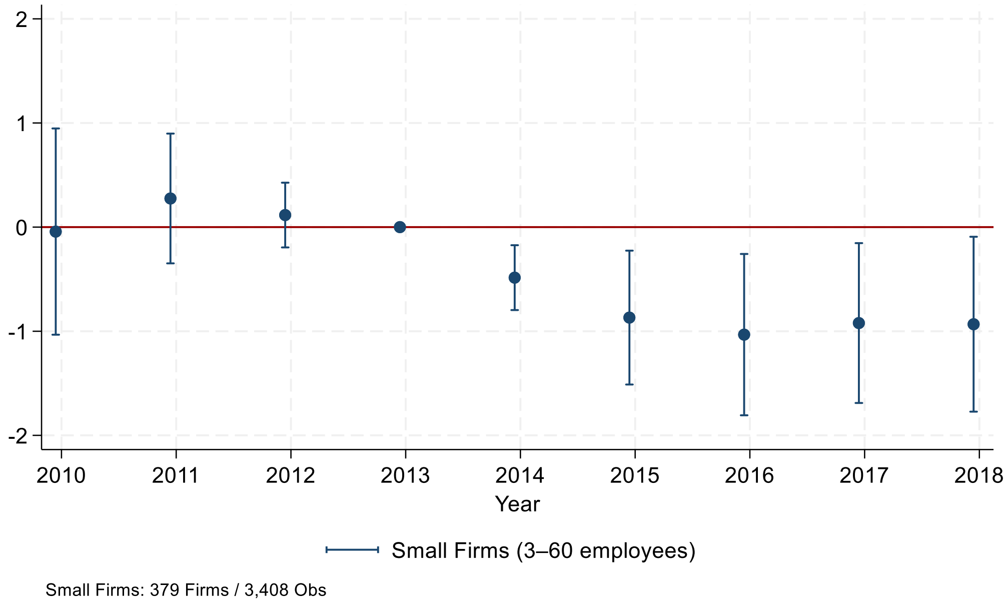
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Panel (a) plots coefficients with the inverse hyperbolic sine (IHS) of the number of workers in the firm with more than 15 years of firm tenure as the dependent variable; Panel (b) plots coefficients with the total number of workers as the dependent variable. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level
Source: Calculations based on IAB data

Figure 6 - Event-Studies on Log Capital Stock



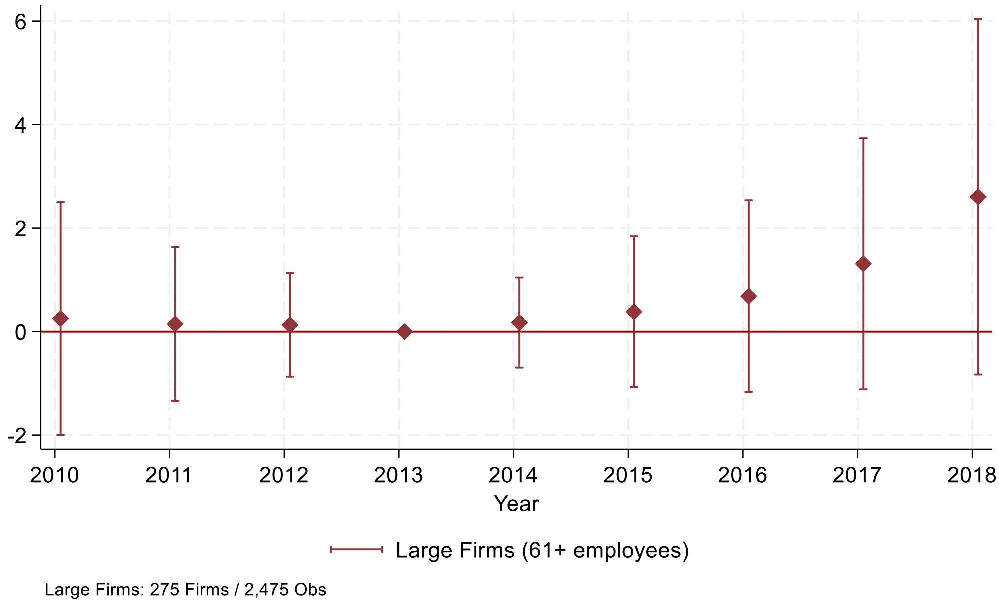
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). The dependent variable is the log capital stock. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure 7 - Event-Study Estimates of Log Capital Stock by Firms Size



(a)

Event-study coefficients on log capital stock for small firms, relative to 2013.

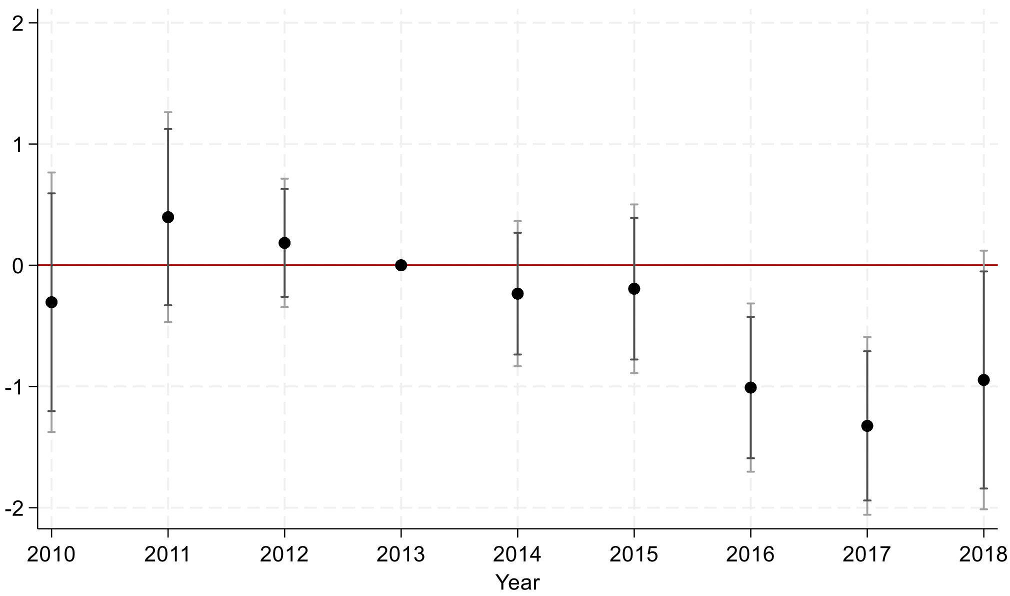


(b)

Event-study coefficients on capital stock for large firms, relative to 2013.

