Longer careers: A barrier to hiring and coworker advancement?*

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Abstract

Government policies are encouraging older workers to delay retirement, which may curb younger workers' career advancement. We study a Dutch reform that raised the retirement age by 13 months and nearly tripled employment at age 66. Using monthly linked employer-employee data, we show that affected firms delay and decrease replacement hiring, and coworkers' earnings fall via reductions in hours worked, wages, and promotions. Combined, the hiring and coworker spillovers offset most of the additional hours worked by older workers, disproportionately affect career advancement for younger workers and women, and considerably increase the policy's ratio of welfare costs to fiscal savings.

Keywords: retirement reform; labor demand; internal labor markets; firms; coworker spillovers

JEL Classification: H55, J23, J26, J63

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1 Introduction

How substitutable are workers of different ages? This question is crucial for understanding the economic implications of demographic trends and policies that disproportionately affect the wages or supply of workers in particular age groups. Population aging is a salient case study: it has led to a surge in the number of older workers across the OECD and forced a range of policies aimed at delaying their retirements.[1] A mature literature shows that many policies are effective in encouraging older workers to delay retirement,[2] but we lack a detailed understanding of how firms respond to these policies, and the extent to which retirement delays benefit or harm younger workers' careers.

Retirement delays by older workers may have non-trivial spillover effects within firms. For example, worker substitutability would imply that employers will delay when they hire replacement workers — potentially harming younger workers' careers by limiting their initial job prospects (Oreopoulos et al., 2012) and slowing their transitions to higher-paying firms (Topel and Ward, 1992; Abowd et al., 1999). Incumbent coworkers may also face slower career progression due to the retention of older workers in senior roles (Buhai et al., 2014; Bianchi et al., 2022). On the other hand, evidence of worker complementarities (Jaravel et al., 2018; Jäger and Heining, 2022) suggests that older workers may sometimes benefit the careers of coworkers.

Overall, there is limited empirical evidence on the sign, timing, nature and magnitude of these career spillovers. Existing evidence is based heavily on a unique pension reform in Italy that significantly delayed the retirement timing of workers that were weeks from retirement eligibility: Bianchi et al. (2022) find that these sudden retirement delays among older workers crowded out opportunities for coworkers — via a decrease in promotions and an increase in layoffs — but had no effect on hiring.[3] However, it is unclear how these findings may generalize

[1]Workers aged 55 and older now account for 22% of employment in OECD countries, up from 12% in 2000.

[2]This literature includes many studies estimating the employment impacts of early and full retirement age thresholds (Börsch-Supan and Schnabel, 1998; Mastrobuoni, 2009; Staubli and Zweimüller, 2013; Behaghel and Blau, 2012; Atalay and Barrett, 2015; Hernæs et al., 2016; Manoli and Weber, 2018; Atav et al., forthcoming; Lalive et al., 2021; Seibold, 2021; Geyer and Welteke, 2021; Morris, 2022*a*; Nakazawa, 2022; Morris, 2022*b*). Several studies have also estimated the effects of changes in retirement benefits (Krueger and Pischke, 1992; Lindeboom and Montizaan, 2020; Ye, 2022), earnings tests (Friedberg, 2000; Gelber et al., 2021; Gelber et al., 2022), or exploited age-specific eligibility criteria/benefit levels within Disability Insurance or Unemployment Insurance schemes (Staubli, 2011; Inderbitzin et al., 2016). A number of other studies examine the impact of targeted labor supply or demand incentives aimed at older workers, such as hiring subsidies (Boockmann et al., 2012), tax credits (Laun, 2017), wage subsidies (Albanese and Cockx, 2019) and payroll-tax subsidies (Bıró et al., 2022).

[3]Bianchi et al. (2022) is not the only paper to study the effects of pension reforms on firms. However, other

to representative pension policies (pre-announced and incremental reforms) and countries that were not experiencing deep recessions. Further limitations of the existing literature include a reliance on annual data, which makes it difficult to understand the dynamics of firms' responses; a lack of data on hours worked, making it impossible to study intensive-margin adjustments, measure hourly wages and quantify worker substitutability in terms of overall workloads; and little analysis of the distributional, welfare and fiscal implications of firms' responses.

This paper aims to address these gaps in the literature. Using detailed administrative data at a monthly frequency, we study how Dutch firms respond to a representative national policy that exogenously delayed the retirement timing of older workers by up to 13 months. In 2011 and 2012, the Dutch government announced gradual increases in a salient retirement threshold, the Statutory Retirement Age (SRA). At the SRA, the retirement hazard is among the largest in the world, with an immediate decline in employment rates of around 70% (Atav et al., forthcoming).[4] Our identification strategy exploits four step-wise, cohort-based, increases in the SRA of 3 or 4 months between 2015 and 2019 (from 65 years and 3 months to 66 years and 4 months), a period in which unemployment rates were low and decreasing. We begin our empirical analysis by isolating the directly affected older workers ("focal workers") and showing that the reform caused proportional retirement delays for most of them — their employment rates nearly tripled at targeted ages (from 30–35% to 80–90%), which is consistent with estimates for the broader population (Atav et al., forthcoming).[5]

To assess the spillover effects of the observed retirement delays, we construct a dataset that follows the monthly outcomes of focal workers' firms and coworkers between 2013 and 2019. Our analysis proceeds in two steps. In the first step, we use an event-study model to estimate the effects of a single focal worker approaching and reaching the SRA on the outcomes of their firms and coworkers. This simple descriptive model provides novel graphical evidence about how and when firms' respond to a worker's retirement, which is only possible due to the large

studies focus primarily on the net effects on firm outcomes, such as total employment (Martins et al., 2009; Hut, 2019; Boeri et al., 2021; Carta et al., 2021; Eckrote-Nordland, 2021; Hernæs et al., 2023) and productivity (Martins et al., 2009; Hut, 2019; Carta et al., 2021; Hernæs et al., 2023), rather than the impacts on workers' careers and the margins on which firms adjust their workforce. Most of these studies find that retirement delays crowd out firms' employment of other workers (except Carta et al. (2021), which finds a crowding-in effect).

[4] Employees must be offered a new contract to continue working past the SRA, which explains the high retirement hazard at the threshold. Similar rules that allow employers to terminate employment contracts at a given age exist in many countries, including Germany, France, Ireland, Finland, Sweden, Norway, Portugal and Iceland.

