Firms, Executives and Retirement*

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Preliminary results, please do not circulate without permission.

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Abstract

Using monthly employer–employee data covering the Dutch population, we document tremendous unobserved heterogeneity across employers in job separation rates at the Statutory Retirement Age (SRA), where job contracts automatically terminate. Employers' propensity to enforce mandatory retirement strongly predicts workers' employment gains from raising the SRA. Yet, employer effects are not fixed: movers' designs show that incoming chief executives transmit more than half of their prior SRA retirement practices, with stronger transmission in the private sector and among male executives. On average, a change in executive shifts private-sector workers' SRA separation probabilities by 8 percentage points. We see no evidence that appointing a stricter executive leads to a broader decline in the share of older workers or changes in firms' financial performance. Our results suggest that interventions targeting softer managerial approaches towards mandatory retirement could meaningfully increase old-age employment without incurring large costs on firms.

JEL Codes: H55, L22, J23, J26,
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> *If employers and other key stakeholders are to adapt to ongoing changes in the world of work, they will need to re-evaluate their policies and practices with an eye toward an age-inclusive workforce.*

OECD, *Promoting an Age-Inclusive Workforce*, 2020

1 Introduction

Reducing barriers towards old-age employment is an important policy goal to address the demographic challenges associated with population aging. Many countries have implemented policies to incentivize workers to delay retirement, such as raising the pension age and scaling back early retirement pathways. While these reforms typically increase old-age employment,[1] they are politically unpopular,[2] often yield undesirable distributional effects,[3] and likely face diminishing returns given the well-documented decline in employment with age. Moreover, these policies may be ineffective at increasing the employment of a large and growing group of retirees who report being forced into retirement by their employer (Dorn and Sousa-Poza, 2010; Steiber and Kohli, 2017; Johnson and Gosselin, 2018). Interventions that encourage employers to retain older workers are thus fertile ground for policy. However, understanding which interventions may be effective is challenging, as we know little about the reasons employers force older workers into retirement, the extent to which these practices vary across employers, and the influence of workplace norms and managerial discretion on retirement timing.

In this paper, we address these gaps in the context of the Netherlands using high-frequency administrative data, which allows us to link the monthly employment outcomes of all older workers to their employer and chief executive. We focus on job separations at the Statutory Retirement Age (SRA), which was historically set at 65 and has increased gradually to 67 since 2015. The SRA is an important threshold where employers can easily push workers into retirement, since most permanent contracts are automatically terminated and workers require a new contract to continue working for the employer. Around one-in-five Dutch people retire at the SRA, with a retirement hazard of nearly 70% for those still working (Rabaté et al., 2024). This high hazard rate is despite the absence of strong incentives for workers to retire at this age, emphasizing the importance of employers.[4] This makes the SRA an ideal setting to detect whether who you work for — and who

[1] See Börsch-Supan and Schnabel (1998); Mastrobuoni (2009); Staubli and Zweimüller (2013); Rabaté et al. (2024); Lalive et al. (2023); Geyer and Welteke (2021); Nakazawa (2022); Lindeboom and Montizaan (2020a).

[2] The general public tends to view such reforms negatively (Bello and Galasso, 2021), a sentiment underscored by the mass protests that erupted in France in 2023 after the retirement age was raised from 62 to 64.

[3] See Cribb and Emmerson (2019); Andersen et al. (2021); Morris (2022).

[4] Reaching the SRA coincides with eligibility for a universal flat-rate public pension. However, since there is no earnings test, workers have little economic incentive to retire at the SRA. Consistent with a key role for employers,

you work under — matters for when you retire.

Our analysis focuses on employers with multiple workers who reached the SRA over our analysis period (2006–2024), which allows us to distinguish the role of employers from idiosyncratic factors affecting individual workers. We start with two descriptive analyses that indicate tremendous heterogeneity in SRA retirement practices across employers, particularly in the private sector. First, we show graphically that workers' job separation rates at the SRA vary considerably across employers — in fact, the variance in average separation rates across employers is four times larger than would be expected by chance. Second, we use a variance decomposition method based on adjusted R-squared values (Bertrand and Schoar, 2003) to assess the key drivers influencing workers' probability of separation at the SRA. We find that employer characteristics — and especially unobserved employer-specific factors — play a much larger role than observable worker characteristics. Employer fixed effects explain 17% of the variance in outcomes (21% in the private sector), over four times more than observable worker characteristics, and more than twice as much variation as industry and sector dummies. Allowing for executive fixed effects further increases the explanatory power to 22% overall and 27% in the private sector, suggesting that differences in managerial practices may significantly influence retirement outcomes.

We then use a series of regression analyses to quantify the importance of unobserved firm and executive effects on workers' retirement outcomes (i.e., employer and executive effects). Specifically, we regress the individual separation outcomes of older workers at the SRA on the separation *propensity* of the employer, based on the leave-one-out (L1O) mean of other workers with the same employer and an Empirical Bayes' shrinkage adjustment for sampling error (Chetty et al., 2014a). We observe a strong relationship: a 10 pp increase in employer separation propensity is associated with a 9.3 pp increase in a worker's own SRA separation rate, even when we control for industry fixed effects. However, this relationship may simply reflect differences in the types of older workers across employers within the same industry or in employers' incentives to enforce mandatory retirement. We thus successively add rich controls for the characteristics of workers, firms, executives, and the macroeconomic environment. Our estimates fall slightly but remain large and statistically significant: our richest model implies that a 10 pp increase in employer separation propensity is associated with a 8.4 pp increase in a worker's own SRA separation rate. To the extent that such a relationship is causal, it implies that moving a worker from an employer in the bottom quartile for SRA separation propensity to an employer in the top quartile would raise their SRA separation probability by 31 pp, with an even larger effect in the private sector (36 pp).[5] These

Rabaté et al. (2024) show that hazard rates are large even among wealthy households who are unlikely to be credit-constrained and much larger among employees than the self-employed. Moreover, the aggregate retirement hazard at the SRA is nearly triple that of countries like the UK with similar pension systems but no discontinuity in employment protection (Cribb et al., 2016).

[5]Our aim with this analysis is to get as close as possible to quantifying the importance of employers on workers'

findings provide compelling evidence that unobservable differences across employers — possibly shaped by executives — play a substantial role in influencing retirement decisions, especially in the private sector where the variation across employers is greatest.

Importantly, we show that employer effects interact strongly with pension reforms: older workers who are employed by a high SRA-bunching firm just below the SRA are much more responsive to increases in the SRA. We show this by dividing workers who are employed at age 64.5 into quintiles based on the L1O employer SRA separation propensity. We then compare the employment trajectories of adjacent birth cohorts, who differ in the SRA by 2–4 months, using difference-in-differences and cohort-based regression discontinuity designs. Consistently, the employment gains are more than twice as large for workers with employer SRA separation propensity in the top quintile compared to those in the bottom quintile. These results relate to Deshpande et al.'s (2024) conclusion that employers "shape retirement decisions as they relate to Social Security's full retirement age" (p. 382). However, in contrast to Deshpande et al. (2024), who shows that the 'stickiness' of employers' retirement practices can limit responses to changes in the SRA, our results demonstrate that the SRA separation propensity of employers strongly influences the magnitude of the employment effects of such reforms. This insight suggests that policymakers can make detailed forecasts of the overall and heterogeneous employment effects of raising the SRA — by calculating employer-specific separation propensities at the SRA from administrative data — even without policy variation.

In the next step, we focus on the role that executives play in determining retirement patterns at a firm. In terms of managing the aging workforce, executives can play a role in several dimensions: age-based discrimination in hiring and retaining older workers (Hoffman and Tadelis, 2021), reviewing the use of seniority wages, promoting older workers' employability (Caliendo et al., 2024) and inclusive aging policies (Sonnet et al., 2014). Despite a growing literature exploring the role of CEOs and managers on firm productivity and workforce dynamics, little is known about how managers influence retirement outcomes.

We start by demonstrating a strong link between the separation probability of a worker and the leave-one-out separation propensity of the executive, even once we control for the leave-one-out separation propensity of the employer. Separate identification comes from employers who have a change in executive or executives who have a change in employer. The positive and significant

retirement outcomes. However, causal identification relies on a selection-on-observables approach, as workers are not randomly assigned to employers. Moreover, since retirement is an absorbing state that happens only once per worker, we cannot exploit worker mobility across employers to isolate employer effects via a standard AKM model (Abowd et al., 1999). Thus, we perform an Oster (2019) test of coefficient stability to assess whether omitted variable bias can explain our findings; we estimate a conservative lower bound that is 69% of the size of our preferred estimate, which confirms that employer effects are substantial.

coefficient on executives' separation propensity suggests that executives differ considerably in their propensity to retain older workers even within the same firm.

We then formalize this idea by exploiting managerial changes across firms using a movers' design (Chetty et al., 2014b; Deshpande et al., 2021; Breunig et al., 2024), where executives move between an 'origin firm' and a 'destination firm'. Specifically, we estimate a dynamic differences-in-differences model of older workers' SRA separation outcomes over a six-year period surrounding the date the incoming manager takes control of the destination firm, with a continuous treatment variable equal to the pre-switch difference in SRA separation propensities. We find that a one standard deviation increase in management separation propensity (10 pp) leads to an immediate and sustained increase in older workers' SRA separation probability of approximately 8.3 pp in the private sector and 4.5 pp in the public sector (5.7 pp overall). This suggests that executives transmit more than half of their prior SRA retirement practices when they take control at a new firm, with stronger transmission in the private sector and among male executives.

Accordingly, workplace retirement norms are malleable, and changes in management practices can meaningfully shape retirement practices. We estimate that a hypothetical policy that reduced executives' separation propensities by one standard deviation would increase private-sector employment above the SRA by 21%. To achieve the same increase in private-sector employment with a pension reform, the government would have to raise the SRA by 6 months. These results highlight the importance of managerial discretion and are consistent with surveys of managers, which show considerable variation in mandatory retirement attitudes (Oude Mulders et al., 2014).

Interestingly, we do not see any evidence that the managerial shocks we exploit are endogenous to employers' decisions to expand or contract (i.e., boards do not appoint 'stricter' executives when they are looking to downsize). Moreover, the differences in attitudes do not appear to be correlated with proxies for broader forms of age discrimination or management quality. Using our movers' design, we observe no association between the appointment of a stricter executive and (i) changes in older workers' employment shares and (ii) changes in firms' financial performance (e.g., profits) Thus, there is little evidence to suggest that a shift towards softer mandatory retirement enforcement would be detrimental to firms.

Our paper relates to an emerging literature that emphasizes the importance of employers in the retirement decision using linked employer-employee data (Kondo and Shigeoka, 2017; Frimmel et al., 2018; Rabaté, 2019; Deshpande et al., 2024; Saez et al., 2024). Frimmel et al. (2018) provide evidence that firms react to incentives to push workers into early retirement, based on the relative pay difference between older and younger workers, using an instrumental-variables approach at the local labor market level. Other closely related studies show strong employment effects from policies that strengthen older workers' employment protections, either by banning the

practice of mandatory retirement (Morris and Dostie, 2023) or raising the mandatory retirement age (Kondo and Shigeoka, 2017; Rabaté, 2019; Saez et al., 2024). These studies show that employers matter — and provide some evidence of heterogeneity across employers with different observable characteristics — but little is revealed about the importance of unobserved heterogeneity, due to differences in workplace norms or management practices.[6]

We contribute to this literature by thoroughly investigating and documenting the importance of unobserved heterogeneity across employers and executives for retirement outcomes in a setting where employers can strongly influence the retirement process. Our administrative data spanning 19 years allows us to precisely define when job separations and retirements occur and link all workers who reach the SRA with the same employer or executive. Our results show considerable unobserved heterogeneity across both employers and executives, even after controlling for a rich set of individual and firm-level controls, including workers' earnings, the nature of the work (industry) and the average age-wage gradient at the firm. These findings contrast with theoretical rationales for the existence and enforcement of mandatory retirement (Lazear, 1979; Saez et al., 2024), which suggest that firms rationally enforce mandatory retirement on a case-by-case basis when a worker's pay exceeds their value to the firm. Moreover, our movers' design suggests that managerial discretion plays a key role, which is inconsistent with such models.

Our paper also relates to a literature on the causes of the widespread and policy-relevant phenomenon of bunching at statutory retirement ages (Staubli and Zweimüller, 2013; Cribb et al., 2016; Lalive et al., 2023; Saez et al., 2024; Coile et al., 2025; Geyer and Welteke, 2021). The literature has emphasized a variety of factors influencing labor supply that may contribute to bunching, including liquidity constraints, high implicit tax rates on labor supply, and reference dependence. However, less is known about the importance of employers, with a handful of exceptions that either confirm (Kondo and Shigeoka, 2017; Deshpande et al., 2024; Rabaté, 2019; Rabaté et al., 2024) or reject (Coile et al., 2025) the aggregate importance of employers in different settings. We contribute to this literature by documenting tremendous unobserved heterogeneity in the amount of bunching across employers in the Dutch setting and showing that this heterogeneity strongly influences the employment effects of pension reforms. We also provide the first evidence on the role of managers in driving retirement bunching, which is facilitated by our comprehensive administrative data and analysis of managerial changes. Our results show that workplace norms and managerial

[6] Deshpande et al. (2024) provide some evidence of unobserved employer heterogeneity for benefit claiming and retirement in the context of increases in the Full Retirement Age in the US. They show that workers who move firms in their 50s show some conformity to the average retirement practices of workers at the destination firm, even after accounting for origin firm fixed effects. However, these estimates are subject to measurement error in both the timing of the retirement decision — which can only be measured at an annual frequency — and coworkers' retirement practices, as a 10% random sample of Americans is used. These data limitations also prevent the authors from investigating the importance of specific mechanisms underpinning employer effects, such as management practices.

practices can strongly influence bunching at the SRA, which highlights both a challenge and an opportunity for policymakers to raise old-age employment without further pension reforms.