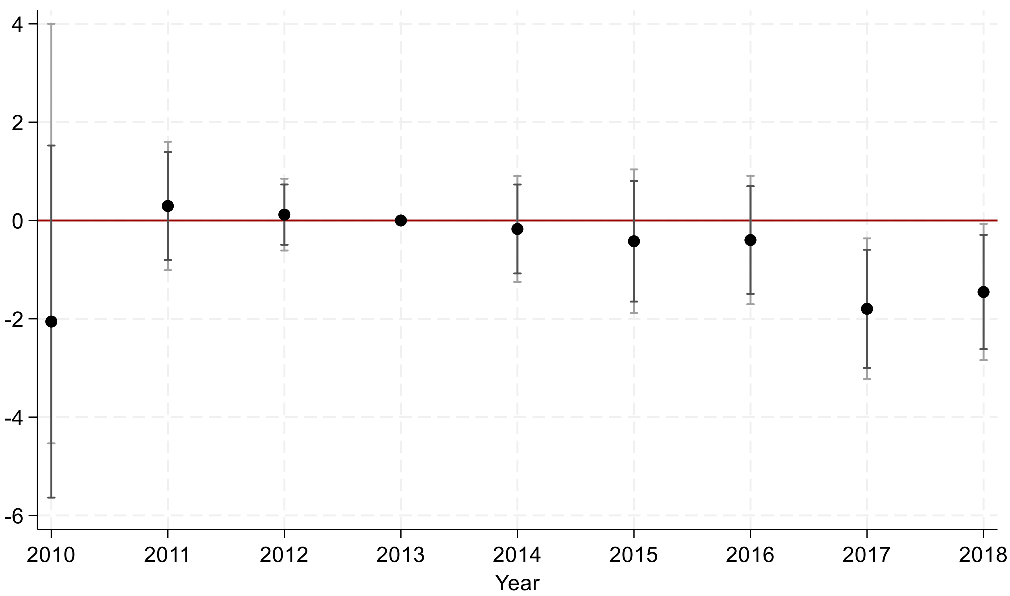
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Panels plot coefficients with the log capital stock as the dependent variable for (a) small firms (3–60 employees) and (b) large firms (>60 employees) in 2013. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure 8 - Event-Studies on Log Revenue and Log Value Added



(a)

Event-study coefficients on log revenue

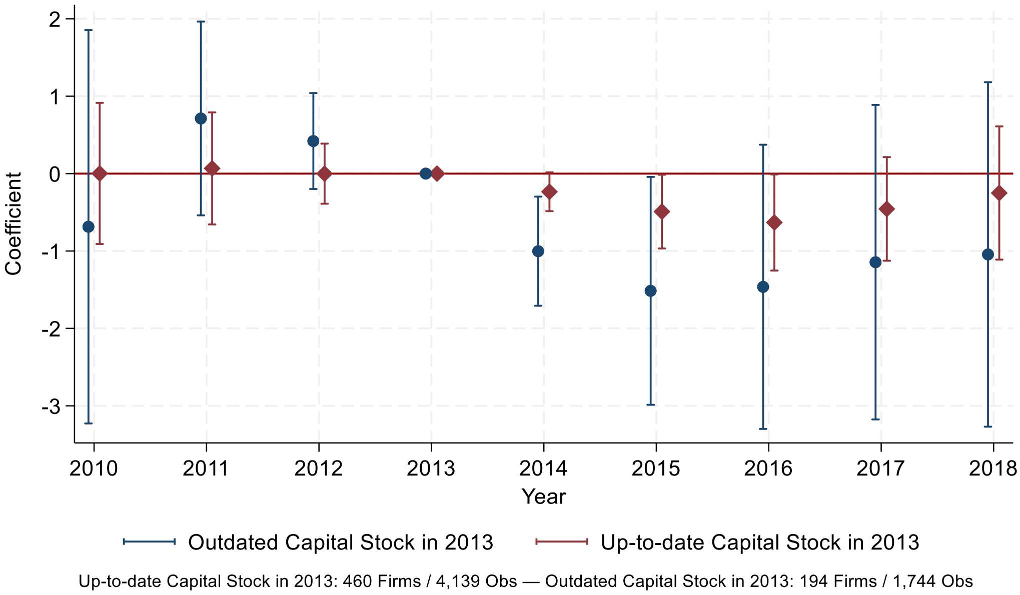


(b)

Event-study coefficients on log value added

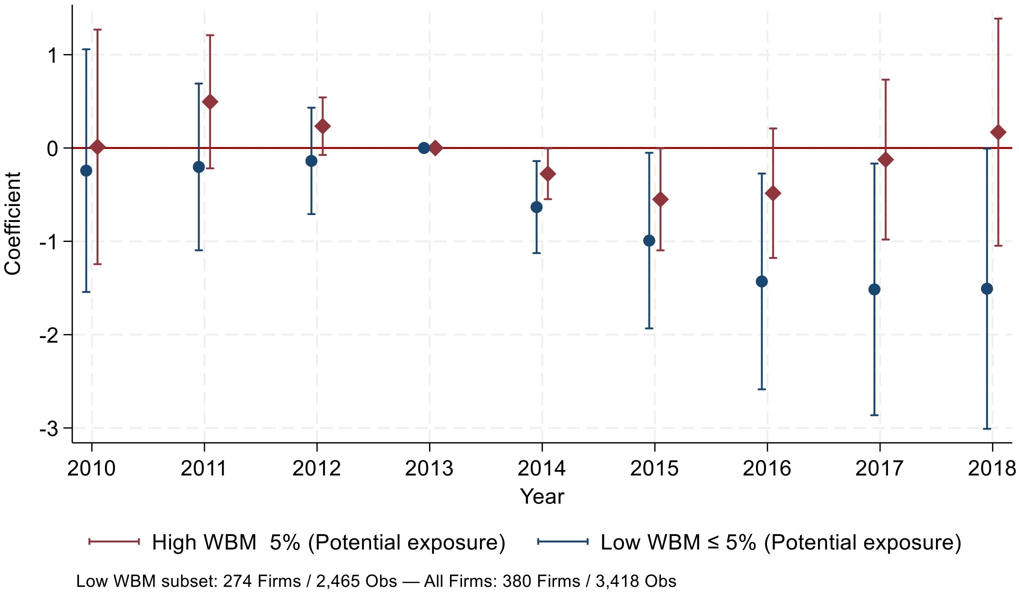
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Panels plot coefficients with log revenue (Panel a) and log value added (Panel b) as the dependent variable. Log revenue is reported revenue in euros. Value added is calculated as reported revenue multiplied by the reported share of revenue not used to purchase externally sourced intermediate inputs. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure 9 - Event-Studies on Log Capital Stock by Capital Vintage and Training Intensity



(a)

Event-study coefficients on log capital stock by self-assessed capital modernity, relative to 2013.