[5] Our sample consists of older workers in the private sector with a strong attachment to a small-to-medium-sized firm (5–200 workers), which is similar to Bianchi et al.'s (2022) sample.

retirement hazard at the SRA and the availability of monthly data. In particular, our estimates reveal that: (i) at the SRA, the hours worked by focal workers sharply decline (mostly at the extensive margin); (ii) firms' hiring rates start to increase three months earlier and remain elevated for five months, indicating that firms often hire replacement workers several months in advance, (iii) there is no change in coworkers' separation rates; (iv) coworkers experience higher earnings growth after the SRA, mostly due to an increase in the incidence of promotions; and (v) coworkers' earnings growth reflects changes in both hours worked and hourly wages. Overall, these results show that the retirements of older workers at the SRA increase the career opportunities available to other workers, although at a rate less than one-for-one.

In the second step, we use difference-in-differences (DiD) models to examine the consequences of raising the SRA on the same outcomes. Rather than having one treatment and one control group, our identifying variation comes from a sequence of SRA increases that progressively delayed the retirements of multiple cohorts of focal workers. We use this variation to implement a stacked cohort-pairs DiD model (Cengiz et al., 2019), with the earlier cohort in each pairwise comparison providing the control group for the later cohort (that is subject to a higher SRA). With this model, we estimate event-time models of firms' responses to a single retirement delay. This results in a clear presentation of the reforms' dynamic impacts and allows us to distinguish delays from net changes in the outcomes of interest. We estimate separate regressions for short and long retirement delays, which reveals a consistent dynamic pattern to the way firms respond and that the net effects increase with the length of the retirement delay.

We find that the SRA increases lead to hiring delays and an apparent reduction in hiring overall. We also find a negative effect on coworkers' earnings growth driven by delays and a clear decline in the number of promotions. Declines in hours worked and hourly wages both contribute to the earnings effects, but the effects on hours are stronger and more immediate. The relative strength of the hours response is likely facilitated by several features of the Dutch labor market, including high rates of part-time work, flexibility regarding work hours, and an inflexible system of wage setting. We find no effects on coworkers' separation rates, both overall and separately for groups of workers that are more vulnerable to layoffs.

To put the size of firms' responses in context, we compare the estimated changes in hours worked by focal and non-focal workers. Over a 21-month period around the SRA, we estimate that over 60% of the increase in hours worked by focal workers is offset, implying a high level of substitution between older and younger workers. Around three-fifths of this substitution results from hiring adjustments, with the remaining two-fifths explained by effects on incumbent coworkers. These spillovers are important because they limit opportunities for job seekers and coworkers to climb the career ladder, which is a key source of earnings growth (Bayer and Kuhn, 2020). For job seekers, most of the hiring response affects individuals who are either moving to higher paid jobs or into work. For coworkers, the earnings decline is concentrated among those who miss out on promotions. These effects are large: one six-month retirement delay reduces coworkers' earnings by around €4,700 (21% of the increase in the focal worker's earnings).

We quantify the distributional implications of the spillovers within firms, which are likely to be a major component of the policy's overall distributional impacts.[6] We show that the reform increased the number of high-paid workers in affected firms and reduced the number of low-paid workers. This widened earnings gaps by workers' age and gender: the estimated net earnings effects are positive for workers who are older (age 50+) and male, and negative for workers who are young (age 20–34) and female. These results provide causal evidence at the firm level in support of Bianchi and Paradisi's (2022) conclusion that "older workers with progressively longer working lives [have] harmed the careers of younger workers" (p. 1), and they show that the detrimental effects can extend to other lower-earning groups including women.

Finally, we consider the implications of our estimates for welfare analysis by quantifying the implied Marginal Value of Public Funds of the policy (Hendren and Sprung-Keyser, 2020). We show that, if we only consider the effects on the older population, the policy imposes a small welfare cost relative to its fiscal saving and thus appears an efficient measure for reducing government expenditure. However, accounting for within-firm spillovers approximately doubles the policy's ratio of welfare costs to fiscal savings, which suggests that policymakers should not ignore them and could use additional policies to address the effects on lower-income groups.

Our study contributes to an emerging literature on worker substitutability that exploits firm-level labor supply shocks. Studies have used exogenous shocks to hiring stemming from visa lotteries (Doran et al., 2022) and exogenous separations stemming from worker deaths (Jäger and Heining, 2022; Bertheau et al., 2022; Illing and Schwanck, 2021), parental leave

[6]Within-firm changes in inequality are important: within-firm disparities explain most of the gender wage gap in Portugal (Card et al., 2016), are contributing to a rise in the age-wage gap (Bianchi and Paradisi, 2022), and can explain around one-third of the rise in earnings inequality in the US from 1978 to 2013 (Song et al., 2019).

spells/extensions (Ginja et al., 2023; Brenøe et al., forthcoming; Gallen, 2019; Johnsen et al., 2020; Schmutte and Skira, 2022; Huebener et al., 2022), quits (Kuhn and Yu, 2021) and pension reforms (Martins et al., 2009; Hut, 2019; Bianchi et al., 2022; Boeri et al., 2021; Carta et al., 2021; Eckrote-Nordland, 2021; Hernæs et al., 2023). We contribute most directly to this final set of studies by clearly documenting the dynamics and margins on which firms respond to a representative policy that delays older workers from retiring. We show that these responses offset most of the increase in hours worked by older workers, limit the career progression of other workers, affect work hours and wages, and have important distributional, welfare and fiscal implications. In addition, our results show that many firms elect to replace retirees several months early, possibly to allow for a designated hand-over period. To our knowledge, this pattern of early replacement hiring has only been documented empirically in one other study (Kuhn and Yu, 2021), where it was studied over a shorter (two-week) notice period.

Like the studies above, our estimates are partial equilibrium, which is important to acknowledge when thinking about the overall impacts of the policy. For example, much of the decrease in hiring from affected firms may have reallocated workers across firms rather than caused aggregate employment losses. This would be consistent with the fact that only two studies have found a negative general-equilibrium effect of workforce aging on youth employment (Vestad, 2013; Bertoni and Brunello, 2021), with most finding no effect (Gruber et al., 2009; Munnell and Wu, 2013; Börsch-Supan and Schnabel, 2010; Mohnen, forthcoming). However, our results suggest that many of the affected younger workers may have ended up in worse jobs, since the hiring decline mainly blocks movements into higher-earning jobs and coworkers miss out on promotions. This is consistent with causal estimates of the effects of an aging society on youth labor market outcomes within commuting zones in the U.S. (Mohnen, forthcoming) and similar descriptive trends in many developed countries (Bianchi and Paradisi, 2022).