2 Background and Data

2.1 Dutch Pension System

The Dutch pension system consists of three pillars. The first pillar is a flat-rate public pension (*AOW, Algemene Ouderdomswet*), which is indexed to the minimum wage and financed by pay-as-you-go social insurance contributions. In 2019, this pension amounted to €1,298.19 per month for singles and €1,787.52 per month for couples. All Dutch residents can receive the first-pillar pension once they reach their birth cohort-specific "AOW age" or Statutory Retirement Age (SRA). The SRA was 65 years old before 2013. It increased by one month per year in 2013–15, three months per year in 2016–18 and four months in 2019. For cohorts born between January 1948 and December 1955, the SRA increased from 65 to 67 (Figure A.1a). Early claiming of the first-pillar pension is not possible. Moreover, individuals lose eligibility for unemployment and disability benefits after reaching the SRA.

The SRA constitutes the endpoint of most employment contracts, which means that individuals who wish to continue working past the SRA have to draw up a new contract. Similar "mandatory retirement" age thresholds are common policies in many OECD countries (Van Vuuren, 2014; Saez et al., 2024), particularly those with more stringent employment protection legislation, such as Germany, France, and the Netherlands.

The second pillar consists of mandatory employer contribution schemes, which are collective pension schemes connected to a specific industry or company. Through the second-pillar schemes, employees can retire earlier (i.e., before the SRA). The third pillar, which consists of individual savings for retirement, is less important than in other developed countries (Grip et al., 2012).

2.2 Mandatory Retirement and Rehire Practices

In the Netherlands, mandatory retirement practices are closely tied to the statutory retirement age (SRA), which is embedded in employment law and typically triggers an automatic termination of the employment contract when reached. Employers are legally permitted to dismiss employees at the SRA without the usual dismissal procedures or severance obligations, making mandatory retirement a widely used mechanism for workforce renewal and personnel planning. At the same time, Dutch labor law allows employers to rehire workers after they reach the SRA on flexible, temporary contracts, often with fewer employment protections and lower non-wage costs. This

dual structure — automatic dismissal at SRA combined with the legal option to rehire — creates a system in which firms and executives have substantial discretion over whether older workers can continue working after statutory retirement, and under what terms.

Survey evidence underscores the central role of employers in shaping these post-retirement employment opportunities. Employers largely determine whether continued work is possible, the tasks older workers can take on, and the conditions under which they are rehired. For example, Oude Mulders et al. (2014), using a 2017 Dutch employer survey, demonstrates that employer attitudes and expectations are pivotal: organizations that view older workers as productive, reliable, or difficult to replace are significantly more likely to employ working retirees. Conversely, firms concerned about productivity, physical demands, or administrative burdens are less inclined to offer post-retirement roles. These attitudes often outweigh objective measures of worker performance, highlighting the subjective and discretionary nature of rehire decisions.

Executives and upper-level managers likely play an especially influential role in this process. They shape firm-wide HR policies on mandatory retirement, allocate budgets for continued employment, and set the tone for organizational norms regarding aging workers. Thus, variation in executive leadership likely contributes substantially to heterogeneity in post-retirement employment practices across Dutch firms.

2.3 Data

Our empirical analyses explore linked-employer-employee administrative data provided by Statistics Netherlands (2006–24). The baseline sample comes from several linked population registers. The cornerstone of our dataset is the SPOLISBUS register, which is a monthly payroll register that tracks the full population of workers living in the Netherlands from January 2006 to June 2024. The key information is workers' earnings and work hours (both regular and overtime) and a set of hashed IDs (individual and firm). Individual IDs are used to link relevant information from other population registers, such as the worker's gender and month-year of birth.

2.4 The Aggregate Retirement Spike at the SRA

To quantify retirement bunching at the SRA, we start with the universe of Dutch residents born between 1941 and 1957, who reach the SRA within the span of our data. Over this period, the SRA progressively increased from 65 to 67 based on month and year of birth (Figure A.1). Our analysis of aggregate retirement bunching will focus on workers' transitions to retirement in the 12 months either side of the SRA. We define retirement as an event that occurs when the individual stops working for at least 6 months. That is, the worker i retires in the month t if $\text{employed}_{i,t} = 1$

and employed$_{i,t+k} = 0$ for $k \in [1, 6]$.

Figure 1 shows the monthly hazard of exiting the labor market around the SRA for Dutch workers. We observe a huge increase in the retirement hazard in the three months around the SRA, with the largest increase in the month the worker reaches the SRA. The retirement hazard is nearly 50% in the SRA month (and elevated in the previous and subsequent months). Beyond the SRA, the retirement hazard appears to remain elevated. In Appendix A, we estimate a 64.9-percentage-point increase in the retirement hazard in the three months around SRA, relative to the months before. Moreover, the retirement hazard remains elevated in subsequent months compared to the level prior to the SRA. Overall, we estimate that around 71% of Dutch workers who are still working near the SRA retire within one year of reaching it.[7] The magnitude of the estimate is broadly consistent with earlier studies that focused on a narrower set of birth cohorts (Rabaté et al., 2024).

3 Firms' Role in SRA Bunching

What role do firms play in the high level of retirement bunching at the SRA observed in the Netherlands? We investigate this question by examining the key factors influencing the average job separation rate at the SRA. We focus on separations because they are the main margin through which firms affect workers' retirement timing.[8]

3.1 Analysis Sample and Descriptive Statistics

Our definition of job separations at the SRA considers individuals who are still employed by a firm in event-month -1: the month before their SRA. We refer to these workers as "focal workers". Our definition of SRA separations allows for separations over a 3-month period: that is, separates$_{if} = 1$ if worker i is no longer employed by firm f in event-month 2 (otherwise separates$_{if} = 0$).[9]

To study the influence of firms, we restrict the set of older workers and firms in several ways. First, we restrict the set of focal workers to have at least 12 months of consecutive employment in the firm by event-month -1 and to be employed by only one firm in that month. Second, we focus on firms that had at least five focal workers, since we cannot learn much about the importance of firms in influencing SRA separations if firms have very few workers reaching that age.[10] Third, we

[7] $71\% = 0.69 + (1 - 0.69) \times 0.075$.

[8] Indeed, a very high proportion of job separations at the SRA coincide with workers exiting employment and retiring.

[9] Figure 1 shows that the spike in workers' retirement hazard around the SRA starts in event-month -1 and continues until event-month 1, so we allow for job separations to occur in any of these three months.

[10] For example, firms with a single worker reaching the SRA will either have an average retirement hazard of 0% or 100%. Moreover, these workers are often the owners of the business.

exclude a small fraction of older workers who could not be assigned to an establishment. In total, we retain 57.3% of all Dutch workers who reach the SRA (see Table A.1).

We summarize the characteristics of older workers (N=384,775) and firms (N=13,936) in the sample in Table 1. Among older workers, 40.1% are female, 90.1% are permanent employees, the average SRA is 65.9 years, and older workers rank at the 57th percentile within their firms for hourly wages and at the 48th percentile for hours worked. Our focus on firms with at least five older workers reaching the SRA means that the average firm is large (489 employees) and most have multiple establishments (59.3%). It also means that the private sector is underrepresented relative to total old-age employment: 45.4% of older workers in the sample are in the private sector, which is lower than the share for all jobs with workers just below the SRA (62.4%). Among firms in the sample, the average wage-age gradient is a 1.4% increase in hourly wages per year of age.

The last panel of Table 1 shows the characteristics of the highest-earning worker in the firm at the time, whom we call the executive. On average, the highest earners are mostly male (82.6%), around 52 years old, and earn six times the average salary within the firm €17,167. There are 48,653 unique executives in this sample. We validate our interpretation of highest-earners as executives by linking a subset of highest-earners in the sample to their contemporaneous responses in the Dutch Labour Force Survey (LFS).[11] Based on the International Standard Classification of Occupations, 79% of individuals are classified as managers, and, of those asked, 93% report managing others, with 70% of these individuals managing at least 20 people. Moreover, 96% report managing personnel decisions and 95% report managing strategy decisions. Overall, these responses suggest that the highest earners have influential positions within their firms and are can influence retirement policies. Previous studies have also shown that top earners are usually top executives, partners, or top-level managers. For example, Bakija et al. (2010) shows that 40% of US taxpayers in the top 0.1% of the distribution are executives, managers or supervisors. Song et al. (2019) assume that highest-earners are the chief executive.

3.2 Differences in SRA Separations Across Firms

To show that firms play a role in shaping the retirement bunching at the SRA, we begin by documenting how the average job separation rate at the SRA varies across firms. While some variation is expected due to random factors — such as differences in workers' retirement preferences — the magnitude and pattern of variation across firms can be informative about the influence firms exert on retirement timing.

[11]The LFS is a nationally representative quarterly survey with a rotating panel — individuals are re-interviewed in the panel for up to 5 quarters — that contains detailed information on individuals' occupation and job characteristics. We managed to link 1,939 observations on 748 highest-earners, which is approximately 1.5% of the highest-earners in the sample.

Figure 2 shows the distribution of firm-specific SRA separation rates (solid black line). We weight firms by the number of older workers (i.e., the denominator of their average separation rate), which means larger firms and firms with more older workers matter more.[12] In panel a, we show the distribution for all employers, while the other panels focus on firms in the private sector (panel b) and public sector (panel c). Overall, we observe considerable variation across firms, with a standard deviation of 0.19 around the mean of 0.78. For example, approximately 15% of workers work in a firm with a separation rate of at least 0.95, but a similar proportion have a rate below 0.6 (15%). There is also a large mass of older workers in firms with the maximum possible separation rate of 1 (5%).

How large is this variation across firms? One useful benchmark to consider is the case where the separation outcomes of coworkers are independent. In this case, we may expect less variation in the firm-specific separations because we explicitly rule out relevant firm-level factors, such as correlations in coworkers' job characteristics, peer effects and employer decision-making. To implement this comparison, we create a pseudo-distribution of firm-specific separation rates after randomly reshuffling workers across the firms in our sample (dark gray dotted line).[13] As shown in Figure 2, this pseudo-distribution has a bell-shape and is much less dispersed, with a standard deviation of 0.079, which is 42% as smaller than the observed distribution. In fact, the observed variance in firm-specific separation rates is 4 times larger than would be expected by chance.

A valid critique of this analysis is that we are not controlling for other factors that may vary between workers in different firms, such as the type of work involved. Workers in some jobs, for example, may be more likely to voluntarily retire at the SRA than other workers. However, we observe no correlation, neither positive nor negative, between the firm-specific SRA separation rate and job separation rates at slightly younger ages.[14] This suggests that workers in high-separation firms are not intrinsically more likely to retire for other reasons (e.g., because their jobs are more taxing).

Moreover, we show in Figure 2 that the excess variation in firms' SRA separation rates persists within industries. To implement this comparison, we create a pseudo-distribution of firm-specific separation rates after randomly reshuffling workers across firms in the same industry (gray dashed line). While industry-specific factors clearly explain some of the excess variation across firms,

[12] We can thus interpret this distribution as the distribution of firm-specific SRA separation rates faced by the population of older workers (who work in sufficiently large firms). Older workers are the focal workers who are employed by a firm in one month before reaching SRA.

[13] We maintain the distribution of firm sizes (i.e., the number of older workers in each firm) to ensure comparability across the two distributions.

[14] We measure the average firm-specific separation rates among workers in the three years prior to the SRA, restricting the set of workers to those with at least 12 months continuous tenure (in months -48 to -37 relative to the SRA). Our estimates are weighted by the number of older workers in the firm who reach the SRA.

about half is unexplained, suggesting that "firm effects" matter in addition to "industry effects".[15] This highlights the influence of firm-specific factors on workers' retirement timing.

Excess variation in the firm-specific SRA separation rate is widespread, appearing in both the private and public sectors, yet it is particularly stark in the private sector (panel b). There, we observe much more distribution, with a much larger standard deviation (0.232) compared to the public sector (0.123). The two pseudo-distributions fail to capture this dispersion, particularly at the top and bottom of the distribution. For example, it is twice as common to observe private-sector firms with a separation rate of 100% than would be expected by chance, and 17-times more common to observe private-sector firms with a separation rate below 30%.

3.3 Variance Decomposition and Observable Characteristics

Next, we use a variance decomposition method similar to Bertrand and Schoar (2003) to show how different components, such as worker characteristics, firm characteristics, sector and executive characteristics, and unobserved firm-level and executive-level factors account for the individual separation outcome at the SRA. Rows 1 to 5 of Table 2 report the adjusted R^2 when we regress separates_{if} on different sets of controls, including (1) a rich set of worker characteristics; (2) firm characteristics; (3) industry and sector dummies; (4) the characteristics of executives / high-earners within the firm; and (5) time fixed effects.[16] Individually, these controls explain only a relatively small share of the variance in workers' SRA separation outcomes, ranging from 1% to 9% when we do not condition on sector. Industry and sector dummies are the most important, explaining 9%, while worker and firm characteristics explain 4% and 5% respectively. Time fixed effects explain less than 1% of the variation. Combined, these five types of controls (with 29 different variables and five dimensions of fixed effects) explain 13% of the variance in the full sample (row (6)) . Interestingly, columns (2) and (3) show that the adjusted R^2 values are consistently about twice as high for workers in the private sector. For example, the full list of controls explains 15.8% of the

[15]The standard deviation of the within-industry pseudo-distribution is 0.135, approximately half way between the standard deviation of the observed distribution and the other pseudo-distribution.