(b)

Event-study coefficients on log capital stock by share of in-company trainings per worker, relative to 2013.

Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Panels plot coefficients with the log capital stock as the dependent variable. Panel (a) splits the sample between firms reporting a rather outdated capital stock in 2013 and those reporting a rather up-to-date capital stock. Panel (b) presents results for all firms and for firms reporting very low levels of in-company training per worker. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors are clustered at the firm level.
Source: Calculations based on IAB data

Tables

Table 1 – Summary Statistics

	Exposure=0	Exposure>0	Exposure <=0.002	Exposure>0.002
Number of Workers 2013	45.89 (73.12)	408.76 (2286.80)	193.13 (1897.59)	213.71 (481.93)
Log Capital Stock 2013	14.69 (1.87)	16.41 (1.78)	14.87 (2.03)	16.28 (1.68)
Average Age 2013	43.19 (5.63)	45.19 (3.98)	43.21 (5.51)	45.33 (4.02)
Share Age 56-61 in 2013	13.05% (10.69%)	16.03% (6.84%)	13.10% (10.45%)	16.20% (6.96%)
Retirement Share 2013	1.08% (2.87%)	1.73% (2.10%)	1.10% (2.83%)	1.76% (2.10%)
Number of Firms	374	280	396	258

Note: This table shows means and standard deviations of the sample of firms split by low and high exposure to the reform one year before the reform in 2013.
Source: Own calculations based on IAB data

Table 2 – Distribution of Potential Exposure Measure

E_j^{pot}	N Obs	Mean	Sd	P10	P25	P50	P75	P90
Exposed Firms	280	0.0179	0.0225	0.0027	0.0059	0.0117	0.0219	0.0360
Non-exposed Firms	374	0	0	0	0	0	0	0
All Firms	654	0.0360	0.0172	0	0	0	0.0093	0.0229

Note: This table shows the distribution of the potential exposure E_j^{pot} measure across firms in my sample. Potential exposure is calculated by the number of workers who are eligible to retire earlier, weighted by their months of early eligibility, and divided by the total number of workers subject to full social security contributions in the firm weighed by 24. The corresponding number is between 0 and 1 and represents the share of labor input in worker months in a two year window that is at risk of loss to the firm as a result of the reform. The reference day for calculation is the 01.05.2014, two months before the reform takes effect. Columns labeled P10, P25, P50, P75, and P90 report the 10th, 25th, 50th, 75th, and 90th percentiles of the distribution of potential exposure.
Source: Own calculations based on IAB data

Table 3 – Distribution of Actual Exposure

E_j^{act}	N Obs	Mean	Sd	P10	P25	P50	P75	P90
Exposed Firms	163	0.0107	0.0136	-	0.0026	0.0064	0.0154	-
Non-exposed Firms	491	0	0	0	0	0	0	0
All Firms	654	0.0027	0.0082	0	0	0	0	0.0082

Note: This table shows the distribution of the potential exposure E_j^{act}measure across firms in my sample. Potential exposure is calculated by the number of workers who are eligible to retire earlier and who are not observed anymore in the firm on 01.05.2015, weighted by their months of early eligibility, and divided by the total number of workers subject to full social security contributions in the firm weighed by 24. The corresponding number is between 0 and 1 and represents the share of labor input in worker months in a two year window that is at risk of loss to the firm as a result of the reform. The reference day for calculation is the 01.05.2014, two months before the reform takes effect. Columns labeled P10, P25, P50, P75, and P90 report the 10th, 25th, 50th, 75th, and 90th percentiles of the distribution of potential exposure. The P10 and P90 are not reported for exposed firms due to the number of observations required for data disclosure.
Source: Own calculations based on IAB data

Table 4 – Investment connected with product or process innovation planned but not carried out

	(1)	(2)
	2014	2014
Potential Exposure	1.784**	1.742**
	(0.821)	(0.790)
2013 Share Age ϵ [56-61] (%)	-0.188*	-0.147
	(0.096)	(0.102)
Industry Controls		x
R^2	0.01	0.04
Observations	654	654

Note: Estimates from a linear probability model. The dependent variable is a binary indicator if the firm reported that it had not carried out an initially planned investment connected with a product or process innovation in that year. Coefficients represent the change in the probability of this event when exposure increases from 0 to 1. Exposure is scaled [0,1, where 0.01 corresponds to a 1-percentage-point increase in exposure. Thus, the effect of a 1 pp increase equals 0.01 times the reported coefficient. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own calculations based on IAB data

Table 5 - Regression Results New Technology introduced between 2016-2018 & 2018-2020

	(1)	(2)
Potential Exposure	-4.167***	
	(1.269)	
Actual Exposure		-6.501***
		(2.089)
Average Age in Firm (pre-2014)	-0.004	-0.004
	(0.004)	(0.004)
Share $\geq$ 60 in Firm (pre-2014)	-1.067***	-1.181***
	(0.272)	(0.268)
Industry Controls	x	x
Year Controls	x	x
R^2	0.053	0.052
Observations	11,208	11,208

Note: Estimates from a linear probability model. The dependent variable indicates whether a new technology was introduced at the workplace in the last two years (LPP-Employee Survey, pooled 2016–2018 and 2018–2020). Coefficients represent the change in the probability of this event when exposure increases from 0 to 1. Exposure is scaled [0,1, where 0.01 corresponds to a 1-percentage-point increase in exposure. Thus, the effect of a 1 pp increase equals 0.01 times the reported coefficient. Standard errors clustered at the firm level in parentheses. Industry and year controls included. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own calculations based on IAB data

Table 6 Effect of Reform Exposure on Profitability

	(1) 2014	(2) 2015	(3) 2016	(4) 2017
Potential Exposure	-0.03	–2.71**	–0.84	–1.04
	(0.82)	(1.26)	(1.04)	(1.16)
Profit in 2013	0.59***	0.43***	0.26***	0.35***
	(0.06)	(0.06)	(0.06)	(0.06)
Share 56-61 (2013)	–0.06	–0.05	–0.20	–0.06
	(0.15)	(0.16)	(0.17)	(0.15)
R^2	0.33	0.18	0.08	0.14
Observations	534	532	532	527