The rest of the paper is organized as follows. Section 2 presents our conceptual model, which shows that the implications of retirement delays are dynamic and depend on the substitutability of older and younger workers. Section 3 describes relevant features of the Dutch labor market and pension system. Section 4 discusses the data and shows descriptive evidence of the reforms' effects on older workers. Section 5 presents the main results. Section 6 highlights the magnitude of our estimates and their distributional, fiscal and welfare implications. Section 7 concludes.

2 Conceptual framework

We outline a conceptual model that shows how anticipated retirement delays affect firms' personnel decisions. We consider a three-period model, where the firm chooses its labor inputs H to maximize its profits (the capital inputs are fixed). Firm output F in period t is determined by the hours worked by three types of workers: older workers who are close to retirement (O); incumbent workers (I); and new hires (N). Under the baseline scenario, older workers reach the SRA at the end of period 1. Under the reform scenario, a pre-announced policy change moves the SRA to the end of period 2 (each period is a short unit of time, such as a few months or a year).[7] Older workers stay in the firm until the SRA, where most retire. New hires and incumbents face an exogenous probability $(1-\delta)$ of exiting the firm at the end of each period. New hires that do not exit become incumbents in the next period.

We assume the firm is a price taker in the input and output markets. We normalize the price of the output good to 1 and assume the firm pays hourly wages w_O to older workers and w_{IN} to incumbents and new hires, with $w_O > w_{IN}$.[8] Initially, we assume that the firm is not liquidity constrained (i.e., any combination of labor inputs is feasible) but relax this assumption below. We assume that firms face diminishing marginal productivity of labor inputs and decreasing returns to scale, which is the likely situation of many small-to-medium sized firms.

The firm's maximization problem is as follows:

$$\begin{aligned}
\max_{H_{1,N}, H_{2,N}, H_{3,N}, i_2, i_3} \quad & \underbrace{F_1\left(\overline{H_{1,O}}, \overline{H_{1,I}}, H_{1,N}\right) + F_2\left(\overline{H_{2,O}}, H_{2,I}, H_{2,N}\right) + F_3\left(\overline{H_{3,O}}, H_{3,I}, H_{3,N}\right)}_{\text{revenue}} \\
& \underbrace{- \sum_{t=1}^{3}\left\{w_O \overline{H_{t,O}} + w_{IN}(H_{t,I} + H_{t,N})\right\}}_{\text{labor costs}} \underbrace{- \sum_{t=1}^{3}\left\{\frac{a_N (H_{t,N})^2}{2}\right\}}_{\text{hiring costs}} \\
& \underbrace{- \sum_{t=2}^{3}\left\{\frac{a_I (i_t)^2}{2}\right\}}_{\text{adjustment costs}} \underbrace{- \sum_{t=2}^{3}\left\{\mathbf{1}(i_t < 0) T(-i_t)\right\}}_{\text{firing costs}} \qquad (2.1)
\end{aligned}$$

[7] To match our empirical analysis, we focus on the effects of raising the SRA for workers that are already old. In reality, younger workers today will eventually become old and we would expect them to retire later due to the policy. However, this is not yet relevant in our empirical setting due to the recency of the reform we study.

[8] To keep the model tractable, we assume (i) that hourly wages are exogenous and (ii) that there is an abundant supply of labor at w_{IN}. The model could be extended to allow for upward-sloping labor supply and endogenous wages. In general, we would expect the effects on incumbents' wages to have the same sign as the effects on hours — i.e., the effects would be negative (positive) when labor demand falls (rises).

subject to:

$$H_{t,N}, H_{t,I} \geq 0, \text{ for } t = 1, 2, 3 \tag{2.2}$$

$$i_t = H_{t,I} - \delta(H_{t-1,I} + H_{t-1,N}), \text{ for } t = 2, 3 \tag{2.3}$$

where $H_{t,O}, H_{t,I}$, and $H_{t,N}$ denote the hours worked by the three worker types. The firm maximizes over the hours worked by new hires ($H_{1,N}, H_{2,N}, H_{3,N}$), and hours worked by incumbents (i_2, i_3). The incumbents' hours are expressed in relation to their hours observed in the previous period, with positive (negative) values denoting an increase (decrease) in their workload (see equation 2.3). To simplify the model, we assume that the hours worked by incumbents in the first period ($\overline{H_{1,I}}$), and the hours worked by older workers ($\overline{H_{t,O}}$) are exogenously given.[9]

The firm faces quadratic cost functions in (i) the hours worked by new hires $C(H_{t,N}) = \frac{a_N(H_{t,N})^2}{2}$ and (ii) the hours change for their incumbent workforce $C(i_t) = \frac{a_I(i_t)^2}{2}$, where a_N and a_I are cost parameters. These functions reflect search costs, training costs, and costs of reorganizing the firm's internal structure. The quadratic functional form is convenient, simplifying the first order conditions in our model, and is common in models with adjustment costs (Bond and Van Reenen, 2007). In addition to the hiring and adjustment costs, it is costly to dismiss incumbent workers or to reduce their workloads. The firm must pay cost T for each hour that workloads are reduced. Finally, we note that new hires and incumbents may be imperfect substitutes (Mercan et al., forthcoming; Bertheau et al., 2022), which implicitly allows for firm-specific human capital accumulation (Jäger and Heining, 2022). This learning process may depend on the presence of older workers due to knowledge spillovers (Jarosch et al., 2021).[10]

The policy change leads older workers to delay retirement by one period. That is, at baseline we have $\overline{H_{1,O}} >> \overline{H_{2,O}} = \overline{H_{3,O}}$, but after the policy change $\overline{H_{1,O}} = \overline{H_{2,O}} >> \overline{H_{3,O}}$. To examine firms' responses to this policy, we derive the FOCs of the firm's maximization problem for hiring:

$$H_{1,N}: \; H^*_{1,N} \geq \left(F_{1,N}(\cdot) + \delta F_{2,I}(\cdot) + \delta^2 F_{3,I}(\cdot) - \left(1 + \delta + \delta^2\right) w_{IN}\right)\left(\frac{1}{a_N}\right) \tag{2.4}$$

[9] We treat the labor supply of older workers as exogenous to reflect the stringency of their employment protection. The Netherlands employs a rigorous system of rules governing the dismissals of workers on permanent contracts. The employer can either submit a case to the labor office or go to court. Both options are costly: the former involves an inconvenient and time-consuming bureaucratic procedure, while the latter implies a substantive severance payment that is proportional to workers' salary and tenure (Kabátek et al., 2022).