[16] The full list of controls is as follows: (1) worker characteristics —- a female dummy; SRA-cohort dummies; a dummy for having a regular employment contract; dummies for being a 1st / 2nd generation immigrant; pre-SRA earnings, hours worked and hourly wage in 2024 €(and the worker's percentile ranks within the firm for each); a dummy for being in the top 5% of earners within the firm; a dummy for being the highest earner; and a dummy for being a director or major shareholder of the firm; (2) firm characteristics — dummies for each decile of firm size; a dummy for having multiple establishments; the number of establishments; the average age, earnings, hours worked and hourly wages of workers in the firm; the share who are female, immigrants; the wage-age gradient in the firm; and the average separation rate of older workers in the firm in the three years prior to the SRA; (3) industry dummies at the two-digit level; (4) time fixed effects at the calendar month-year level; and (5) executive characteristics — a dummy for having a director / major shareholder observed in the data; the number of directors / major shareholders; average age, earnings and female share of the directors; age, earnings and a female dummy of the highest earner in the firm; average age, earnings and female share of the top 5% earners within the firm.

variance in the private sector and only 8.5% in the public sector.[17]

In row (7), we demonstrate the importance of unobservable differences across firms: by themselves, firm fixed effects have an adjusted R^2 of 0.17 for all employers and 0.21 in the private sector. These values are approximately 35% higher than the full list of controls. In the public sector, the adjusted R^2 terms are also higher with firm fixed effects, but the increase is smaller. Across all columns, the adjusted R^2 values increase further when we augment our firm fixed effects model with the full list of controls. In row (8), we show that we achieve slightly higher adjusted R^2 terms if we allow the firm fixed effects to vary across establishments, while executive fixed effects achieve even higher adjusted R^2 values (row 9). Our richest model, with executive fixed effects and the full list of controls, attains an adjusted R^2 value of 0.22 for all employers and 0.27 in the private sector.

Overall, this exercise highlights a number of interesting patterns. First, firm effects appear important in determining workers' separation outcomes at the SRA: firm fixed effects explain five-times as much of the variation in workers' outcomes as workers' own characteristics, and more than twice as much as industry and sector fixed effects. Second, establishment-level factors also seem to matter, but differences across establishments appear to be much less important than differences across firms. Third, differences in HR practices across executives may be an important component of the firm fixed effects. Fourth, the higher R^2 values with executive fixed effects suggest that firm policies are not set in stone.

3.4 Quantifying Firm Effects

While the descriptive analysis above highlights the importance of firms in explaining the variation in individuals' SRA separation outcomes, it is subject to several limitations. First, since older workers only reach the SRA once in their lives (and typically when employed by a single firm), it is difficult to disentangle unobserved individual and firm effects. Second, the R^2 terms with firm fixed effects may (slightly) overstate the importance of firms effects, since a worker's own

[17] We observe several interesting correlations between individuals' binary SRA separation outcomes and worker/firm characteristics. First, in terms of worker characteristics, we find considerably high SRA separation rates for female workers (7.3pp), while separation rates are decreasing in the earnings rank of the worker within the firm. Second, in terms of firm characteristics, we observe smaller SRA separation rates for firms in the bottom three deciles of firm size, with the smallest effects in the bottom decile. Conditional on the full set of firm covariates, we observe a positive and significant relationship between SRA separation rates and the wage-age gradient in the firm, consistent with theoretical predictions (Lazear, 1979). However, the pre-SRA separation rate of older workers within the firm is negatively correlated with SRA separation rates. This means that high SRA separation rates are not due to high separation rates in general, and it suggests that firms are the main instigators of the separations at the SRA. Third, most of the above relationships weaken considerably once we control for industry and sector dummies and month-year fixed effects, although a strong positive relationship between SRA separations and having a regular employment contract emerges (13.3pp).

outcome partly determines the mean separation rate in his/her firm, mechanically inflating the R^2 terms. Although we use adjusted R^2 terms for this reason, our analysis may still be subject to overfitting. Third, the mechanisms underpinning the firms effects remain unclear.

To address these limitations, we quantify the importance of firm effects using a series of regressions. Our basic approach is to estimate the relationship between an individual's SRA separation outcome and the separation propensity of their coworkers.[18] Following the literature (Mas and Moretti, 2009; Cornelissen et al., 2017), we construct leave-one-out (L1O) mean SRA separation rate of workers in the same firm, as follows:

$$\text{L1O_Firm}_{if} = \left(\frac{\sum_{j \in f, j \neq i} \text{separates}_{jf}}{N_f - 1} \right)$$

where the L1O mean SRA separation rate for worker i in firm f is the mean of all other workers in the same firm over our sample period. Since we are after a measure of the firm's separation *propensity*, we adjust for sampling error using an Empirical Bayes adjustment. This adjustment shrinks the L1O means towards the global mean in the sample, with the amount of shrinkage depending on the sampling error:

$$\text{L1O_Firm}_{if}^{EB} = (1 - \lambda_{if})\mu + \lambda_{if}\text{L1O_Firm}_{if}, \quad \text{with} \tag{1}$$

$$\lambda_{if} = \frac{\sigma^2_{true}}{\sigma^2_{true} + \frac{\mu(1-\mu)}{N_f - 1}} \tag{2}$$

where σ^2_{true} is the true across-firm variance after removing sampling error and $\frac{\mu(1-\mu)}{N-1}$ is the Binomial sampling variance of a sample mean with $N_f - 1$ observations. Intuitively, the degree of shrinkage $1 - \lambda$ is inversely related to N_f: when N_f is large, λ converges to 1.[19]

The regression equation is the following:

$$\text{separates}_{ift} = \alpha + \beta\text{L1O_Firm}_{if}^{EB} + X_{ift} + \epsilon_{ift} \tag{3}$$

where i denotes an individual older worker in firm f who is just about to reach the SRA at time t;

[18] We cannot exploit the commonly used method of comparing behaviors of workers moving between different firms to address possible selection biases. This is because separation at the SRA (retirement) occurs only once for each individual. This is also why we cannot implement the widely used Abowd, Kramarz, and Margolis (AKM) (Abowd et al., 1999) two-way fixed effects model, which would rely on observing individuals' retirement behaviors across multiple firms to separately identify worker and firm effects.

[19] To implement the Bayesian adjustment, we set $\mu = \hat{\mu}$, the global mean SRA separation rate in the data and input N_f into (2). To estimate σ^2_{true}, we subtract an estimate of the variance due to sampling error (0.006) from the total variance in the sample (0.035). Our estimate of the variance due to sampling error comes from randomly reshuffling workers across firms (while maintaining the same distribution of firm sizes), recomputing the L1O means and then calculating the variance.

the term in parentheses is the leave-one-out (L1O) SRA separation propensity of older workers in the firm; X_{ift} is a set of individual-level, firm-level and time controls (see footnote 16 for the full list); and ϵ_{ift} is an idiosyncratic error term. We cluster standard errors by firm. The coefficient of interest is β, which shows the relationship between workers' SRA separation outcomes and those of other workers in the same firm: $\beta = 1$ would imply a perfect correlation, while $\beta = 0$ would imply that there is no correlation. Positive values of β suggest that there is a common factor — the firm — that determines the SRA separation rates of workers. As expected, a placebo regression that rules out this common factor — by randomizing workers across firms and recalculating leave-one-out separation rates — yields a precise-zero estimate of β.

We present the estimates in Table 3. In all models, we include industry and sector dummies, which is important to account for mandatory retirement provisions in collective agreements, which differ across industry. The estimate of β in column 1, which includes no other controls, is 0.925 and highly statistically significant ($p < 0.001$). This means that a 10pp increase in the average SRA separation rate of coworkers is associated with a 9.3pp increase in the worker's own separation probability, even after controlling for differences in separation probabilities across industries and sectors. Nonetheless, this relationship may not be causal evidence of *firm effects*, since coworkers' SRA separation rates may be positively correlated for other reasons. Thus, to get closer to causality, we progressively add detailed controls at multiple levels in columns 2–5. We see that the estimates fall with both the individual- and firm-level controls, while controlling for the characteristics of the executive(s) and adding time fixed effects barely moves the coefficient. In all models, the estimated β coefficient remains large and highly statistically significant. Our preferred estimate, with the inclusion of all controls, indicates that a 10pp increase in the average SRA separation rate of coworkers is associated with a 8.4pp increase in the worker's own SRA separation probability. This estimate implies that moving a worker from the bottom quartile to the top quartile of firm SRA separation propensity would raise their own SRA separation probability by 31pp.[20]

To what extent could omitted variable bias (i.e., spurious correlations in coworkers' SRA separation propensities) explain our positive estimate of β? To assess this question, we perform an Oster (2019) test of coefficient stability using the associated Stata command `psacalc`. This test uses information on (i) the stability of treatment coefficients when additional controls are included and (ii) R^2 movements to determine a lower bound for β that is robust to selection on unobservables. Following Oster's (2019) recommendations, we impose an equal selection assumption (i.e.,

[20]We examine how the estimated effects vary between the private and public sector. Although the point estimates of β are similar in both sectors, there is much more variation in separation rates across firms in the private sector. This means that firm effects are much more important in the private sector. For example, moving a worker from a bottom-quartile private-sector firm to a top-quartile firm (in terms of SRA separation propensities) would raise the worker's SRA separation rate by 36pp, while the comparable effect is 22pp within the public sector.

selection on observables and unobservables are equally important) and assume that the R^2 on the hypothetical regression of all observable and unobservable controls would be 30% higher than the R^2 with all observable controls.[21] Under these assumptions, we estimate a lower bound for β of 0.577, which is 69% of the size of our preferred estimate and well above 0. Thus, we have strong evidence that the *firm effects* documented in Table 3 are robust to selection on unobservables.

3.5 Implications for Retirement Policies

The firm effects documented above suggest that retirement policies are likely to have very heterogeneous impacts on old-age employment across workplaces. For example, reforms that raise the SRA will likely have much stronger effects on old-age employment in firms that have high SRA separation propensities. To test this assertion, we exploit the six discrete increments in the SRA, by three or four months, from 65 years and 3 months to 66 years and 10 months. We match older workers to their firms at an age close to but below these ages (63 to 64.5 years old) and show how the employment effects of the SRA policies vary based on the SRA separation rate of older workers' firms.[22] Specifically, we follow previous work (Rabaté et al., 2024) by estimating regression discontinuity (RD) regressions based on month of birth and focusing on employment at the first age above the control group's SRA.[23] For example, we estimate the employment effects of the increase in the SRA from 65 years and 3 months to 65 years and 6 months at the age of 65 years and 4 months. At this age, the treatment group remains below the SRA while the control group has reached the SRA. Figure 3 shows that the employment effects of this particular SRA increment depend strongly on the SRA separation rates of the firm. The estimated effects are more than twice as large for workers in firms with SRA separation rates in the top 20% (80.8pp) compared to the bottom 20% (39.2pp). This is driven by the much larger decline in employment rates at the SRA for workers in high-bunching firms in the control group, as there is little difference between workers' employment rates within the treatment group (i.e., for workers who remain below the SRA due to the policy). We observe extremely similar results when we estimate the employment effects of the other five (subsequent) increases in the SRA (Appendix Figures A.3b–f).

These results indicate that the behavior of employers, in ousting older workers at the SRA, can magnify the employment effects of increases in the SRA. This finding is related to Deshpande et

[21] For this test, we assume the 'uncontrolled' regression contains industry and sector dummies (Table 3, column 1) but no other controls.

[22] We consider older workers who are (i) continuously employed for a single firm at these ages and (ii) work for a firm that belongs to our analysis sample (with at least five older workers who reach the SRA). Previous research shows no effects of these SRA policies on employment at any age below the SRA (Ferrari et al., 2023; Rabaté et al., 2024), and no evidence that the policies affect how older workers sort between firms (Ferrari et al., 2023).

[23] We present bias-corrected RD estimates and robust standard errors using the `rdrobust` Stata command (Calonico et al., 2017). Following Rabaté et al. (2024), we use a nine-month bandwidth, a uniform kernel and local-quadratic polynomials on either side of the threshold.

al.'s (2024) conclusion that employers "shape retirement decisions as they relate to Social Security's full retirement age" (p. 382). However, their study shows that the 'stickiness' of employers' retirement practices can actually limit the responses to changes in the SRA, while our results demonstrate that the SRA separation propensity of employers strongly influences the magnitude of the employment effects of such reforms. This insight may be useful for policymakers who are considering an increase in the SRA, since firm-specific separation propensities at the SRA can be easily calculated from administrative datasets, even when there is no variation in the SRA. Understanding this information will allow policymakers to make richer forecasts of the likely employment effects of raising the SRA, and in which types of organizations these effects will be largest.

To illustrate this point, we show how the employment effects of raising the SRA vary between the public and private sectors. SRA separation rates are considerably higher in the public sector, a phenomenon that is common across countries with mandatory retirement policies (Morris and Dostie, 2023; Saez et al., 2024; Rabaté, 2019). This suggests that increasing the SRA will disproportionately increase old-age employment among public servants, which likely is an unintended consequence given that such policies aim to reduce government expenditure. To test this conjecture, we re-estimate the employment effects of the SRA increments by sector using the same RD design.[24] As expected, the estimated employment effects of each SRA increment are considerably larger (about 20%) among workers in the public sector.

4 Executives' Role in SRA Bunching

4.1 Quantifying Executive Effects

In this section, we ask the question to what extent the differences in SRA separation rates across firms are driven by different HR policies (e.g., regarding rehire after mandatory retirement)? And are these policies immutable, or can they be influenced by leaders within the firm?

Through a vignette experiment among Dutch managers, Oude Mulders et al. (2014) find that Dutch managers play an important role in the decision to rehire employees after mandatory retirement, which is the key margin to reduce the separation probability at the SRA. They show that managers are overall disinclined to rehire employees after the SRA, but large differences exist between employers. Therefore, it is reasonable to expect that executives may exert significant influence over firm-level retirement behavior, either through formal policies, managerial culture, or implicit norms.

[24] We broaden the analysis sample to include all workers who work for a single employer from 63 to 64.5 (i.e., including those who work for smaller employers), since we do not require information on firm-specific SRA separation rates here.