Note: Estimates from a linear probability model. The dependent variable is a binary indicator whether the firm reported a profit as opposed to break-even or a loss in the respective year. Coefficients represent the change in the probability of this event when exposure increases from 0 to 1. Exposure is scaled [0,1, where 0.01 corresponds to a 1-percentage-point increase in exposure. Thus, the effect of a 1 pp increase equals 0.01 times the reported coefficient. Profit in 2013 is a binary indicator whether the firm reported a profit in 2014. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own calculations based on IAB data

Appendix A1 Calculation of the Potential Financial Gain from the 2014 Reform

On July 1, 2014, the German government introduced the *Rente für besonders langjährig Versicherte*, granting workers with at least 45 contribution years the right to retire at age 63 without deductions. Before this reform, claiming an early old-age pension at age 63 always entailed a permanent reduction of 0.3 percent per month before the *Regelaltersgrenze* (normal retirement age). For the first eligible cohort, the normal retirement age was 65 years and 5 months, so retiring at 63 meant 29 months earlier, implying a deduction of 8.7 percent that was now removed.

The financial gain from this change depends on the average monthly pension, the expected number of years of benefit receipt, and the avoided deduction rate. Using data from the German Pension System we can approximate the value as follows.

According to data from DRV (2024), the average monthly pension for workesr with at least 45 years of pension contributions in 2014 was €1,184 (p. 89). The average remaining life expectancy at pension start (2013–2015) was 17.47 years for men and 20.95 years for women (pp. 148–149). In 2014, 72.5 percent of all new pensions in this category were claimed by men (pp. 62–63). Weighting these values gives an expected average duration of benefit receipt of

$$0.725 \cdot 17.47\, years + (1 - 0.725)\ \cdot 20.95\, years = 18.43\, years$$

Adding two years to reflect the earlier claiming age yields a total expected duration of pension payments of 20.43 years. The average annual gross wage in 2014 was €34,514 (p. 258). The avoided deduction thus equals

$$0.087 \cdot 1184 \frac{EUR}{Month} \cdot 12 \frac{Month}{Year} \cdot 20.43\, Year = 25{,}243.76\, EUR$$

This corresponds to a lifetime financial gain of 25,243.76 EUR under the assumption that future nominal pension adjustments track inflation. Relative to the 2014 average gross annual earnings, this gain equals

$$\frac{25{,}243.76}{34{,}514} = 0.7315$$

Or approximately three-quarters of one year´s gross income. Using the German CPI index this corresponds to roughly 32,056 EUR in 2024 or 34,620 USD.

Appendix A2 Additional Empirical Specifications

This appendix details the empirical specification underlying additional results reported in Section 5.

Instrumental Variable Estimation of Capital Change

To estimate the causal effect of the unexpected increase in firms´ retirement shares on capital, I use a two-stage least squares (2SLS) specification. The outcome variable is the change in log capital between 2013 and 2015. The endogenous regressor is the change in the retirement share, defined as the difference between the average retirement rate in 2014–2015 and the average in 2012–2013. The change in retirement share is instrumented with firms' scaled potential exposure.

First Stage: $\Delta R_i = \alpha_1 + \pi \cdot E_j^{pot} + \gamma \cdot Share56to61_i + \epsilon_i$

Second Stage: $\Delta K_i = \alpha_1 + \pi \cdot E_j^{pot} + \gamma \cdot Share56to61_i + \epsilon_i$

with $\Delta R_i = \frac{1}{2}\left(R_{i,2014} + R_{i,2015}\right) - \frac{1}{2}\left(R_{i,2012} + R_{i,2013}\right)$ and $R_{i,t}$ being the retirement share in firm i in year t as well as $\Delta K_i = K_{2015} - K_{2013}$ and $K_{i,t}$ being the log capital stock in firm i in year t.

Regression on Investment Cuts

I define a binary investment cut indicator Cut_t to capture a more than 30 percent drop in investment relative to pre-reform averages as follows:

$$Cut_t = 1\left[\frac{I_{2016} + I_{2015} + I_{2014}}{I_{2013} + I_{2012} + I_{2011}} < 0.7\right]$$

where I_t denotes the firm´s investment in year t. The regression is specified as

$$Cut_i = \alpha + \beta \cdot E_j \ + \gamma \cdot Share56to61_i + \epsilon_i$$

With E_j being either potential exposure E_j^{pot} or actual exposure E_j^{act} and $Share56to61_i$ the share of workers aged between 56 and 61 in 2013.

Regression on Profitability

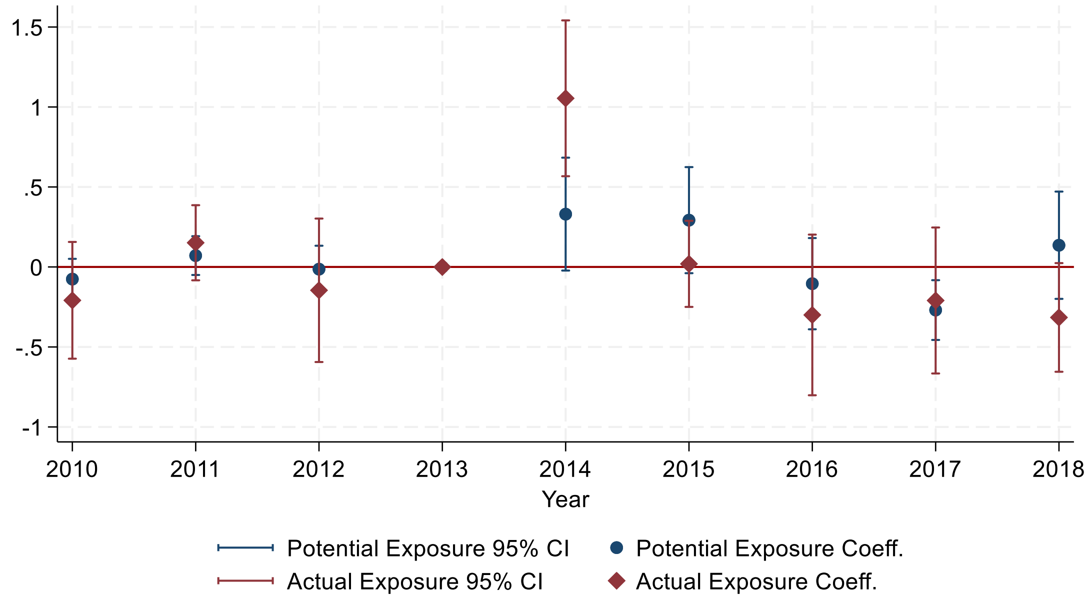
Firms report whether they incurred a profit, a loss or a break-even. Due to the sensitivity of this information, exact Euro values of profits, as they are available for revenue, are unfortunately not available. I construct a binary indicator $Profit_t$ that takes 1 if the firm reported a profit in a given year. I then estimate the following linear probability model

$$Profit_t = \alpha + \beta \cdot E_j^{pot} + \gamma \cdot Profit_{2013} + \delta \cdot Share56to61_i + \epsilon_i$$

With E_j^{pot} being potential exposure and $Share56to61_i$ the share of workers aged between 56 and 61 in 2013.