[10] The classic example is a designated 'hand-over' period for a new hire (or incumbent) when an experienced worker is leaving the firm. In the model, such learning spillovers could be captured by a positive relationship between the marginal productivity of incumbents and the number of older workers in previous periods.

$$H_{2,N}: \; H^*_{2,N} \geq (F_{2,N}(\cdot) + \delta F_{3,I}(\cdot) - (1+\delta)w_{IN}) \left(\frac{1}{a_N}\right) \tag{2.5}$$

$$H_{3,N}: \; H^*_{3,N} \geq (F_{3,N}(\cdot) - w_{IN}) \left(\frac{1}{a_N}\right) \tag{2.6}$$

where $F_{t,N}$ and $F_{t,I}$ denote the marginal products of new hires and incumbents in period t. These FOCs are binding unless the firm does not hire ($H^*_{t,N} = 0$). They demonstrate that the hiring response is proportional to the difference between the marginal product and the added wage costs of new hires, with the response attenuated by the hiring costs a_N.

For incumbents, the FOCs depend on whether the change in hours is positive or negative:

$$i_2: \quad \begin{cases} i^*_2 \geq (F_{2,I}(\cdot) + \delta F_{3,I}(\cdot) - (1+\delta)w_{IN}) \left(\frac{1}{a_I}\right), & \text{if} \quad i^*_2 \geq 0 \\ i^*_2 = (F_{2,I}(\cdot) + \delta F_{3,I}(\cdot) - (1+\delta)w_{IN} + T) \left(\frac{1}{a_I}\right), & \text{if} \quad i^*_2 < 0 \end{cases} \tag{2.7}$$

$$i_3: \quad \begin{cases} i^*_3 \geq (F_{3,I}(\cdot) - w_{IN}) \left(\frac{1}{a_I}\right), & \text{if} \quad i^*_3 \geq 0 \\ i^*_3 = (F_{3,I}(\cdot) - w_{IN} + T) \left(\frac{1}{a_I}\right), & \text{if} \quad i^*_3 < 0 \end{cases} \tag{2.8}$$

These expressions hold with equality unless $i^*_t = 0$. The FOCs imply that the response in terms of incumbents' hours depends on the difference between the marginal product of incumbents and the added wage costs, with the response attenuated by the adjustment costs a_I. Notice that, for firms wanting to decrease incumbents' hours ($i_t < 0$), the firing cost T drives a wedge between the optimal decrease in hours with and without such a cost. As T increases, $i^*_t \to 0$, since the costs of dismissing workers becomes prohibitive. In this case, the FOCs may be slack.

2.1 Comparative statics and predictions of the model

To assess how firms may respond to a policy that increases the hours worked by older workers in period 2, we take the partial derivatives of equations (2.4)–(2.8) with respect to $\overline{H_{2,O}}$:[11]

$$\frac{\partial H^*_{1,N}}{\partial \overline{H_{2,O}}} = \left(F_{1,N;2,O}(\cdot) + \delta F_{2,I;2,O}(\cdot) + \delta^2 F_{3,I;2,O}(\cdot)\right) \left(\frac{1}{a_N}\right) \tag{2.9}$$

$$\frac{\partial H^*_{2,N}}{\partial \overline{H_{2,O}}} = (F_{2,N;2,O}(\cdot) + \delta F_{3,I;2,O}(\cdot)) \left(\frac{1}{a_N}\right) \tag{2.10}$$

$$\frac{\partial H^*_{3,N}}{\partial \overline{H_{2,O}}} = F_{3,N;2,O} \left(\frac{1}{a_N}\right) \tag{2.11}$$

$$\frac{\partial i^*_2}{\partial \overline{H_{2,O}}} = (F_{2,I;2,O}(\cdot) + \delta F_{3,I;2,O}(\cdot)) \left(\frac{1}{a_I}\right) \tag{2.12}$$

[11] We focus on the case where one of the FOC binds but discuss how things may vary if the FOCs are slack.

$$\frac{\partial i_3^*}{\partial \overline{H}_{2,O}} = F_{3,I;2,O}(\cdot)\left(\frac{1}{a_I}\right) \tag{2.13}$$

where $F_{t,N;2,O}(\cdot)$ and $F_{t,I;2,O}(\cdot)$ denote the effect of a marginal change in the hours worked by older workers in period 2 on the respective marginal products of new hires and incumbents in period t. Based on these expressions, we can make the following predictions:

1. Anticipatory hiring responses $\left(\frac{\partial H_{1,N}^*}{\partial \overline{H}_{2,O}}\right)$:
   - Channel 1.1: Depends on the effect of having more older workers in period 2 on the marginal productivity of new hires in period 1 ($F_{1,N;2,O}$). Assumed to be zero.
   - Channel 1.2: Depends on the substitutability of incumbents and older workers in period 2 ($F_{2,I;2,O}$). This is because a share δ of new hires become incumbents in period 2. This channel will decrease (increase) hiring in period 1 if incumbents and older workers are substitutes (complements). We expect that they are substitutes (e.g., if an older worker retires, the firm may ask incumbents to work more hours).
   - Channel 1.3: Depends on the dynamic effect of older workers on the marginal productivity of incumbents in the next period ($F_{3,I;2,O}$). We expect this effect to be positive due to dynamic complementarities (e.g., due to knowledge spillovers).[12]

The net effect depends on the relative importance of channels 1.2 & 1.3. We expect channel 1.2 (substitutability in the current period) to dominate, leading to a decrease in hiring in period 1.

2. Contemporaneous hiring responses $\left(\frac{\partial H_{2,N}^*}{\partial \overline{H}_{2,O}}\right)$:
   - Channel 2.1: Depends on the substitutability of new hires and older workers in period 2 ($F_{2,N;2,O}$). Hiring will fall if new hires and older workers are substitutes. This is likely to be the case in most firms: Mercan and Schoefer (2020) find that 56% of vacancies are posted in response to quits, implying a high level of substitutability.
   - Channel 2.2: Depends on $F_{3,I;2,O}$ (see Channel 1.3). This may be positive due to dynamic complementarities.

[12]We note that similar complementarities may exist between incumbents and older workers within periods even if incumbents and older workers are net substitutes (i.e., $F_{2,I;2,O} < 0$). This is because the complementary effect of older workers on incumbents within the same period may be more than offset by a scale effect that reduces incumbents' marginal product of labor (Boeri et al., 2021).

The net effect depends on the relative importance of the two channels. Channel 2.1 (substitution in the current period) is likely to dominate, leading to a decrease in hiring.