To assess these questions, we study how workers' SRA separation outcomes are correlated when they reach the SRA under the same executive (the highest-earning worker in the firm). Specifically, we define the leave-one-out SRA separation rate of older workers who reach the SRA under the same executive (either within the same firm or another firm), $\text{L1O}_{ie} = \left(\frac{\sum_{j \in e, j \neq i} \text{separates}_{je}}{N_e - 1}\right)$, where e the executive. Then, after implementing a similar Empirical Bayes adjustment to that described above, we re-estimate equation (3), where we augment the L1O firm separation propensity with the L1O executive separation propensity.[25] Specifically, we estimate the following model

$$\text{separates}_{ift} = \alpha + \beta_f \text{L1O_Firm}_i^{EB} + \beta_e \text{L1O_Executive}_i^{EB} + X_{ift} + \epsilon_{ift} \tag{4}$$

Column 2 of Table 4 shows that the estimates of β_f (0.471) and β_e (0.453) are both positive and highly statistically significant. This suggests that we have sufficient variation in separation rates across executives within the same firm, and firms' SRA separation propensities are not immutable. To illustrate the magnitude of these effects, consider an older worker i in a firm that has two executives over its history: executive A and executive B. Suppose that, among i's coworkers, executive A always enforced mandatory retirement and never re-employed older workers (100% separation rate), while executive B re-employed 50%. The estimates suggest that the older worker will be 23pp more likely to separate from the firm at the SRA if they reach the SRA under executive A instead of executive B.

While these results suggest that executives matter, it is difficult to completely rule out other factors, such as firm-by-time effects. This is particularly the case because most executives are only linked to a single firm in our sample. Thus, we further restrict the sample to observations where the executive is linked to at least five observations in firm f and at least five observations in other firms. Then we augment equation (4) with the L1O separation propensity of the executive-firm pair, with coefficient $\beta_{e,f}$. In this augmented model, we are interested in the coefficients of β_e and $\beta_{e,f}$. Identification comes from differences in the separation propensities of the executive and executive-firm pair, which arise because executives are linked to multiple firms. Such differences are likely to be larger when the number of observations linked to the executive in the focal firm $N_{e,f}$ is small relative to the total number of observations linked to the executive N_e.

We find that both the estimates of β_e (0.086) and $\beta_{e,f}$ (0.454) are positive and statistically significant at the 1% level.[26] The positive estimate of β_e suggests that executives have intrinsic differences in their SRA separation propensities and (at least some of) these differences persist across firms, even after accounting for a rich set of control variables and unobserved firm-specific het-

[25] We use a slightly modified sample, excluding observations where individual executives are linked to fewer than five older workers (to mirror our sample restriction on firms). This reduces the size of the sample by about 20%.

[26] $\beta_f = 0.340$

erogeneity. Meanwhile, the positive estimate of $\beta_{e,f}$ suggests that the executives' SRA separation propensities within the same firm are especially relevant. This could indicate that executives' separation propensities at least partially depend on firm-specific factors, such as wage-age gradients, collective agreements and the financial position of the firm.

4.2 Impact of Managerial Changes

The evidence above suggests that executives often differ in their approach to rehire after mandatory retirement (separation at the SRA), with some executives much more open to older workers continuing with the firm beyond the SRA than others. This suggests that managerial discretion may be important for explaining the differences in retirement outcomes across firms and executives. To get closer to causality, we adopt a 'movers design' (Chetty et al., 2014b; Deshpande et al., 2024; Breunig et al., 2024), exploiting changes in a firm's executive (i.e., highest-earner) on the separation rate of older workers in the firm at the SRA.

4.2.1 Main Results

Our basic approach is as follows. First, we identify incoming executives who have previously been executives of one other firm in our sample.[27] For these executives, we define the origin firm (f') as the firm that the executive worked for first, and the destination firm (f) as the firm the executive works for subsequently.[28] Second, we examine how the SRA separation rate at the destination firm, f, evolves in the three years before and after the new executive takes over.[29] Specifically, we are interested in understanding whether the changes in the SRA separation rates after the switch can be predicted by differences in SRA separation propensities prior to the switch. Our treatment variable, $\text{shock}_f = \text{sep_propensity_pre}_f - \text{sep_propensity_pre}_{ef'}$, is thus defined as the SRA separation propensity at the destination firm f prior to the managerial switch minus the separation propensity at the origin firm f' while the incoming executive e was in charge.[30] The

[27] For conceptual simplicity, we limit our focus to executives who are observed as the highest earner in exactly two firms in our sample (i.e., those containing at least five older workers who reach the SRA).

[28] Although we do not rule out executives working for intermediate firms or initially working for the destination firm in a non-executive position (since such restrictions would weaken statistical power), most executives move from the origin firm to an executive position at the destination firm within 12 months of leaving the origin firm (61%). Our results are robust to considering only these moves.

[29] We require the incoming executive (e) to be observed in the same role for more than 12 months. On average, incoming executives last 54 months, and 64% last until the end of our 36-month post-period window.

[30] In our baseline estimates, we limit the sample to firms where there are at least four workers used to define the shock_f (at least two in both the origin and destination firms), which matches our L1O analysis (as at least four workers must contribute to the L1O mean).

Table 5 shows the summary statistics for the switching sample. Our baseline switch sample contains 659 destination firms, 21,037 older workers, and 2,202 unique executives, which is about 5% of the broader analysis sample. Workers in this sample work in large organizations with 946 workers, as compared to 489 in the LIO analysis sample. The

executives have an average age of 55 and 24% of them are women. They earn around 20,000 euros per month.

To facilitate interpretation, we rescale shock_f in terms of the number of positive or negative standard deviations (a one-standard deviation shock is a 10pp change in the SRA separation propensity).We then estimate the following dynamic difference-in-differences (DiD) regressions for older workers in destination firms:

$$\begin{aligned}\text{separates}_{ift} =& \alpha \text{shock}_f + \sum_{k=-36}^{30} \beta_k \left(\text{shock}_f \times \mathbf{1}(\text{event_time}_{ft} \in [k, k+5])\right) + \\ & \sum_{j=-36}^{35} \gamma_j \mathbf{1}(\text{event_time}_{ft} = j) + \theta_t + X_{ift} + \epsilon_{ift} \end{aligned} \tag{5}$$

where separates_{ift} is a dummy variable equal to 1 if older worker i separates from firm f at the SRA (or in the months either side) and 0 if they continue working for the firm, and t is the calendar month that falls two months before i reaches the SRA;[31] post_{ft} is a dummy variable equal to 1 for $\text{event_time}_{ft} \in [0, 35]$ (i.e., after the incoming executive takes over at the destination firm); $\sum_{j=-36}^{35} \gamma_j \mathbf{1}(\text{event_time}_{ft} = j)$ is a complete set of event-time fixed effects;[32] θ_t is a set of month-year fixed effects; X_{ift} is a vector of individual, firm, industry and sector controls (see footnote 16); and ϵ_{ift} is an idiosyncratic error term. Standard errors are clustered by firm.

Figure 4 shows the results. For the full sample of employers (panel a), we observe no evidence of any pre-trends, with or without the inclusion of controls X_{ift}.[33] After the managerial switch, the estimates become positive and statistically significant at the 5% level. The effects are observed within the first six months of the switch and do not appear to grow or decay over the next 2.5 years. In terms of magnitude, the estimates indicate that a positive managerial shock (i.e., appointing an executive with a 10pp higher SRA separation propensity than) increases the separation rate of workers reaching the SRA by approximately 5.7pp. These results suggest that differences across executives transmit most of their prior retirement practices to the new firm and have substantive effects on the retirement timing of older workers.

Overall, the point estimates display little sensitivity to the inclusion of controls, although the estimates with controls are around 20% more precise. This lack of sensitivity suggests that the estimated changes in separation rates cannot be explained by other fundamental changes in the

majority are in the public sector (70%).

[31] Our thought process behind this choice for t is that workers and firms would likely discuss whether a worker will retire at the SRA — or negotiate a new employment contract — before the worker reaches the SRA.

[32] event_time_{ft} is the number of calendar months since the incoming executive takes over.

[33] F-tests of the joint significance of the five pre-period event-time coefficients yield p-values well above 0.1 for all panels of Figure 4 (see Table 6).

firms' characteristics or the type of older workers who are reaching the SRA that coincide with the managerial switch.

Furthermore, we demonstrate in the Appendix that the managerial switches in our sample are associated with only minor changes in the firms' characteristics (Appendix Figure A.4),[34] and, most importantly, that there is no association between these changes and our treatment variable shock_f (Appendix Figure A.5).

We also estimate a more parsimonious model with a single treatment effect. This allows us to summarize the overall effects and more transparently assess effect heterogeneity and robustness. We estimate the following regressions:

$$\text{separates}_{ift} = \alpha \text{shock}_f + \beta(\text{shock}_f \times \text{post}_{ft}) + \theta_t + \sum_{j=-36}^{35} \gamma_j \mathbf{1}(\text{event_time}_{ft} = j) + X_{ift} + \epsilon_{ift} \quad (6)$$

where post_{ft} is a dummy variable equal to 1 for $\text{event_time}_{ft} \in [0, 35]$ (i.e., after the incoming executive takes over at the destination firm). We summarize the estimates and standard errors of β in the text on Figure 4 and in Table 6. For the full sample of employers, we estimate a β coefficient of 5.7pp, with a t-statistic of 7.7.

4.2.2 Robustness

We verify the robustness of our estimates in different ways (Appendix Table A.2). First, we impose more stringent restrictions on the minimum number of observations required to define both components of our treatment variable, sep_rate_pre_f and $\text{sep_rate_pre}_{ef'}$, from 2 to 10. The point estimate is virtually unchanged (rising from 6.2pp to 6.4pp) but standard errors are twice as large. Nonetheless, the estimates remain statistically significant at the 1% level. Second, we impose a restriction that the incoming executive moves into the executive role at the destination firm within 12 months of leaving the origin firm. Again, the estimated effect is slightly larger (6.6pp) but a bit less precise (the t-statistic falls from 7.7 to 7.2). Third, we show that the estimates are very similar if we add firm fixed effects, which account for potential biases from the unbalanced nature of the panel.[35] Fourth, we fully interact our calendar time dummies and event-time dummies. These fixed effects mean that firms are exactly matched based on the timing of the change in executive, with identification coming exclusively from differences in the magnitude/direction of the shock (rather than also from the timing of the shock). We find that the estimated effect of the shock is slightly

[34] We observe modest increases in firm size (about 3%) and the average earnings of workers (about 0.5%) at the time of the switch.

[35] The panel is unbalanced because (i) firms may have several months between workers reaching the SRA and (ii) the firm may go out of business.

larger (7.6pp, t-statistic = 7.8). Fifth, we show that the estimated effects are similar if an executive at the destination firm retires in the six months prior to the new manager starting (5.7pp, t-statistic = 3.6), or in the month before (5.2pp, t-statistic = 2.8). The associated estimates are similar if we further restrict to 'early retirements' (5.3pp and 5.7pp). Such changes in leadership at the firm may be more likely to be involuntary, and seem unlikely to reflect a strategic decision to change the firm's retirement practices.

4.2.3 Heterogeneous Effects

By Sector. As executives in the private sector typically have greater discretion to enact changes, we examine the results for the private and non-private sectors separately. Table 6 columns 3-6 display the DID results by sector. Figure 4 (b) and (c) compare the event-study plots. Consistent with the L1O analysis, we find that the estimated effects in the private sector are large and the dynamic DiD estimates are compelling. We estimate an average treatment effect of 8.3pp in the post period, which suggests that an increase of one standard deviation in the executive's SRA separation propensity increases the likelihood of a worker retiring at SRA in the destination firm by 8.3pp. The effect is also significant but smaller for the public sector (4.5pp, $p < 0.01$).

By Gender and Age of the Executives. We also assess whether the gender of the executives matters for the estimated effect. Table A.3 and Figure 5 display the results. We find that male managers tend to have a larger impact when their pre-moving SRA separation is one standard deviation larger than the destination firm. When we look at the results by age of the executives, we do not find any significant difference between younger and older executives. The effects are 0.067 and 0.049, respectively (columns 3 and 4 in Table A.3).

By Sign of the Shocks. Furthermore, we assess whether positive and negative shocks have symmetric impacts. If the effects were only driven by shocks in one particular direction, the results would appear less credible. To implement this analysis, we construct separate dummy variables for positive shocks, 1.pos_shock$_f$ (=1 for top quartile values of shock$_f$: $\overline{\text{shock}_f} = 1.29$ standard deviations) and negative shocks, 1.neg_shock$_f$ (=1 for bottom quartile values: $\overline{\text{shock}_f} = -1.15$ standard deviations). Figure 5 (c) visually shows that the impacts are of opposite signs. The estimated effects of positive shocks are positive (8.6pp), while the estimated effects of negative shocks are negative (-3.4pp). Moreover, a positive shock tends to make a larger impact than a negative shock. The p-value testing the differences is 0.052. This result is reasonable, as we expect that pushing older workers out of the firm when they reach SRA in practice is easier to implement than rehiring workers beyond SRA.

4.2.4 Are Stricter Executives More Productive?

In this subsection, we explore how changes in managerial SRA separation propensity impact a firm's financial outcomes. To do so, we explore the linkage to a dataset that contains information from private companies' balance sheets and income statements. This data is available from 2000 to 2023 at a yearly frequency and only for firms in the private sector. Nevertheless, it allows us to study whether a stricter executive towards older workers close to SRA also affects the financial outcomes. Figure A.6 shows the event study plots for sales, wage costs, capital investment, and net profit at both par capita level and the total amount. All monetary values are measured in thousand euros per year and CPI adjusted to 2024 euros. Interestingly, in terms of per capita outcomes, we do not find any significant changes. However, when we look at the total changes per year at the firm level, we find that a positive managerial shock (i.e., appointing an executive with a 10pp higher SRA separation propensity) decreases total sales by 768 thousand euros per year and also somewhat a decline in capital investment. However, these impacts are not statistically significant and should be interpreted with caution.

Another possible consequence of the increase in SRA separation could be a change in worker composition in the destination firm. For example, it might change retention and hiring decisions of workers at other age groups. In Figure **??**, we examine the impact of changes in executives' SRA-separation propensities on the share of workers below 35, between 35 and 49, between 50 and 59, and between 60 and 64. One might expect that with fewer older workers in the firm, firms might hire more younger workers. However, we do not find any significant changes in all of these outcomes.

4.2.5 Relative Magnitude

How important are differences in management in shifting retirement behavior relative to policy reforms? This question is important because, to the extent that managers' attitudes can be changed, policymakers may be able to boost old-age employment without unpopular policies like raising the retirement age.