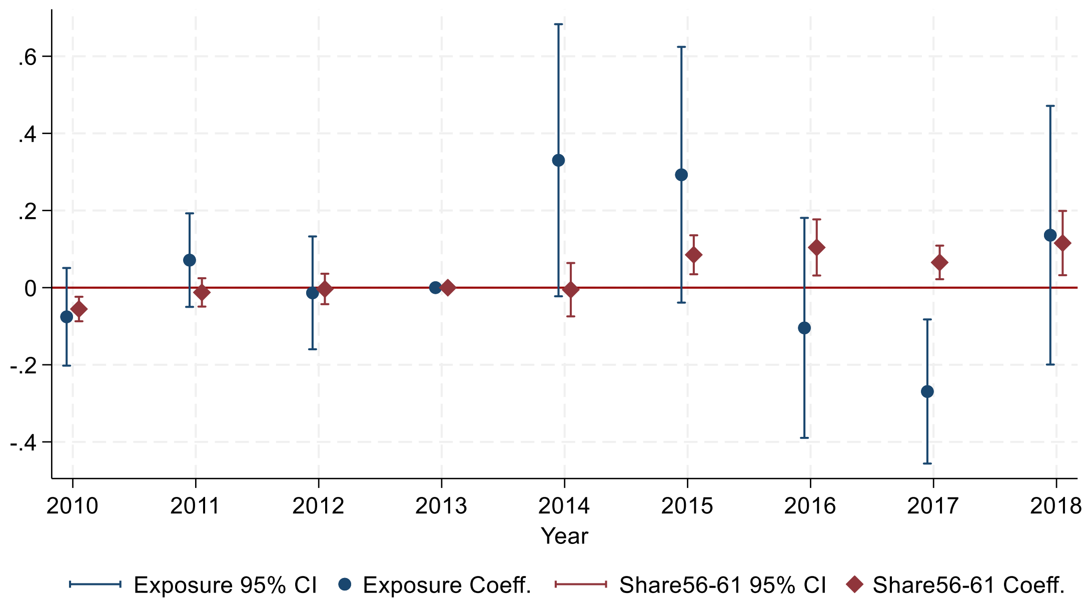
Appendix Figures

Figure A1 - Event-Study on Retirement Shares



(a)

Retirement Share – Comparison Potential Exposure vs Actual Exposure

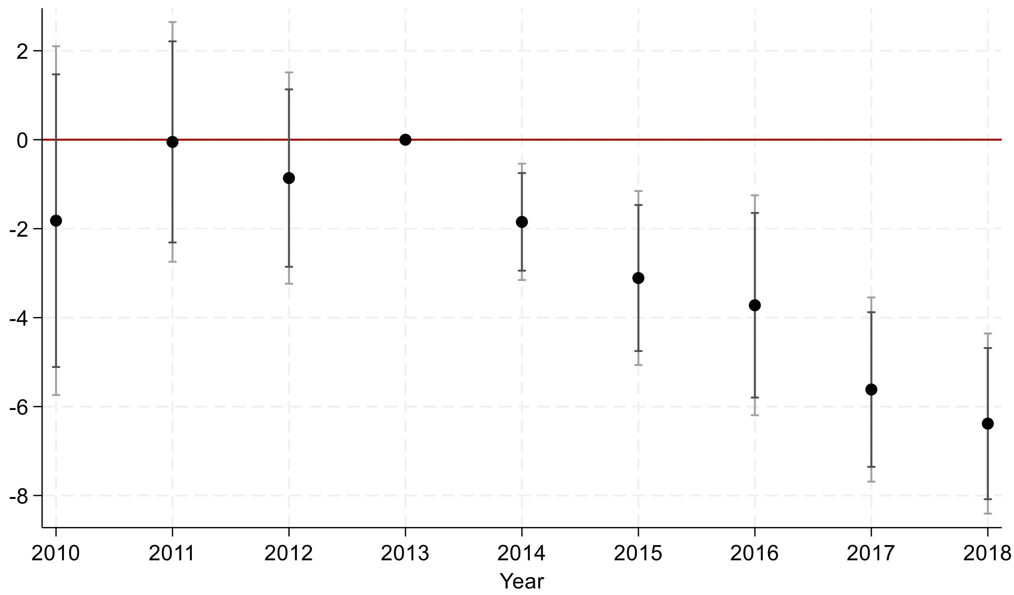


(b)

Retirement Share – Comparison Potential Exposure vs Share 56-61

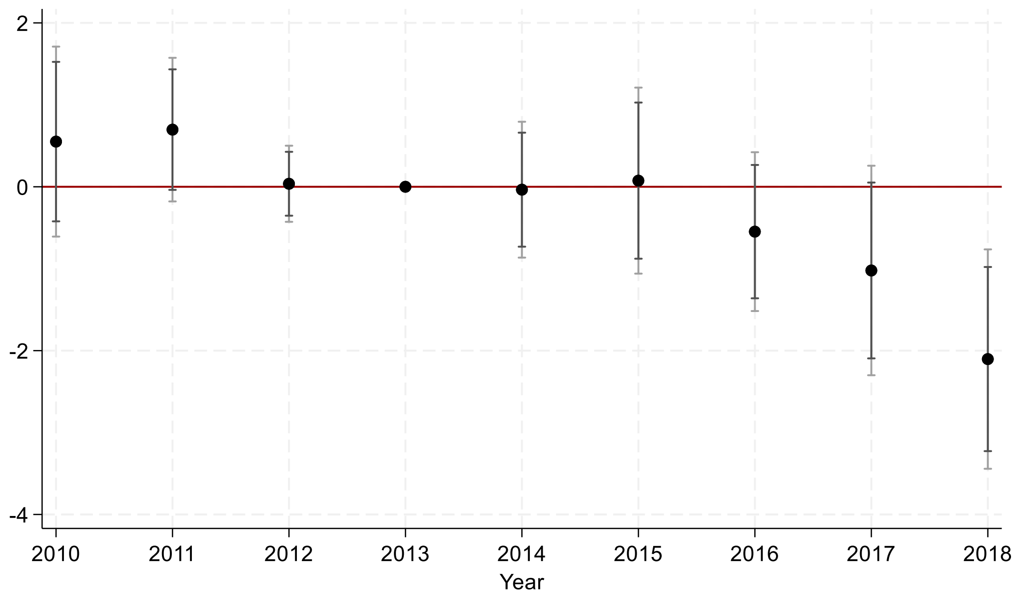
Note: Estimates from an event-study according to Equation (1). In panel (a), I run two separate regressions where the treatment variable is either potential exposure or actual exposure; in both cases the pre-reform share of workers aged 56–61 is included as a control but its coefficients are not shown. Panel (b) reports results from a single regression including both potential exposure and the pre-reform share aged 56–61 simultaneously; the figure displays the coefficients on each variable.
Source: Calculations based on IAB data