3. Hiring responses after the shock $\left(\frac{\partial H^*_{3,N}}{\partial H_{2,O}}\right)$:

   - Channel 3.1: Depends on $F_{3,N;2,O}$. We assume this effect is positive, capturing the benefit to firms of replacing outgoing workers. This benefit is apparent in the data based on the strong vacancy response to quits (Mercan and Schoefer, 2020).

Overall, the model suggests a dynamic effect on hiring. Assuming that workers are substitutes, we should see a hiring drop in periods 1 & 2 that is compensated by an increase in period 3. It is unclear whether this drop will be fully compensated.

4. Contemporaneous adjustments in incumbents' hours $\left(\frac{\partial i^*_2}{\partial H_{2,O}}\right)$:

   - Channel 4.1: Depends on $F_{2,I;2,O}$ (see Channel 1.2). We expect this term to be negative, reflecting the substitutability of these workers in the same period.
   - Channel 4.2: Depends on $F_{3,I;2,O}$ (see Channel 1.3). This term may be positive due to dynamic complementarities.

The net effect depends on the relative importance of the same two channels as the hiring responses in period 1. Thus, we expect the contemporaneous adjustments in incumbents' hours to have the same sign as the anticipatory hiring response. As noted above, we expect channel 1 to dominate, leading to a negative effect on incumbents' hours growth. However, the effect may be zero if firms have $i^*_2 = 0$ at baseline. These firms may want to set i_2 to be negative but may not be willing to pay the firing costs.

5. Adjustments in incumbents' hours after the shock $\left(\frac{\partial i^*_3}{\partial H_{2,O}}\right)$:

   - Depends on $(F_{3,I;2,O})$ (see Channel 1.3). We expect this effect to be positive. We further note here that this term will capture any benefit to firms of replacing the hours worked by retiring workers with additional hours from incumbents. Firms may favor this option over replacing workers via hiring if hiring costs are large (Jäger and Heining, 2022) or firm-specific human capital is present.

Overall, similar to the predictions for hiring, we expect a dynamic pattern in the effects on incumbents' hours growth, with a decrease in period 2 and an increase in period 3. In essence, the reform delays both the retirements of older workers and the timing of when firms replace these workers via additional hours from incumbents and new hires.

To conclude this section, we discuss the potential implications of liquidity constraints on the presented predictions (Hut, 2019; Schoefer, 2021). We assume that a liquidity-constrained firm must pay its labor costs at the start of each period out of its current cash-on-hand, while its revenue is not received until the end of the period. In this case, the policy change makes the firm more liquidity-constrained in period 2. The firm must respond to a one-unit increase in $\overline{H_{2,O}}$ by decreasing $H_{1,N} + H_{2,N} + H_{2,I}$ by approximately $\frac{w_O}{w_{IN}}$.[13] It may be optimal for firms to spread out this response across the different margins and periods due to the diminishing marginal productivity of labor inputs. Thus, liquidity constraints are likely to reduce firms' overall demand for younger workers in addition to the dynamic delay effects outlined above.[14]

3 Key institutional details

We test the theoretical predictions in the context of the Netherlands. This section discusses key features of the Dutch labor market, pension system and the 2011/12 SRA reform.

3.1 Features of the Dutch labor market

The Netherlands is a densely populated European country with approximately 17.5 million residents. In 2019, it ranked highly among OECD countries for employment rates and GDP per capita (4$^{\text{th}}$ and 9$^{\text{th}}$ respectively). Workers enjoy high levels of job security,[15] but job tenure is similar to other high-income countries.[16] Unemployment rates have been low and stable since the turn of the century, peaking in 2014 at 7.4%, before declining over the next five years to 3.4% in 2019. One important feature of the Dutch labor market is flexibility regarding working hours and a culture of part-time work (56% of employment, 1$^{\text{st}}$ in the OECD). Part-time work

[13] This assumes that the net change in the firm's profit in period 1 is small.

[14] The impact on the liquidity constraint in period 3 is likely to be less important than in period 2. This impact will depend on the net change in firm profits in the first two periods, which would be negative if firms pay older workers more than the value of their marginal product (Lazear, 1979).

[15] The Netherlands has the strictest regulation on individual and collective dismissals for workers on regular contracts in the OECD. Source: `https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV`.

[16] Source: `https://stats.oecd.org/Index.aspx?DataSetCode=TENURE_AVE#`

is particularly common among women (75% of employment), especially mothers (Rabaté and Rellstab, 2022). This results in one of the highest gender earnings gaps in the OECD.[17] The hourly wages of most workers are governed by collective agreements (81%) and often directly linked to job tenure (Mulders, 2018). This translates into relatively steep wage-tenure profiles compared to other countries (Deelen, 2012). Overall, these features of the Dutch economy suggest that (i) a significant fraction of older workers may be paid more than the value of their marginal product of labor (Lazear, 1979), and (ii) firms may find it easier to respond to retirement delays by altering hiring decisions and making intensive-margin adjustments (flexible margins) than firing workers or adjusting wages (less flexible margins).

3.2 Overview of the Dutch pension system

The Dutch pension system consists of three pillars. The first pillar is a flat-rate public pension, which is indexed to the minimum wage and financed by pay-as-you-go social insurance contributions (de Vos et al., 2018). In 2019, this pension amounted to €1,298.19 per month for singles and €1,787.52 for couples. The second pillar consists of mandatory employer contribution schemes, which are also an important part of the system. Through the second-pillar schemes, employees receive (mostly) defined-benefit payments that supplement their first-pillar pensions. The third pillar, which consists of individual savings for retirement, is less important than in other developed countries (De Grip et al., 2012).

Individuals start receiving the public pension once they reach the Statutory Retirement Age (SRA), which was fixed at 65 years prior to the reform. The SRA also constitutes the end point of all ongoing employment contracts, which means that individuals who wish to continue working past the SRA have to be offered a new contract. Similar 'mandatory retirement' age thresholds are used by many OECD countries (OECD, 2017), particularly those with more stringent employment protection legislation. The ease of employment termination at the SRA contrasts with high levels of employment protection at earlier ages, which means that the SRA represents an important threshold for firms looking to downsize or restructure. This, combined with the fact that individuals at the SRA lose eligibility for unemployment and disability benefits, helps explain the large retirement hazard at the SRA (Atav et al., forthcoming).