We consider the employment effects of a hypothetical intervention that reduces executives' SRA separation propensities by one standard deviation (10pp), keeping everything else constant. Our point estimates suggest that such an intervention would reduce older workers' separation rates in the six months around the SRA by 5pp, from 82% to 77%. The total increase in employment in the five years after the SRA is 5%. To achieve the same increase in employment, the government would have to raise the SRA by three months.

We then consider the same hypothetical intervention in the private sector, where (i) the standard

deviation in executives' separation propensities is larger (10 pp), (ii) executives' separation propensities seem to exert a stronger influence on workers' retirement outcomes and (iii) the employment effects of raising the SRA are smaller. In the private sector, a one standard deviation reduction in manager's separation propensity would reduce older workers' SRA separation probability by 8.3pp, from 80% to 71.7%, causing a 10% increase in employment in the five years after the SRA. To achieve the same increase in private-sector employment, the government would have to raise the SRA by 6 months.

5 Conclusion

In this paper, we study the extent to which employers and executives shape workers' retirement behavior at the Statutory Retirement Age in the Netherlands. Using comprehensive administrative data linking older workers to their firms and chief executives, we examine how much of the pronounced spike in retirements at the SRA can be attributed to employer and managerial discretion rather than to worker characteristics or pension incentives.

Our results show that the identity of the employer—and especially the executive—matters greatly for whether a worker retires at SRA. We document substantial heterogeneity across firms in separation rates at the SRA, far exceeding what would arise from worker composition alone. Unobserved firm factors explain a large share of the variation in retirement outcomes, and incorporating executive fixed effects further increases explanatory power. Regression analyses confirm that workers employed at firms with higher historical separation propensities are significantly more likely to retire at the SRA, even after conditioning on rich controls. Managerial changes provide especially compelling evidence: when a manager with a stricter retirement style arrives, workers' separation rates adjust immediately and persistently, indicating that executives causally influence retirement practices.

These findings have important implications for retirement policy. They show that workplace norms surrounding mandatory retirement are neither fixed nor solely determined by national institutions, but instead are shaped by discretionary choices made by firms and their leaders. As a result, policymakers seeking to extend working lives need not rely exclusively on politically costly increases in pension ages. Interventions that encourage or incentivize employers and managers to retain older workers could generate employment gains comparable to sizable shifts in the SRA. More broadly, our results demonstrate that understanding employer behavior is essential for predicting the effects of pension reforms and for designing effective policies to promote old-age employment.

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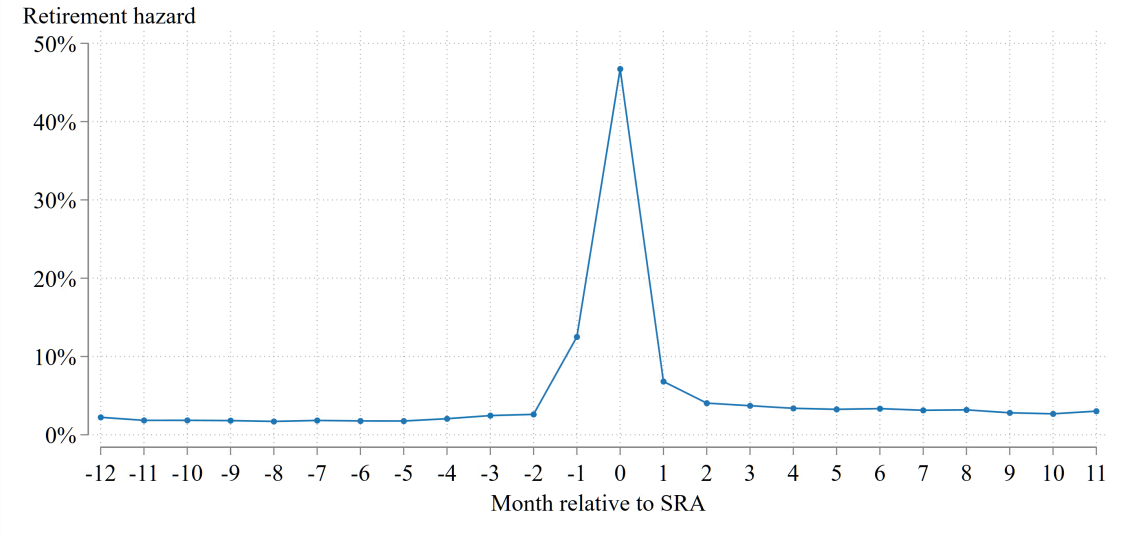
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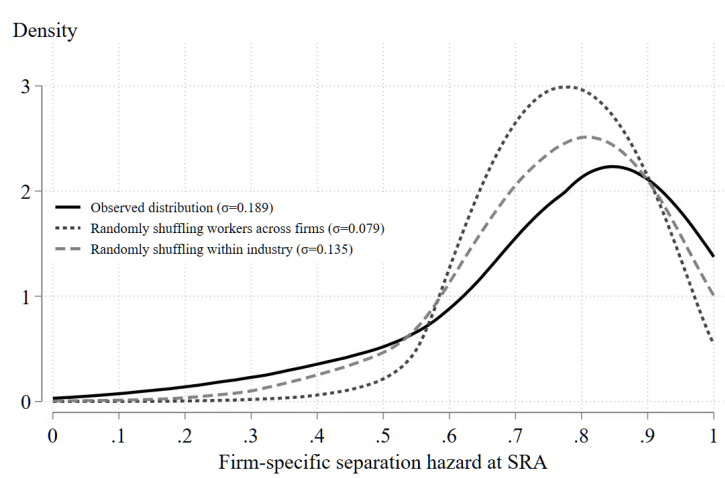
Figure 1: Transitions into retirement around the statutory retirement age



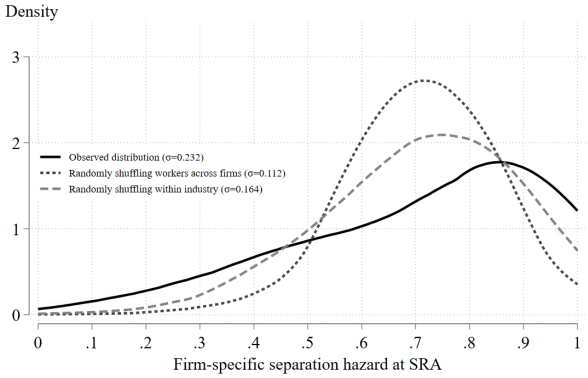
Notes: This figure plots the average retirement hazard of Dutch workers by the number of months until the SRA.
Source: CBS

Figure 2: Distribution of SRA retirement hazards across employers

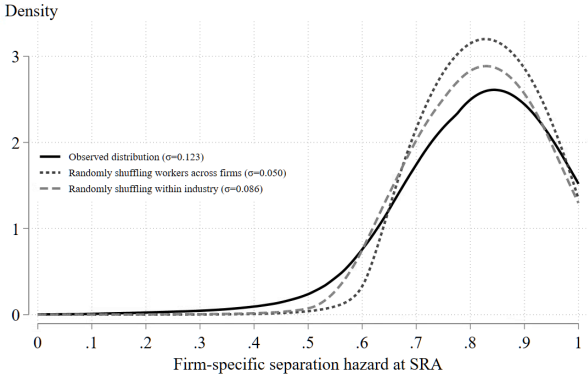
(a) All employers



(b) Private sector



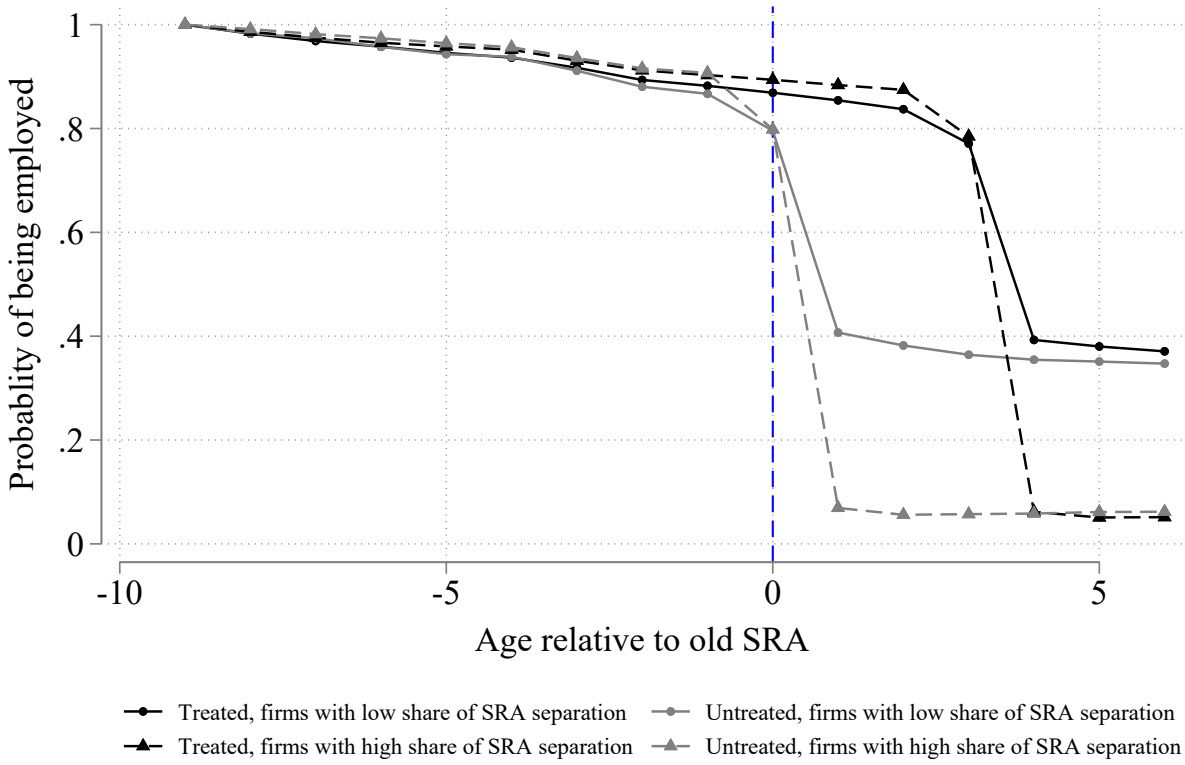
(c) Public sector



Notes: These figures compare the distribution of firm-level incidences of bunching at statutory retirement age (solid black line) with the distribution when randomly shuffle workers across firms (dotted line) and randomly shuffle within sectors (dashed line) for all firms (Figure a), private sector firms (Figure b) and public sector firms (Figure c), respectively.

Source: CBS.

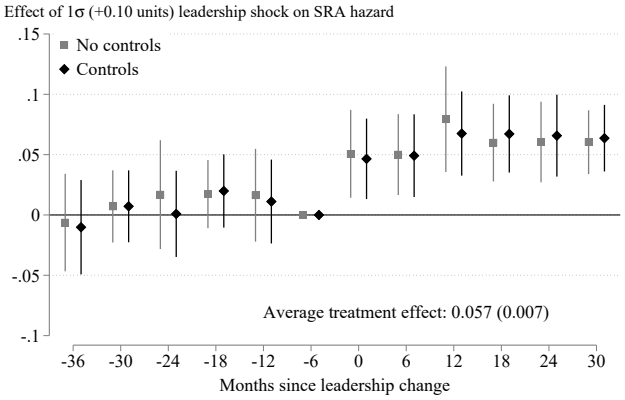
Figure 3: Heterogeneity in the employment effects of raising the SRA from 65 years and 3 months to 65 years and 6 months on workers by firms' SRA-separation propensities



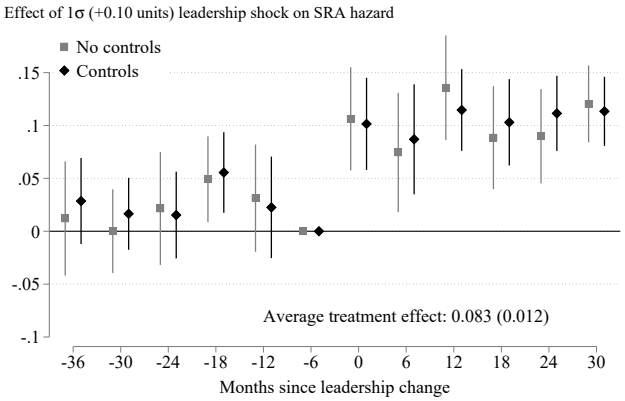
Notes: This figure shows that the heterogeneous employment effects of an increase in the SRA from 65 years and 3 months to 65 years and 6 months at the age of 65 years and 4 months by firms' SRA-separation propensities. The low share of SRA separation firms are indicated by gray lines (bottom 20% (39.2pp)) and the high share of SRA separation firms are indicated by black lines (top 20% (80.8pp)).
Source: CBS.

Figure 4: Impact of Changes in Executives' SRA-separation Propensities: SRA separation rates

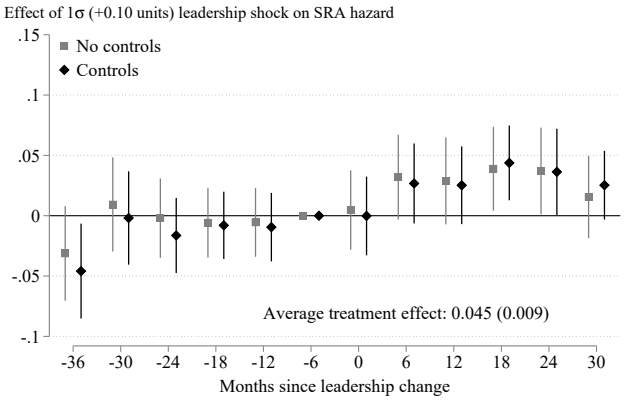
(a) All employers



(b) Private sector



(c) Public sector

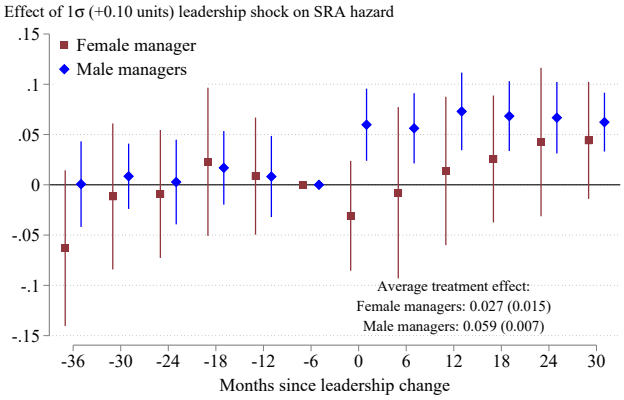


Notes: These figures show the event-study estimates of a change of one standard deviation of leadership shock. The shock variable measures the differences in SRA separation rates between the destination and the original firm of the moving executive. Figure (a) shows the results for all firms, while Figures (b) and (c) are for private and non-private firms. The event-study plots display the estimates with a full set of controls in black diamonds and the estimates without controls in gray squares.