Figure A2 - Event-Study on Number Workers with more than 5 years of tenure and Log Total Number of Full-time Employees



(a)

IHS of Number of Workers with more than 5 years of tenure

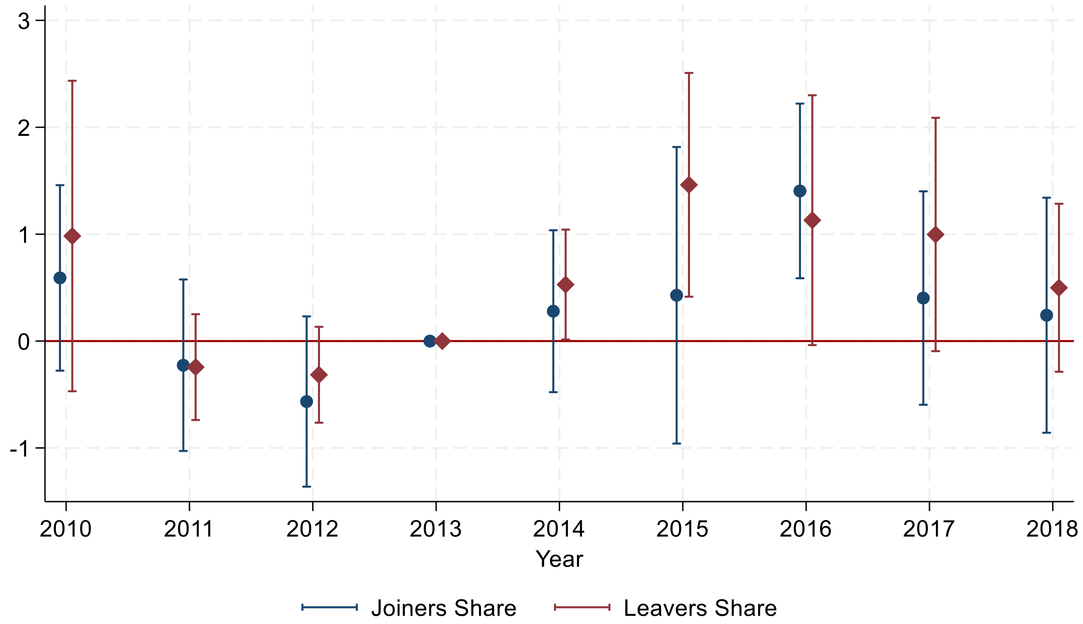


(b)

Log Total Number of Full-Time Workers

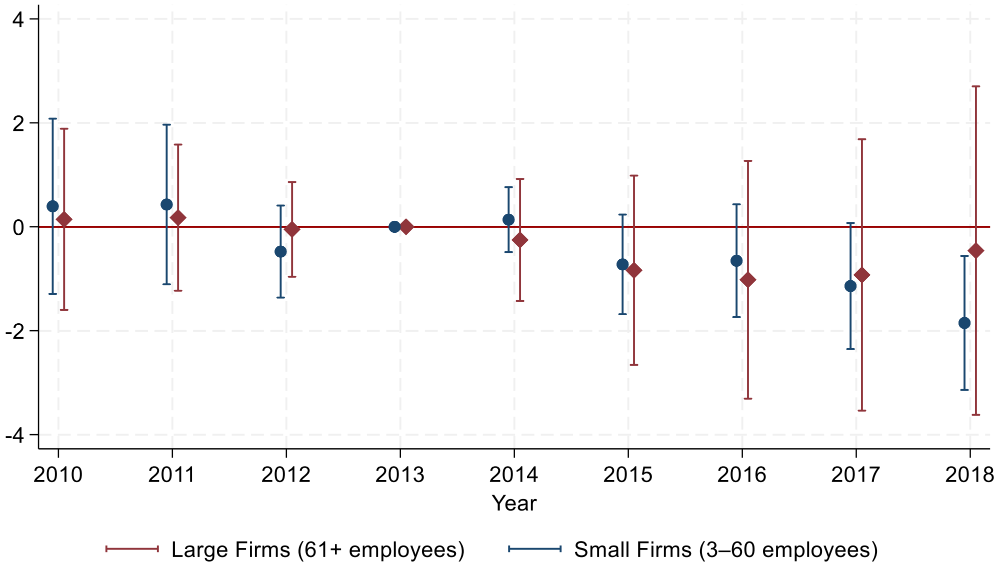
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). The dependent variable is the inverse hyperbolic sine (IHS) of the number of workers with more than 5 years of tenure (Panel A) and the log total number of fulltime employees (Panel B). Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure A3- Event-Study on Firm Leavers and Joiners



Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Coefficients are from two separate regressions. Regression one uses the Joiners Share as the dependant variable calculated the number of workers joining the firm in a given year divided by the total number of workers. Regression two uses the Leavers Share as the dependant variable calculated the number of workers leaving the firm in a given year divided by the total number of workers. Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