[17] Source: 2018 OECD Jobs Strategy, www.oecd.org/netherlands/jobs-strategy-NETHERLANDS-EN.pdf

3.3 The 2011/12 reform of the SRA

In 2011, motivated by concerns about the long-term sustainability of the pension system, the Dutch Government passed legislation to gradually increase the SRA. Under this legislation, the SRA was set to increase by one month per year in 2013–15, two months per year in 2016–18 and three months in 2019. However, the reform was modified in 2012 to augment the increases in 2016–19 (Atav et al., forthcoming). Ultimately, the SRA increased by one month per year in 2013–15, three months per year in 2016–18 and four months in 2019.[18] Figure 1 shows the resulting cohort variation: the SRA was raised by 16 months in total, from 65 years for workers born by December 1947 to 66 years and 4 months for workers born from January 1953.

While our analysis focuses on the 2011/12 reform of the SRA, workers born after 1949 were affected by another reform passed in 2006 (see Figure 1). The 2006 reform decreased the incentive to retire early by reducing the generosity of the second-pillar payments. This increased the share of the population that worked until the SRA (Lindeboom and Montizaan, 2020; Atav et al., forthcoming). To ensure our analysis is not contaminated by cohort differences in early retirement incentives, we focus on individuals born after 1949. This means that we evaluate the SRA increases between 2015 and 2019 from 65 years and 3 months to 66 years and 4 months.

4 Data and descriptives

Our data comes from several linked population registers maintained by Statistics Netherlands. The cornerstone of our dataset is the SPOLISBUS register, which is a tax-based dataset that tracks the full population of workers living in the Netherlands from 2010 to 2020. The key information is monthly records of 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 worker's gender and month-year of birth.

4.1 Sample restrictions

To construct our analysis sample, we start with the population of Dutch residents born on or after January 1, 1950, and restrict ourselves to those who reached the SRA within the span of our data. We assign them to five nine-month birth cohorts based on their SRAs:

[18] In 2020, the SRA was frozen at 66 years and 4 months, following a 2019 reform. The SRA continued increasing in 2022 and will reach 67 in 2024. After that, it will be linked to life expectancy.

	Birth months	SRA	SRA months
Cohort 1	01/1950 – 09/1950	65 years, 3 months	04/2015 – 12/2015
Cohort 2	10/1950 – 06/1951	65 years, 6 months	04/2016 – 12/2016
Cohort 3	07/1951 – 03/1952	65 years, 9 months	04/2017 – 12/2017
Cohort 4	04/1952 – 12/1952	66 years, 0 months	04/2018 – 12/2018
Cohort 5	01/1953 – 09/1953	66 years, 4 months	05/2019 – 01/2020

Next, we impose restrictions on their employment histories. We restrict our attention to workers who were employed at the age of 64.5, since the reform is irrelevant for the employers of workers who left the labor market well before the SRA. We require workers at age 64.5 to have a strong attachment to a particular firm (continuous employment of at least 20 hours per week for the firm over the previous 1.5 years). This restriction allows us to remove seasonal laborers and other workers that are likely to exit the firm before the SRA.[19] We also exclude workers that are directors or shareholders of their firms, since their retirement incentives are more complex and they are less responsive to the SRA (Atav et al., forthcoming; Nagore García et al., 2021).

The analysis sample is built around this baseline sample of 'focal workers'. We link focal workers to their firms using their firm IDs, and we track the monthly outcomes of all workers employed by these firms over a period of four years (starting from the month the focal worker turned 63). Note that we do not require focal workers to be attached to the same 'focal firms' above age 64.5 (they can change firms or retire), and we follow the outcomes of their original firms regardless of these choices.[20] The resulting dataset spans the years 2013–2020, but we drop 2020 from the sample to avoid possible confounding effects of the COVID-19 pandemic.

The last set of sample restrictions pertains to firm characteristics. We focus on private-sector firms with a single plant/establishment and 5–200 employees.[21] We do not consider firms with multiple plants as we cannot match focal workers to their plants, making it impossible to identify their immediate coworkers. We do not consider firms with more than 200 employees because they are likely to employ multiple focal workers from different SRA birth cohorts (which complicates identification), and because the impacts of retirement delays by individual workers in large firms are difficult to quantify (low signal-to-noise ratio). To harmonize the dataset

[19]We find no evidence that the SRA increments affected workers' probability of working prior to the targeted ages (see Appendix A), which means that the SRA increases do not affect selection into the sample. This finding is consistent with Atav et al. (forthcoming) and most of the international literature.

[20]Changing firms prior to the SRA is rare among focal workers. By age 65 years and 3 months, the lowest SRA among our sample, just 2.2% of focal workers have changed firms.

[21]Very few public-sector enterprises are recorded as this small in our data (e.g., because we cannot observe the particular station that a policewoman works at), so we do not consider them in our analysis.

further, we also drop firms that experience month-to-month growth or loss of more than 10 workers, or double in size. Our results are robust to these restrictions.

Our final sample consists of 19,505 focal workers, working in 12,159 firms with approximately 550,000 coworkers. Table A1 shows the impact of each restriction on the size of the sample.[22]

4.2 Outcome variables

The key outcome variables for our regression analysis are defined at the firm level at a monthly frequency. They can be divided into three groups: focal worker outcomes, job flows and coworkers' career outcomes. For focal worker outcomes, we calculate the total firm-level (contractual) hours worked and earnings by focal workers. To facilitate the comparisons across firms of different sizes, we divide these outcomes by the size of the firm (measured when the reference focal worker is 64.5 years old).[23] For job flows, we define similar per-worker measures of hiring and coworker separations, as the monthly number of hires/separations divided by firm size.

For coworkers' career outcomes, we use information on their earnings and hours worked. We focus on *contractual* earnings and work hours, which means that we abstract from temporary fluctuations in hours and wages (e.g., from changes in overtime hours) and capture adjustments that better reflect career progression. Following Bianchi et al. (2022), we define the average monthly growth rate in coworkers' earnings (in percentage points).[24] Additionally, we define several measures of promotions and construct promotion rates per worker. While our data do not include explicit promotion indicators, we can approximate them using our monthly data on workers' earnings. We define a promotion as a sustained 10% increase in a worker's monthly (contractual) earnings at the firm.[25] Leveraging the strengths of our data, we construct similar rates for sustained increases in (contractual) hours worked and hourly wages. This allows us to decompose the effects on promotions into changes in work intensity and changes in wages.

[22]The sample restrictions retain a similar fraction of focal workers in each cohort (not shown).

[23]We define equivalent variables for non-focal workers to measure the overall substitution within firms.

[24]This measure is sensitive to extreme percentage changes in individuals' earnings, so we winsorize it at the 5$^{\text{th}}$ and 95$^{\text{th}}$ percentiles.