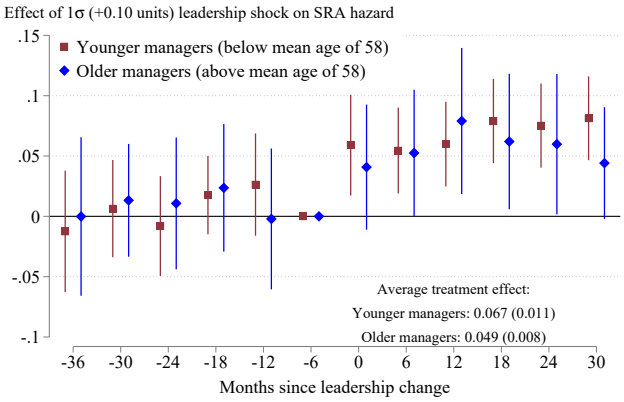
Source: CBS.

Figure 5: Heterogeneous Effect: Impact of Changes in Executives' SRA-separation Propensities

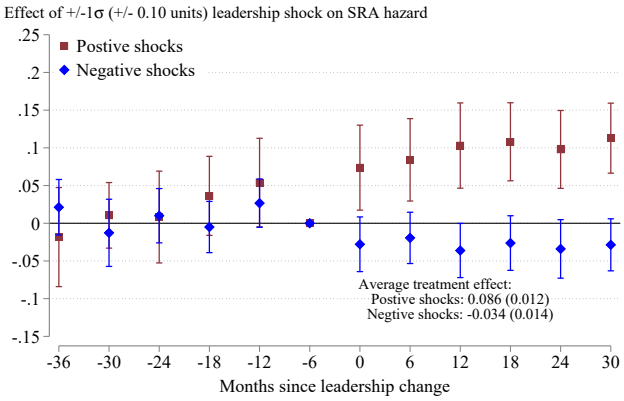
(a) By Gender of the Executives



(b) By Age of the Executives



(c) By Signs of the Change



Notes: These figures show the heterogeneous impact of changes in the executives' SRA-separation propensities. The estimates indicate a change of one standard deviation of leadership shock. The shock variable measures the differences in SRA separation rates between the destination and the original firm of the moving executive. Figure (a) compares the results by gender of the new executives. Figure (b) compares the results by age of the new executives. Figure (c) compares the results by positive or negative shocks.

Table 1: Summary Statistics

	Mean	Std Dev
Older worker characteristics		
Share female	40.1%	
Permanent employee	90.1%	
1st gen immigrant	9.9%	
2nd gen immigrant	5.7%	
Private sector	45.4%	
Director/CEO	0.4%	
SRA in years	65.9	(0.6)
Monthly earnings pre SRA	€3,421	(€2,255)
Percentile rank in firm	0.52	(0.30)
Monthly hours pre SRA	125.4	(46.5)
Percentile rank in firm	0.48	(0.30)
Hourly wage pre SRA	€26.5	(€12.9)
Percentile rank in firm	0.57	(0.29)
Number of older workers	384,775	
Firm characteristics		
Number of employees	489	(2,041)
Number of establishments	5.4	(16.9)
Share with multiple establishments	59.3%	
Size of establishment	235	(617)
Average age of workers (years)	45.0	(5.1)
Average monthly earnings	€3,281	(€1,235)
Average monthly hours	133	(28)
Average hourly wage	€23.8	(€6.4)
Share of female workers	41.7%	(0.286)
Share of 1st gen immigrants	10.6%	(0.126)
Share of 2nd gen immigrants	7.2%	(0.047)
Wage-age gradient (log pts)	0.012	(0.009)
Number of firms	13,936	
Executive characteristics (at executive level)		
Share female	0.184	(0.39)
Age in years	52.6	(8.7)
Monthly earnings	€17,167	(€17,550)
Number of unique executives	48,653	

Notes: This table presents descriptives of older workers, firms, executives (top-earners) in the core sample. We present the means for all variables and standard deviations for continuous variables.
Source: CBS.

Table 2: Variance Decomposition (Based on adjusted R^2)

	(1)	(2)	(3)
		By sector	
	All employers	Private	Public
(1) Own worker characteristics	0.041	0.089	0.043
(2) Firm characteristics	0.053	0.070	0.043
(3) Industry & sector fixed effects	0.088	0.094	0.050
(4) Executive characteristics	0.029	0.036	0.009
(5) Time fixed effects	0.007	0.011	0.006
(6): All controls, (1) to (5)	0.131	0.158	0.085
(7): Firm fixed effects	0.173	0.214	0.093
(7a): (7) + (6)	0.203	0.247	0.127
(8): Establishment fixed effects*	0.185	0.229	0.103
(8a): (8) + (6)*	0.215	0.260	0.136
(9): Executive fixed effects$^{\#}$	0.190	0.237	0.110
(9a): (9) + (6)$^{\#}$	0.219	0.267	0.142
Observations	384,775	174,530	210,245

Source: CBS.
Note: This table shows the share of variance in SRA separation rates explained by different controls, based on adjusted R^2 values.
* Smaller number of observations (about 1% smaller), as observations linked to a single branch are dropped.
$^{\#}$ Smaller number of observations (about 4% smaller), as observations linked to a single executive are dropped.

Table 3: Quantifying Firm Effects with Different Controls

	(1)	(2)	(3)	(4)	(5)
L1O firm mean	0.925***	0.871***	0.847***	0.835***	0.836***
	(0.008)	(0.011)	(0.009)	(0.009)	(0.009)
Controls					
Industry & sector dummies	✓	✓	✓	✓	✓
Worker characteristics		✓	✓	✓	✓
Firm characteristics			✓	✓	✓
Executive characteristics				✓	✓
Time fixed effects					✓
R-squared	0.148	0.169	0.172	0.172	0.174
Mean SRA hazard			0.767		
Observations			384,775		

*** denotes p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered by firm.
Source: CBS.

Table 4: Leave-one-out Analysis: Firm and Executive Effects

	Executive sample		Multi-firm executives		
	(1)	(2)	(3)	(4)	(5)
L1O mean in firm	0.845***	0.471***	0.796***	0.614***	0.385***
	(0.010)	(0.025)	(0.025)	(0.035)	(0.036)
L1O mean of executive		0.453***		0.283***	0.086***
		(0.028)		(0.040)	(0.031)
L1O mean of executive-firm pair					0.454***
					(0.044)
All controls	✓	✓	✓	✓	✓
R-squared	0.166	0.171	0.121	0.123	0.125
Mean SRA hazard	0.784			0.809	
Observations	323,408			62,125	

*** denotes p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered by firm.
Source: CBS.

Table 5: Summary Statistics for the Executive Switching Sample

	Mean	Std. Dev.
Older worker characteristics		
Share female	0.461	(0.498)
Permanent employee	0.927	(0.260)
1st generation immigrant	0.107	(0.309)
2nd generation immigrant	0.060	(0.238)
Private sector	0.303	(0.460)
SRA (years)	65.997	(0.560)
Monthly earnings pre-SRA	3,480.337	(2,079.570)
Percentile rank (earnings)	0.520	(0.294)
Monthly hours pre-SRA	125.957	(42.507)
Percentile rank (hours)	0.483	(0.295)
Hourly wage pre-SRA	27.036	(11.813)
Percentile rank (wage)	0.557	(0.286)
Number of older workers	21,037	
Firm characteristics (at firm level)		
Number of employees	946.70	(2,054.64)
Number of branches	8.39	(24.67)
Multiple establishments (ever)	0.68	(0.47)
Establishment size	457.44	(799.80)
Average age of workers	44.85	(3.81)
Average monthly earnings	3,463.92	(1,109.06)
Average monthly hours	130.45	(24.07)
Average hourly wage	25.75	(5.60)
Share female workers	0.54	(0.28)
Share 1st gen immigrants	0.09	(0.10)
Share 2nd gen immigrants	0.08	(0.04)
Wage–age gradient (log pts)	0.013	(0.007)
Number of firms	659	
Executive characteristics (at executive level)		
Age	54.98	(7.56)
Share female	0.24	(0.43)
Monthly earnings	19,231.86	(17,482.19)
Number of unique executives	2,202	

Notes: This table presents descriptives of older workers, firms, executives (top-earners) in the executive switching sample. We present the means for all variables and standard deviations for continuous variables.
Source: CBS.

Table 6: Estimated Effects of Executive Changes on SRA Separation Rates

	All employers		Private sector		Public sector	
	(1)	(2)	(3)	(4)	(5)	(6)
1σ shock $\times$ post	0.0531***	0.0575***	0.0811***	0.0832***	0.0375***	0.0447***
	(0.0082)	(0.0073)	(0.0142)	(0.0122)	(0.0096)	(0.0096)
R-squared	0.045	0.136	0.111	0.236	0.036	0.116
Controls		✓		✓		✓
p-value: F-test of pre-trends	0.476	0.267	0.171	0.098	0.584	0.316
Mean SRA hazard	0.824		0.800		0.834	
Observations	21,034		6,362		14,660	

This table shows the impact of a one-standard-deviation increase in managerial shock (i.e., appointing an executive with a 10pp higher SRA separation propensity than before) on the separation rate of workers reaching the SRA in the destination firm. Columns 1 -2 show the results for all firms. Columns 3-6 show the results for private and non-private sectors, respectively. The odd columns are estimates without controls, and the even columns show results with controls. *** denotes p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered by firm.
Source: CBS.

Firms, Managers and Retirement

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Han Ye — University of Mannheim

Supplemental Appendix

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A Quantifying the SRA Bunching

We quantify the bunching at the SRA in the retirement hazard using two regressions. First, we use an event-study model, where we set the reference period to be 2 months before the SRA (event-time $= -2$). This choice of reference period reflects the fact that individuals may choose to retire in the month before they reach the SRA, particularly if they will reach the SRA early in the following month (e.g., a person may retire in December if they will reach the SRA in early January). Our event-study model includes time fixed effects (month-year of observation) and worker age fixed effects.[1] The former controls for business cycle fluctuations and seasonality in retirement transitions, while the latter controls for preferences to retire at specific ages (e.g., at particular birthdays like age 65). We estimate the following regressions:

$$\text{retires}_{it} = \alpha + \sum_{\substack{k=-12 \\ k\neq -2}}^{11} \beta_k \mathbf{1}\left(\text{age}_{it} - \text{SRA}_i = k\right) + \text{age}_{it} + \gamma_t + \epsilon_{it} \tag{A.1}$$

where i denotes a worker and t time (the month-year of observation); retires_{it} is a dummy variable equal to 1 if the worker retires in month t (0 if they remain employed in month $t+1$); α is the constant term; γ_t is a set of time fixed effects; age_{it} is a set of age-in-months fixed effects; and ϵ_{it} is an idiosyncratic error term.[2] The sample is an unbalanced panel of workers because workers drop out of the sample as they retire. The β_k terms are the coefficients of interest, denoting the difference in the retirement hazard of workers in event-month k and the reference period. We expect the β_k coefficients to be zero for $k < -2$ and positive for $k = 0$ (and possibly $k = -1$ and $k = 1$). If this is the case, we would conclude that we have robust evidence of bunching at the SRA. The β_k coefficients may also be positive for $k > 1$, if workers are more likely to retire at all ages once they are eligible for a pension (i.e., not just at the first possible age).

We present the estimates of equation (A.1) in Figure A.2. The patterns are qualitatively similar to the scatter plot, showing minimal evidence of any trend in the retirement hazard in the lead up to the SRA, with the coefficients hovering around zero. The retirement hazard then increases in the month before the SRA to 8.3 percentage points (pp) and peaks in the SRA month at 43.4pp, before declining back towards baseline levels. The retirement hazard remains partially elevated thereafter, with some of the subsequent coefficients attaining statistical significance (in event-months 5 and 10). Thus, we observe strong evidence of bunching at the SRA and some evidence that the retirement hazard remains elevated afterwards.

We quantify the amount of bunching at the SRA by estimating a slightly modified version of equation (A.1), where we aggregate the β_k coefficients for $k > -2$ into two groups (around-SRA: event-months -1 to 1 and

[1] We include fixed effects for the worker's age in months.

[2] We cluster standard errors at the level of randomization: birth year and month. This results in 215 clusters.