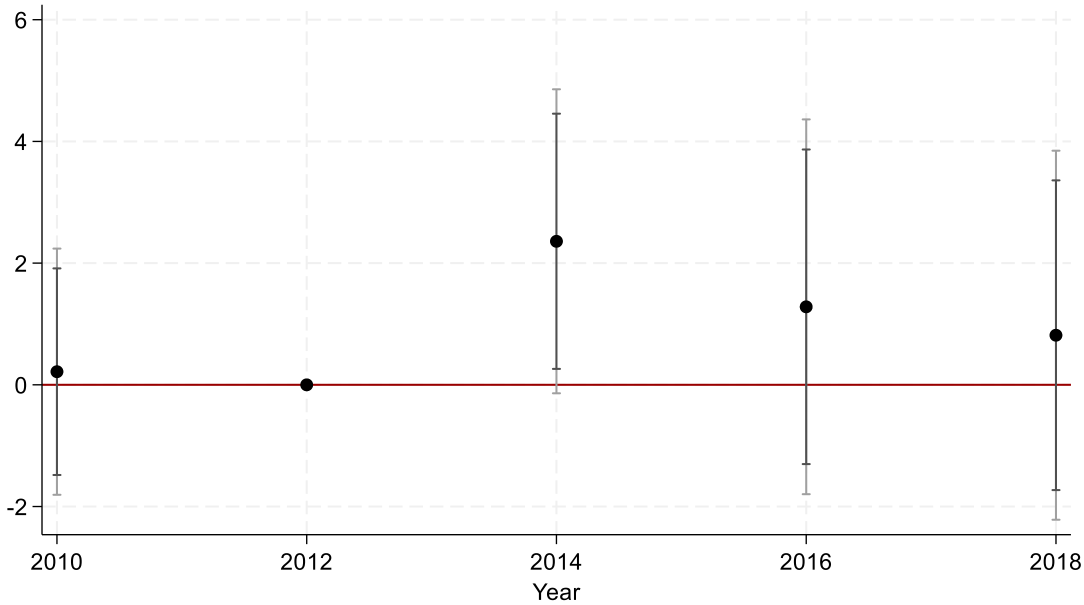
Figure A4 - Event-Study on Total Number of Employees



Log Number of Employees – Comparison Small and Large Firms

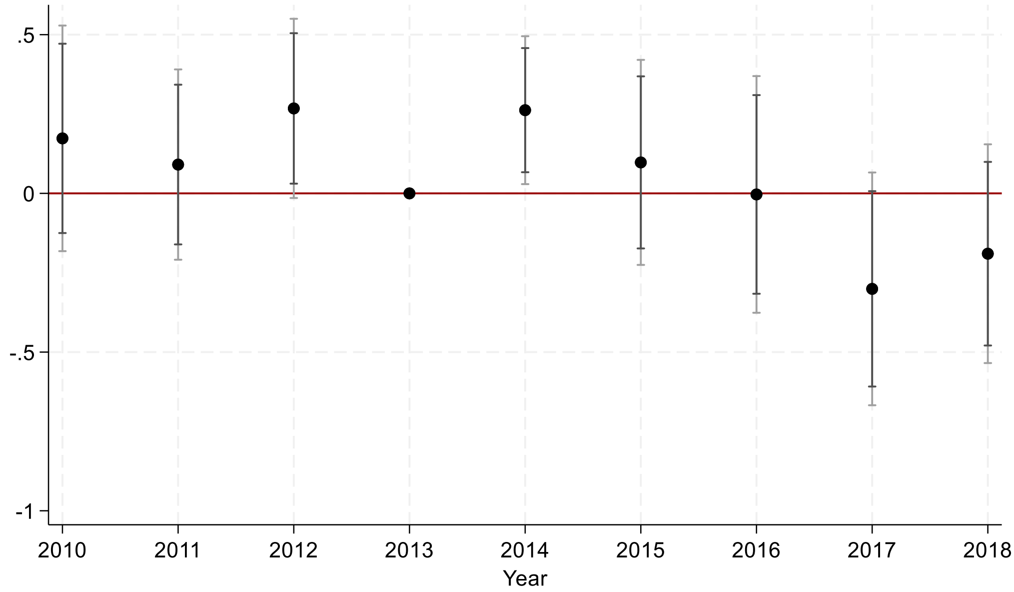
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). Coefficients are from two separate regressions. Regression one uses a large firms (61+ employees) and regression two uses small firms (3-60 employees). Coefficients are shown relative to 2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure A5 - Event-Study on cancelled Investment connected with a Product or Process Innovation



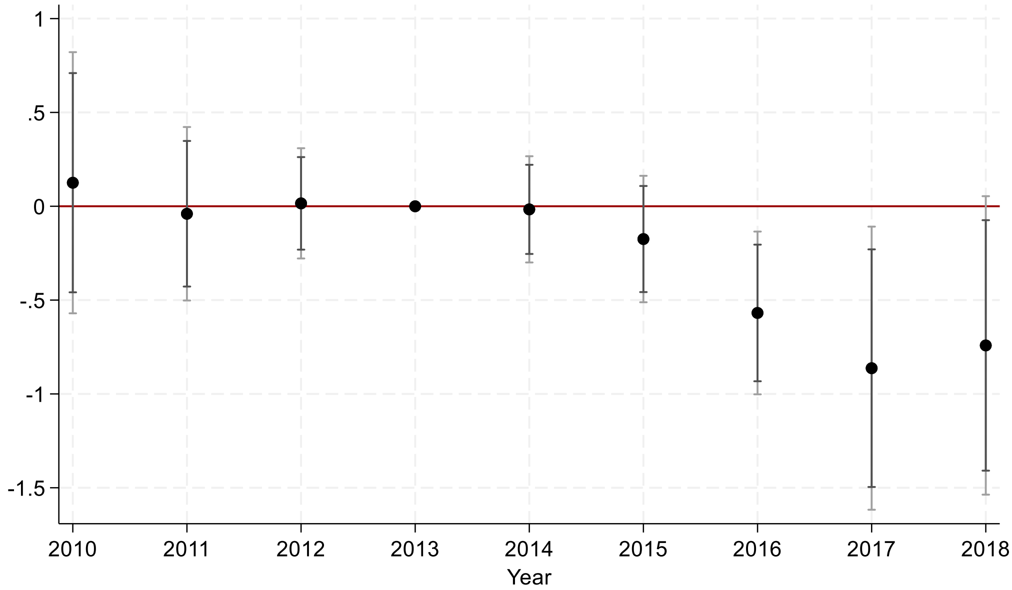
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). The dependent variable is a binary indicator if the firm reported that it had not carried out an initially planned investment connected with a product or process innovation in that year. Data is only available biannually in 2010, 2012, 2014, 2016, and 2018. Coefficients are shown relative to 2012. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure A6 - Event-Study on Mean AKM Worker Fixed Effects



(a)