[25]Recorded earnings can fluctuate from month to month, depending on the length of the month, the number of public holidays and other factors. To avoid capturing these patterns, we require workers' earnings to increase by at least 10% from month $t-1$ to t, and also from month $t-2$ to $t+1$. This promotion measure excludes coworkers who were hired in the last two months to avoid capturing spurious earnings growth resulting from workers being hired midway through a month. In the robustness section, we assess the sensitivity of our findings by using promotion measures with alternative thresholds.

4.3 Descriptive statistics

Table A2 summarizes the characteristics of focal workers, focal firms and coworkers in our sample. All characteristics are measured in the month the focal worker was 64.5 years old. On average, focal workers earn €3,306 per month (in 2019 €), working 152 hours for a wage rate of €21.60 per hour. Their average earnings are 10% higher than those of their coworkers (due to working 3% more hours for 6% higher wages). Focal workers are mainly men (79.4%), which is partly attributable to lower rates of female employment at this age, partly to our focus on the private sector, and partly to our exclusion of individuals working less than 20 hours per week.

On average, focal workers work in firms with 46 employees — of which 11.2 are classified as young (age 20–34), 16.9 as middle-aged (age 35–49) and 17.8 as older (age 50+).[26] The average labor costs per firm are €144,351 per month. The average monthly hiring and separation rates were 1.05 hires and 1.07 separations per 100 workers. Most new hires had a job at some point in the previous three calendar months (72%) and most experienced an increase in their total earnings of at least 10% upon being hired (68% overall; 56% conditional on recent employment). Despite this, new hires are a cheaper source of labor for firms than focal workers, since they are paid 31% less (from the same number of hours). Among coworkers, 1.69 out of 100 experienced a 10% sustained increase in their monthly earnings, our main threshold used to define a promotion. For hours worked, the average promotion incidence was 1.38 per 100 coworkers, and for hourly wages it was 0.96 per 100 coworkers. This indicates that workers are more likely to experience an increase in their work intensity than their hourly wage, and that many promoted workers experience simultaneous increases in their wages and hours worked.

4.4 The effects of the SRA on focal workers' labor supply

In Figure 2, we split focal workers into the five SRA cohorts defined in Section 4.1 and plot the dynamics of their employment, work hours and earnings over the period of observation. Between ages 63 and 64.5, all three outcomes are roughly or exactly constant across cohorts. This is unremarkable as we require focal workers to be continuously employed at these ages. Beyond age 64.5, focal workers are free to retire and so labor supply begins to decline. Of note,

[26]Figure A2 shows the distribution of firm size among workers in the sample. The median focal worker works in a firm of 31 workers. The average firm is smaller (with a mean of 33 and median of 21), since larger firms are more likely to employ multiple focal workers.

we continue to see similar labor supply patterns across cohorts until age 65 years and 3 months (the earliest SRA among the cohorts in our sample). For each cohort, we see a small drop in employment in the month when individuals reach the SRA and a large drop in the subsequent month. The two drops are split more evenly for work hours and earnings, as many workers retire midway through the SRA month. Over these two months, employment rates decline from approximately 80–90% to 30–35%, monthly hours worked decline from 120–140 to 35–40 hours and monthly earnings from €2500–2900 to €700–800. The relative decline in hours worked (70–72%) is larger than for employment (60–63%), because hours worked fall even among those workers who continue working past the SRA (their mean change is -38 hours per month). Past the SRA thresholds, the outcomes are once again indistinguishable across cohorts, indicating that the observed differences in retirement timing are driven by the SRA reform.

These dynamics underscore that the SRA creates a sharp decline in the hours worked by older workers. If workers are substitutable, firms may respond to this discontinuity by replenishing their labor stocks through hiring and providing new career opportunities for coworkers. In addition, Figure 2 confirms that the SRA increases cause proportional retirement delays among the majority of workers who are employed at the targeted ages. This means that, if firms respond to the retirements at the SRA, then the reform is likely to affect the timing (and potentially the strength) of these responses. In the next section, we explore these effects in detail.

5 The effects on firms and coworkers

5.1 Firm dynamics around the SRA

As a starting point, we establish that firms and coworkers do respond to the SRA-induced retirements of focal workers. To this end, we estimate an event-study model that captures the dependence of firms' and coworkers' outcomes on the proximity to focal workers' SRA.

The running variable for this model is a re-centered measure of the focal worker's age ('event age'), which is equal to zero in the calendar month when the worker reaches the SRA. The observation period is restricted to a 19-month window surrounding the SRA, starting 12 months prior and ending 6 months afterwards. The functional form is as follows:

$$y_{it} = \xi_i \left(\alpha + \sum_{\substack{j \\ j \neq \text{ref.}}} \gamma_j \mathbf{1}(\text{ev\_age}_{it} = j) \right) + \tau_t + \epsilon_{it}, \tag{5.1}$$

where y_{it} is a firm/coworker outcome linked to focal worker i in month t (e.g., the monthly hiring rate in the focal firm), ξ_i is a normalization factor, α is the constant term, $\sum_j \mathbf{1}(\text{ev\_age}_{it} = j)$ is a set of event-age dummies, τ_t is a set of calendar month-year fixed effects, and ϵ_{it} is an error term. The normalization factor $\xi_i = \frac{1}{\text{firmsize}_i}$ divides the constant term and the event-age dummies by the number of workers in the firm (measured when the focal worker was 64.5 years old). This aligns the key explanatory variables with our outcome variables, which are also normalized by the same measure of firm size. The implicit assumption is that the retirement of a single focal worker will have the same impact on the numerator of y_{it} (i.e., the *number* of hires, separations or promotions at the firm) regardless of firm size, which means that the impact on per-worker rates will be inversely proportional to firm size. This assumption is consistent with Jäger and Heining's (2022) results, which show that the impact of an unexpected death on the earnings of individual coworkers is declining in firm size.

We focus on the event-age coefficients, γ_j, which quantify the changes in y_{it} relative to the reference period (10–12 months prior to the SRA). Because of the normalization, γ_j can be interpreted as a nominal change in the outcome of interest (e.g., number of workers hired) associated with a single focal worker being j months away from the SRA. The estimates of γ_j for each of our outcomes are presented in Figure 3. In each subfigure, we present the estimates with 95% confidence intervals. We cluster standard errors by firm and present both conventional point-wise confidence intervals and more conservative confidence intervals that account for multiple hypothesis testing.[27]

Before discussing the firm and coworker outcomes, we show how focal workers' proximity to the SRA affects their own work hours and earnings (Figures 3a and 3b). The estimates show a sharp decrease in both outcomes at the SRA. On average, the work hours drop by 80 per month, which is equivalent to 0.46 fewer full-time employees. This is matched by a proportional decline

[27] We use a Bonferroni correction, which assumes that the 16 hypothesis tests in each regression are independent. The associated confidence intervals are thus equivalent to a 99.7% confidence interval ($0.997 = 1 - 0.05/16$).

in earnings of approximately €1,600 per month.