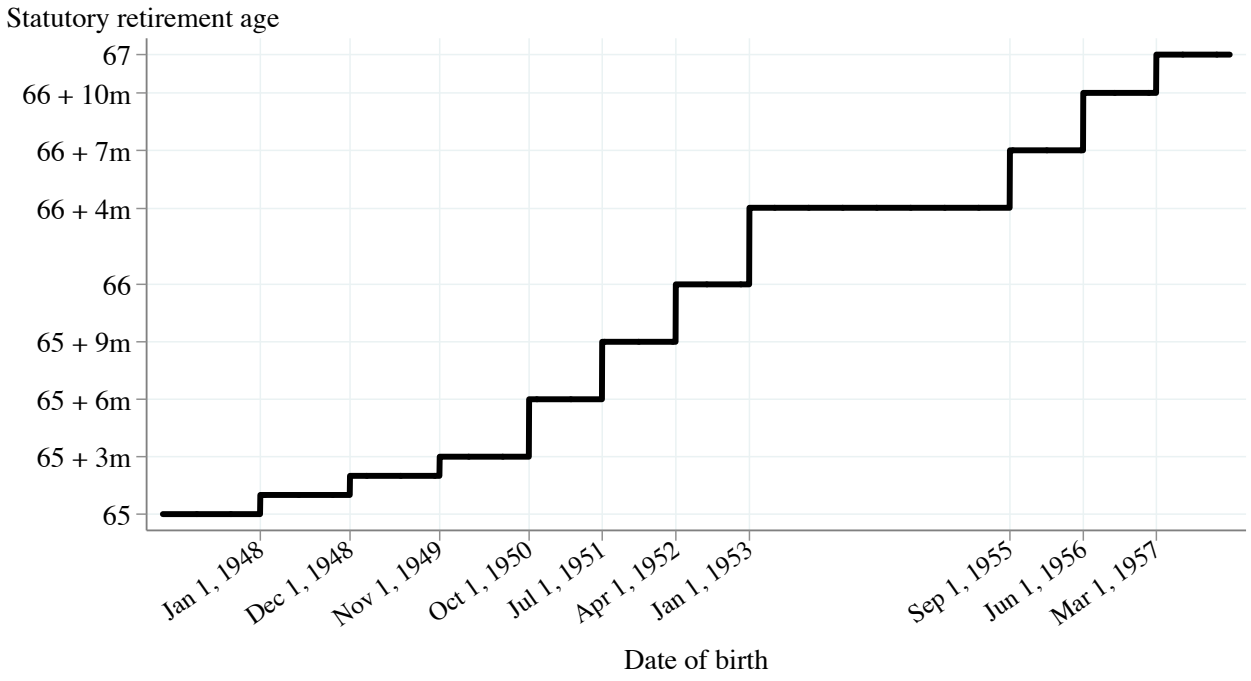
post-SRA: event-months 2 to 11). Specifically, we estimate the following regressions:

$$\text{retires}_{it} = \alpha + \delta_1 \frac{\mathbf{1}\left((\text{age}_{it} - \text{SRA}_i) \in \{-1, 0, 1\}\right)}{3} + \delta_2 \frac{\mathbf{1}\left((\text{age}_{it} - \text{SRA}_i) \geq 2\right)}{10} + \text{age}_{it} + \gamma_t + \epsilon_{it} \qquad \text{(A.2)}$$

where δ_1 quantifies the cumulative amount of bunching in the retirement hazard immediately around the SRA, relative to the year before the SRA, and δ_2 captures the cumulative increase in the retirement hazard in the subsequent 10 months.

In the three months around the SRA, we estimate a 64.9-percentage-point increase in the retirement hazard. This increase in the cumulative hazard over these months amounts to a 819% increase. In the subsequent 10 months, we estimate a further 7.5-percentage-point increase in the retirement hazard ($p < 0.01$). Overall, our estimates suggest that around 71% of Dutch workers who are still working near the SRA retire within one year of reaching the SRA.[3] The magnitude of the estimate is broadly consistent with earlier studies that focused on a narrower set of birth cohorts (Rabaté et al., 2024).[4]

Figure A.1: Statutory retirement ages in the Netherlands

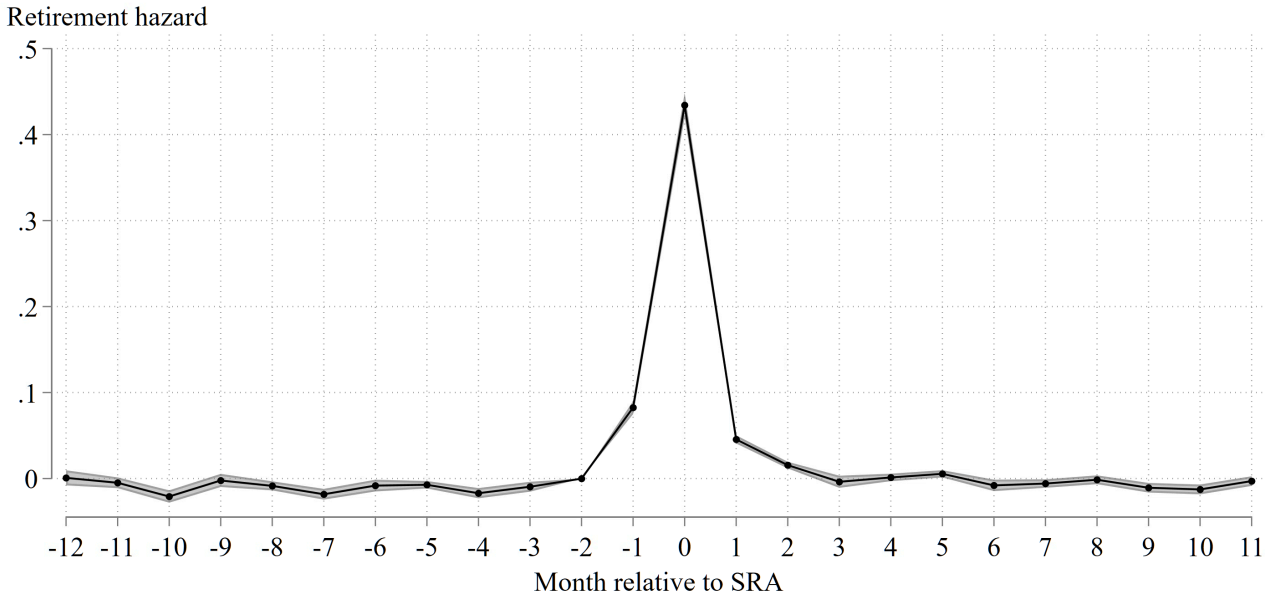


Notes: This figure shows the increase of the SRA implement by pension reforms in 2012. The SRA increases gradually base on birth month from 65 to 67.

[3] $71\% = 0.69 + (1 - 0.69) \times 0.075$.

[4] Rabaté et al. (2024) examine the effects of raising the SRA in the Netherlands from 65 to 66 years and 4 months. They estimate large employment effects, with a retirement hazard at the SRA of 72%.

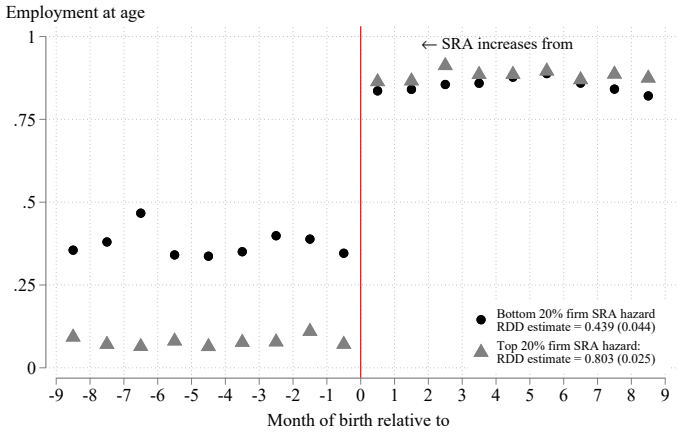
Figure A.2: Event-study Estimates: Separation around the Statutory Retirement Age



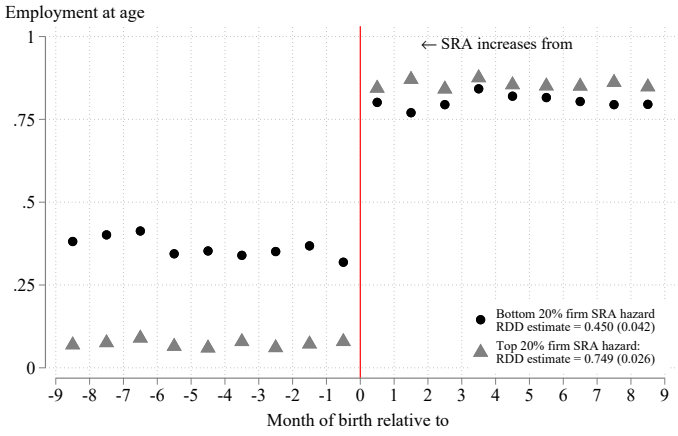
Notes: This figure plots the regression-adjusted retirement hazard around the SRA. The event-study estimates are relative to the reference period (two months prior to the SRA) and are based on equation (A.1). We present 95% confidence intervals with standard errors clustered by month of birth.
Source: CBS

Figure A.3: Heterogeneity in the employment effects of raising the SRA on workers by their firms' SRA-separation propensities

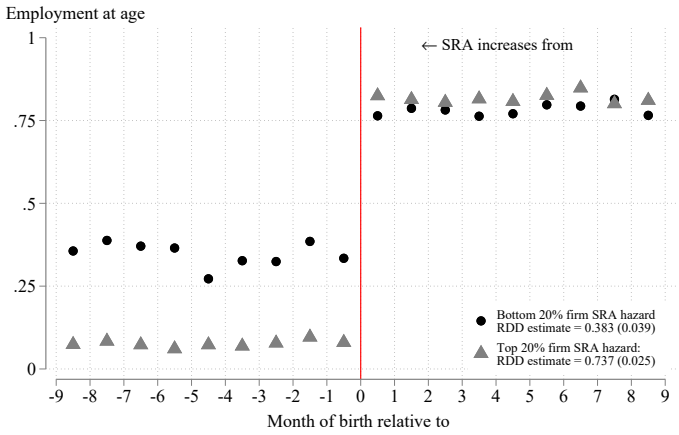
(a) SRA increase from 65.25 to 65.5



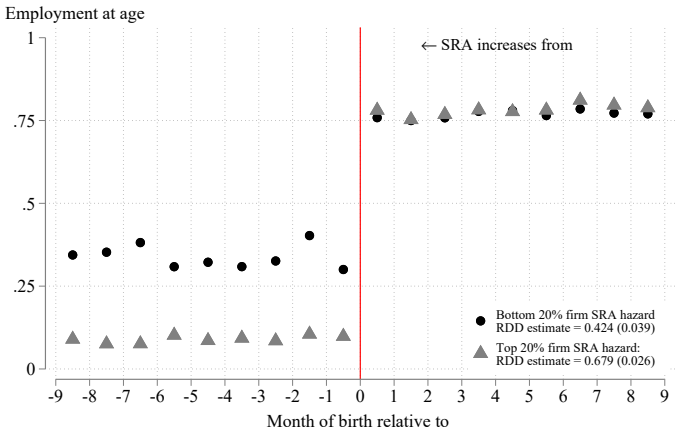
(b) SRA increase from 65.5 to 65.75



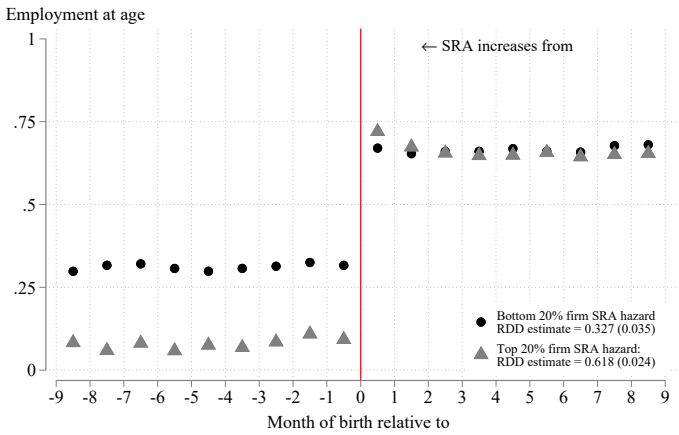
(c) SRA increase from 65.75 to 66



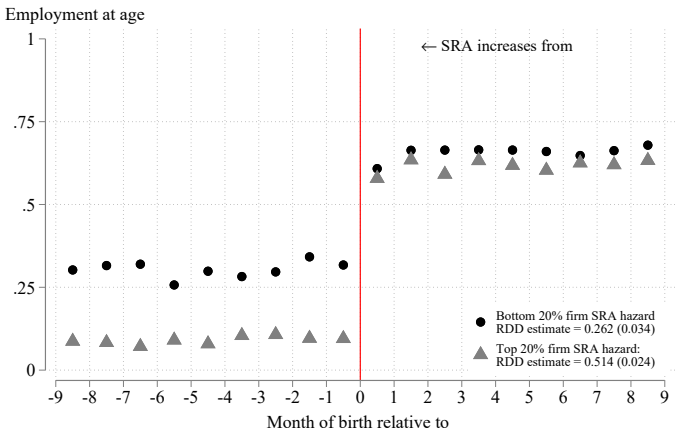
(d) SRA increase from 66 to 66.33



(e) SRA increase from 66.33 to 66.58



(f) SRA increase from 66.58 to 66.83

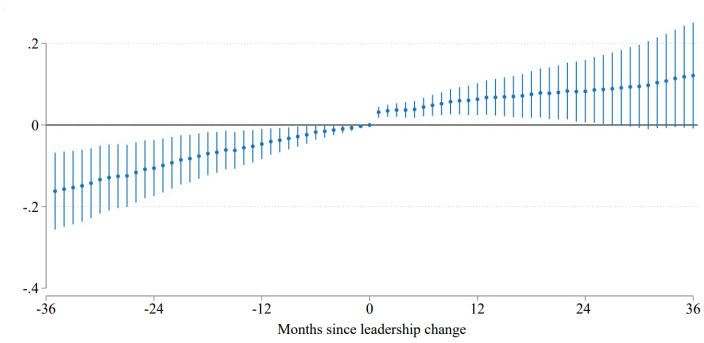


Notes: These figures show the heterogeneous employment effects of raising the SRA on workers by their firms' SRA-separation propensities.