Mean 2007-2013 Person Fixed AKM Effect – All Workers

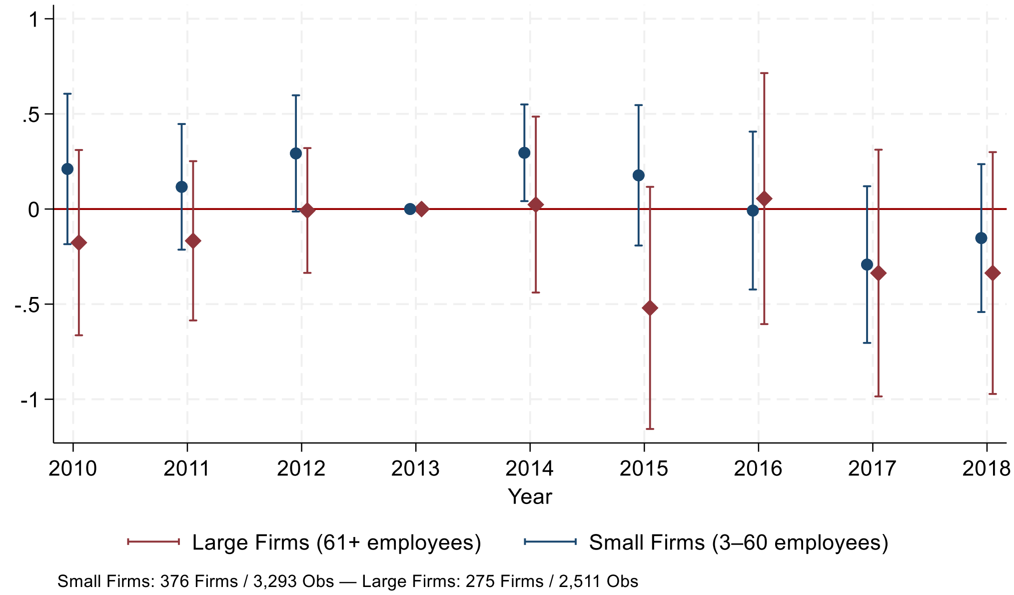


(b)

Mean 2007-2013 Person Fixed AKM Effect – Workers Aged 25-45

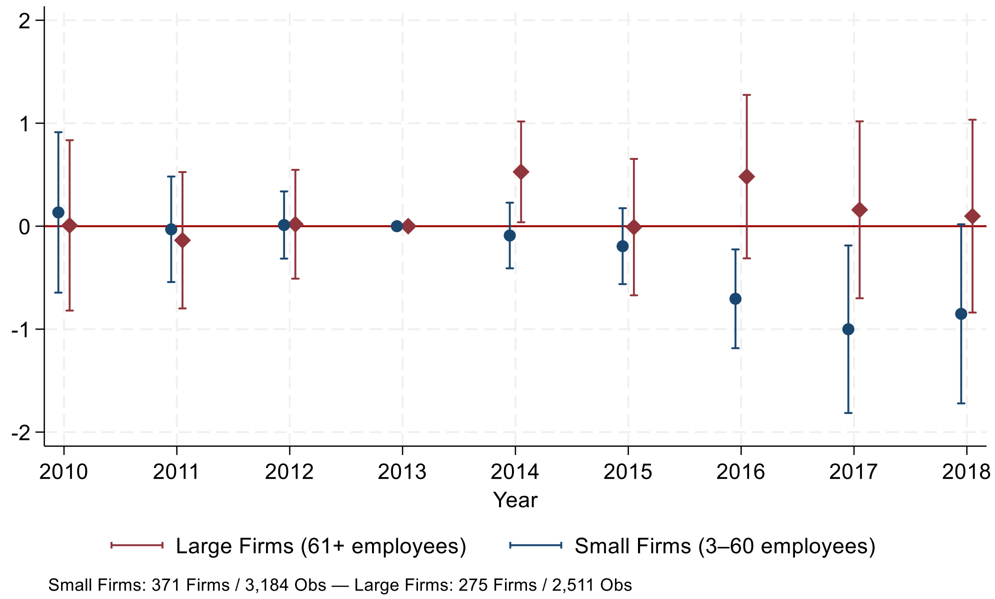
Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). The dependent variable is the mean worker fixed effect from an Abowd–Kramarz–Margolis (AKM) wage decomposition for all workers (Panel A) and for workers aged 25–45 (Panel B). AKM effects were estimated by the IAB using the universe of all German workers and firms during 2007–2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Figure A7 - Event-Study on Mean AKM Worker Fixed Effects by Firmsize



(a)

Mean 2007-2013 Person Fixed AKM Effect – All Workers



(b)

Mean 2007-2013 Person Fixed AKM Effect – Workers Aged 25-45

Note: Event-study estimates of coefficients β_k from Equation (1) capturing the interaction between exposure and event time (year). The dependent variable is the mean worker fixed effect from an Abowd–Kramarz–Margolis (AKM) wage decomposition for all workers (Panel A) and for workers aged 25–45 (Panel B). AKM effects were estimated by the IAB using the universe of all German workers and firms during 2007–2013. Thin lines indicate 95% confidence intervals; thick bars indicate 90% confidence intervals. Standard errors clustered at the firm level.
Source: Calculations based on IAB data

Appendix Tables

Table A1 – Two-stage Least Square Estimate of Change Log Capital 2013 to 2015 on Change in Average Retirement Share 2012/2013 to 2014/2015 instrumented by Potential Exposure

Y_t: $\ln(K_{2015}) - \ln(K_{2013})$ Instrument: Scaled Potential Exposure	(1)
Change avg retirement share	-1.848**
2012/2013 to 2014/2015	(0.882)
2013 Share Age ϵ [56-61] (%)	-0.101
	(0.083)
Constant	0.091***
	(0.019)
Kleibergen -Paap Wald F Statistic	12.70
Observations	647

Note: Estimates from a two-stage least squares regression of the log difference in capital in the year 2015 to the year 2013 on the change in the average retirement rate from 2012 and 2013 to 2014 and 2015 instrumented by scaled potential exposure. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own calculations based on IAB data

Table A2 – Change in Share of Externally Sourced Inputs 2013 to 2018

	(1)
Potential Exposure	-0.562
	(0.846)
Potential Exposure · 1[Below median capital growth 2013-2018]	5.324***
	(1.729)
2013 Share Age ϵ [56,61] (%)	-0.153
	(0.228)
R^2	0.01
Observations	410

Note: Estimates from an OLS regression. The independent variable is the change in the log share of externally sourced inputs from 2013 to 2018. 1[Below median capital growth 2013-2018] is an indicator that takes the value 1 if the change in capital from 2013 to 2018 is below the median change of all firms and zero if the growth was above the median. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Source: Own calculations based on IAB data