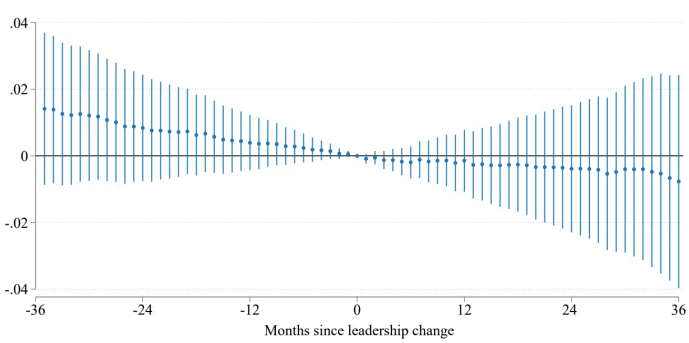
Source: CBS.

Figure A.4: Changes in firm characteristics by time since change in executive

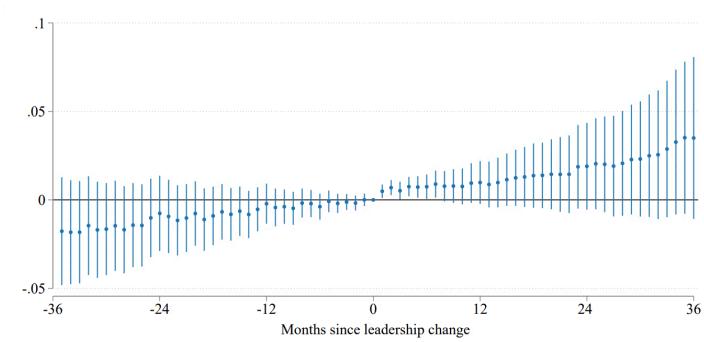
(a) Log (firm size)



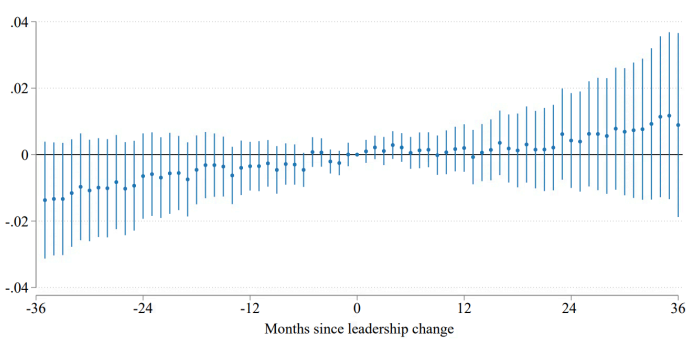
(b) Share female



(c) Log (average earnings)



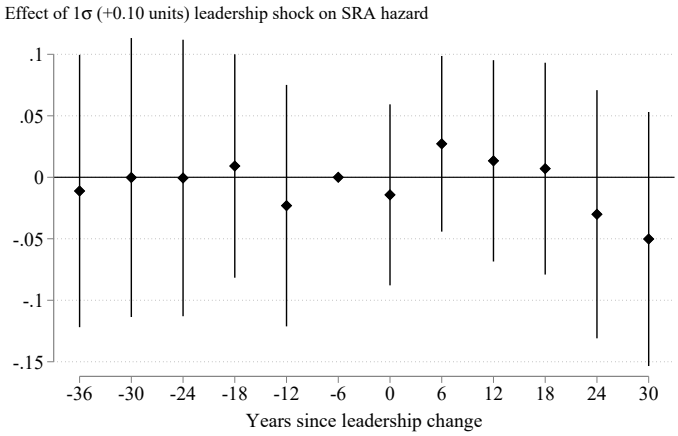
(d) Log (average hours)



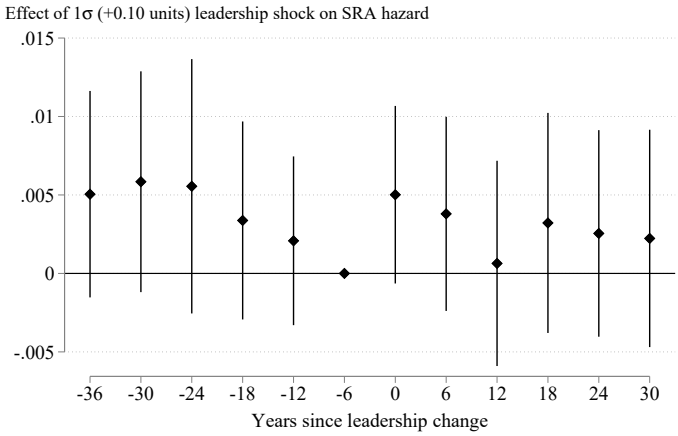
Notes: These figures show the impact of a change in the executive on a list of characteristics of the destination firm.

Figure A.5: Impact of Changes in Executives' SRA-separation Propensities: Firm Characteristics

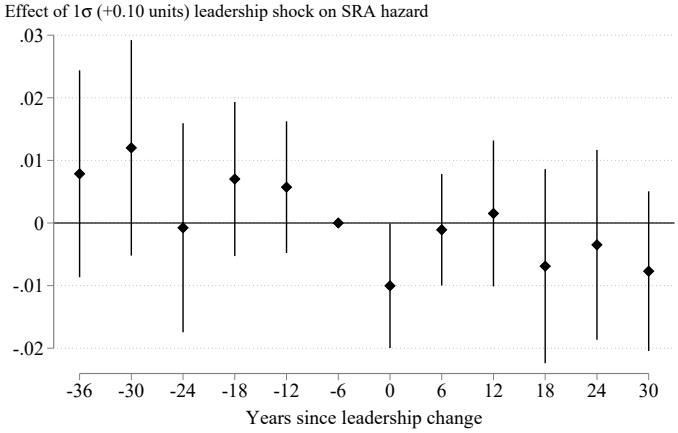
(a) Log (firm size)



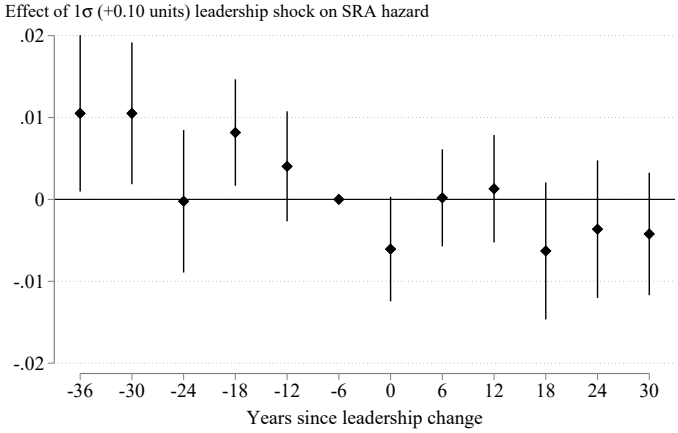
(b) Share female



(c) Log (average earnings)



(d) Log (average hours)



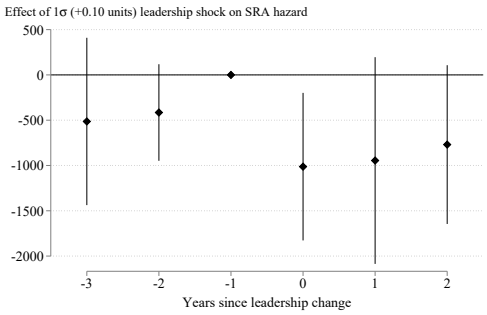
Notes: These figures show the impact of changes in the executives' SRA-separation propensities on a list of characteristics of the destination firm. The estimates indicate a change of one standard deviation of leadership shock. The shock variable measures the differences in SRA separation rates between the destination and the original firm of the moving executive.

Figure A.6: Impact of Changes in Executives' SRA-separation Propensities: Firm Financial Outcomes

(a) Sales per capita



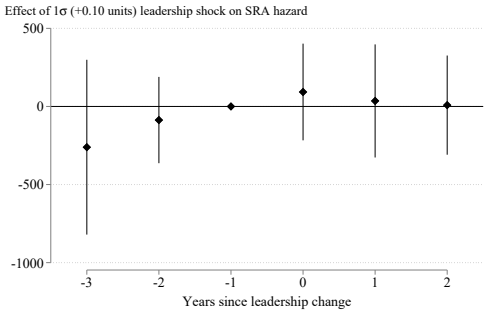
(b) Total sales



(c) Wage costs per capita



(d) Total wage costs



(e) Capital investment per capita



(f) Total capital investment



(g) Net profit per capita



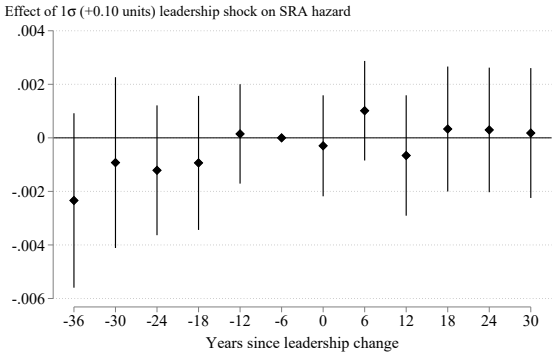
(h) Total net profit



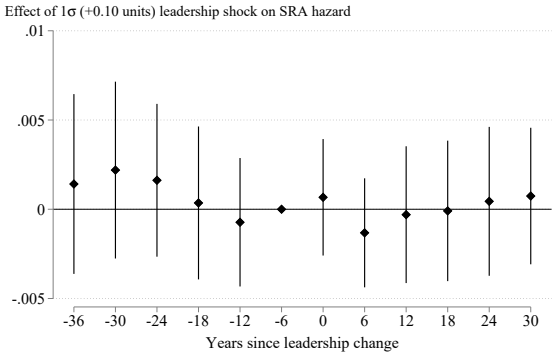
Notes: These figures show the impact of changes in the executives' SRA-separation propensities on a list of financial outcomes of the destination firm. The estimates indicate a change of one standard deviation of leadership shock. The shock variable measures the differences in SRA separation rates between the destination and the original firm of the moving executive.

Figure A.7: Impact of Changes in Executives' SRA-separation Propensities: Worker Composition

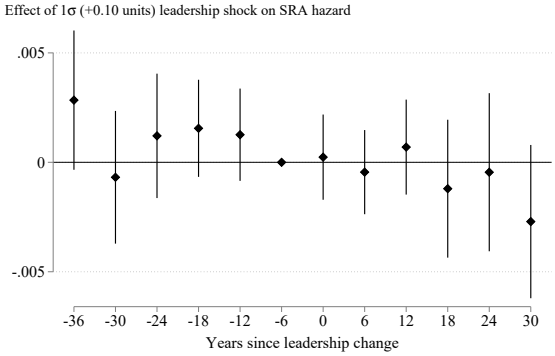
(a) Share of workers below age 35



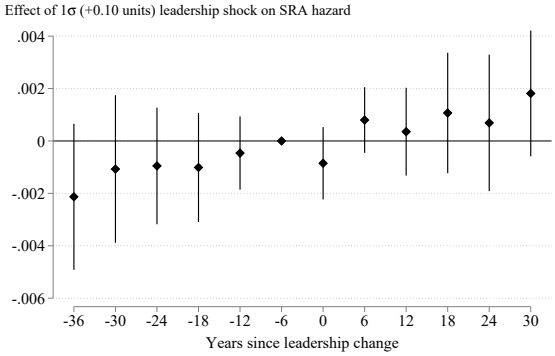
(b) Share of workers between 35 and 49



(c) Share of workers between 50 and 59



(d) Share of workers between 60 and 64



Notes: These figures show the impact of changes in the executives' SRA-separation propensities on worker composition of the destination firm. The estimates indicate a change of one standard deviation of leadership shock. The shock variable measures the differences in SRA separation rates between the destination and the original firm of the moving executive.

Table A.1: Impact of sample restrictions

	Workers	Firms
Employees aged one month below SRA (born Jan 1942–April 1957)	671,495	190,219
Employed by a single firm	650,562	178,944
Continuous tenure of at least 12 months	564,872	135,744
At least 5 such workers ever observed in the firm	394,579	14,177
Non-missing branch info	384,775	13,936

Table A.2: Robustness Checks: Executives Switching Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
						Outgoing executive retires			
		Min. obs	Moves	Firm	Calendar x	at any age		before SRA	
	Baseline	for shock$_f$	within 12	fixed	event-time	no. months before new executive starts			
	estimate	components = 10	months	effects	dummies	1–6	1	1–6	1
1σ shock × post	0.062***	0.064***	0.067***	0.056***	0.074***	0.057***	0.052***	0.053***	0.057***
	(0.008)	(0.016)	(0.009)	(0.007)	(0.010)	(0.016)	(0.019)	(0.019)	(0.023)
R-squared	0.136	0.162	0.149	0.198	0.330	0.214	0.263	0.224	0.264
All controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	21,363	8,505	13,101	21,356	19,334	4,703	2,688	4,063	2,279

Notes: This table shows the robustness of our baseline estimates in the switching analysis by varying sample restriction, regression specification, and type of transitions. All estimates are with controls. *** denotes p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered by firm.
Source: CBS.

Table A.3: Heterogeneous Effects of Executive Changes on SRA Separation Rates

	Female mgr.	Male mgr.	Younger mgr.	Older mgr.	Positive shock	Negative shock
	(1)	(2)	(3)	(4)	(5)	(6)
1σ shock $\times$ post	0.0272**	0.0595***	0.0669***	0.0486***	0.0863***	-0.0340***
(Std. error)	(0.0152)	(0.0073)	(0.0109)	(0.0082)	(0.0120)	(0.0138)
R-squared	0.169	0.153	0.160	0.159	0.1454	0.1250
Controls	✓	✓	✓	✓	✓	✓
Observations	5,603	15,258	10,522	10,498	15,814	15,825

Notes: This table shows the heterogeneous impact of a one-standard-deviation increase in managerial shock (i.e., appointing an executive with a 10pp higher SRA separation propensity than before) on the separation rate of workers reaching the SRA in the destination firm. Columns 1 -2 show the results for female and male managers. Columns 3-4 show the results for younger (below the mean age of 58) and older executives (above 58). Columns 5-6 compare the impact of a one-standard-deviation increase and a one-standard-deviation decrease in managerial shock. All estimates are with controls. *** denotes p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered by firm.
Source: CBS.