Figure 3c shows the dynamics of hiring, and confirms that firms respond to the SRA-induced retirements by hiring new workers. The coefficient estimates are positive and statistically significant between months -3 and +1, with the total hiring effect over this period amounting to 0.152 hires per 1 focal worker. The effects dissipate after this period and hiring returns to a level that is similar to that in the reference period. Overall, the increase in hiring between months -3 and +1 accounts for 33% of the decline in (full-time equivalent) focal workers, which indicates that two-thirds of retirees are not (immediately) replaced by new hires. Interestingly, the estimates suggest that firms often hire replacement workers that start working a few months before the retirement event. This matches our theoretical framework, suggesting that some firms prefer to have a hand-over period to facilitate the transmission of firm-specific human capital from focal workers to the new hires.

Figure 3d shows that the SRA does not have a strong effect on coworkers' separations. While we might expect that coworkers become less likely to leave the firm past the SRA (due to better promotion prospects), this does not seem to be the case. If anything, job separations increase slightly, although our wider confidence intervals include zero.

Figure 3e shows the effects on the average percentage point growth in coworkers' monthly earnings. The estimates indicate that earnings growth is higher in the months surrounding the SRA, peaking in the month when the focal worker reaches the SRA. And since earnings growth returns to baseline levels in subsequent months, the gains accumulated around the SRA persist. With respect to the magnitude of these effects, we note that the numerator of y_{it} here is the sum of individual coworkers' growth rates within the firm. If we were to assume that this increase is fully borne by a single coworker, the earnings growth of the coworker over this period would be 2.61 percentage points above the baseline. For a person with average earnings, this would add €940 to their income over the next 12 months.

Figure 3f shows an increase in coworker promotions (denoting a sustained 10% increase in monthly earnings) across months 0–5, which amounts to 0.130 additional promotions per focal worker. This increase in promotions is similar in magnitude to the estimated increase in hires (0.152 hires), and it can explain almost all of the increase in coworkers' earnings: based on the mean increase in earnings for promoted coworkers (€569 per month), the estimated increase in promotions explains 95% of the estimated increase in coworkers' earnings ($569 \times 12 \times 0.130 =$ €887.7). These results indicate that the increase in coworkers' earnings at the SRA is concentrated among the subset of coworkers who are promoted when the focal worker retires.

Interestingly, a large share of the promotion effect is explained by changes in hours worked rather than hourly wages. Figure 3g shows that 'hours promotions' (denoting a sustained 10% increase in hours worked) display a similar pattern and magnitude as 'earnings promotions', whereas Figure 3h shows that 'wage promotions' (a sustained 10% increase in hourly wages) do increase following the SRA, but not to the same extent or as immediately. Across months 0 to 5, hours promotions rise by 0.108, with 67% of the effect occurring in the first three months, while wage promotions rise by 0.036, with 37% of the effect occurring in the first three months.

Taken together, our results show that the retirements of older workers at the SRA increase the career opportunities available to other workers, although at a rate less than one-for-one. The positive career consequences are obvious in the case of promotions, but similar positive consequences are also typically present for new hires (Bayer and Kuhn, 2020). Indeed, we find that two-thirds of the increase in hiring around the SRA is explained by individuals who experience an earnings increase of at least 10% (Figure A3).

5.2 Analyzing the effects of SRA increases on firms and coworkers

Having established that firms and coworkers do respond to the SRA thresholds, we turn to the effects of the 2011/12 SRA reforms. To obtain causal estimates of the reform effects, we employ a differences-in-differences (DiD) design, in which we compare the outcomes of firms and coworkers linked to focal workers from distinct SRA birth cohorts.

Our five SRA birth cohorts (defined in Section 4.1) form four adjacent cohort-pairs, which are used to estimate the effects of increasing the SRA by 3–4 months. Within each pair, the earlier cohort (e.g., Cohort 1) acts as a control group for the later cohort (Cohort 2) that is subject to the SRA increase.[28] We also estimate models that compare non-adjacent SRA cohorts, which allow us to study the effects of larger SRA increases (up to 13 months). To simplify the exposition, our conceptual discussion focuses on the model with adjacent cohort-pairs.

[28]Adjacent cohorts of focal workers have similar observable characteristics and work in similar firms (see Table A3), although we note that covariate balance is not a strict requirement for the causal identification of our preferred estimates, which allow the outcomes of treated and control units to differ by setting a reference period.

The effects of the SRA increases are assumed to be dynamic, varying with the age of focal workers. In principle, the baseline model could yield four sets of treatment effects, one for each cohort-pair. However, since the underlying variation in the SRA is comparable across cohort-pairs, we aggregate them into a single "stacked regression" (Cengiz et al., 2019).[29] Similar to our event-study model (which pools identifying variation over the five SRA birth cohorts), the stacked regression model yields a single set of treatment effects that pools variation over the four cohort-pairs.[30] One notable deviation from the event-study model is that the event-age measure used here is defined in relation to the SRA threshold applicable to the *control group* within the given cohort-pair. This means that the model compares the firm and coworker outcomes linked to focal workers of the same age, with the difference being that the control group reaches the SRA 3–4 months earlier than the treated group (at event age 0, the control group has reached the SRA while the treated group is still 3–4 months away from reaching it).

The functional form of the stacked regression model is as follows:

$$y_{ipt} = \xi_i \left(\sum_j \sum_p \alpha_{jp} \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{pair}_{ip} + \sum_j \beta_j \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{treat}_{ip} \right) + \tau_t + \epsilon_{ipt} \quad (5.2)$$

where y_{ipt} is an outcome linked to focal worker i in cohort-pair p in month t (e.g., the monthly hiring rate in the focal firm), and ξ_i is the normalization factor. The first term in the parentheses, $\sum_j \sum_p \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{pair}_{ip}$, is a set of event-age by cohort-pair fixed effects. Similar to the event-study model, these effects capture the baseline dependence of control groups' outcomes on the proximity to focal workers' SRA. The second term, $\sum_j \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{treat}_{ip}$, is a set of event-age fixed effects interacted with the treatment dummy. Their coefficients, the β_j terms, capture the treatment effects: the difference in the outcomes of the treatment and control groups at event-age j. Note that these coefficients are identified solely from the variation within cohort-pairs (because the baseline event-age effects $\sum_j \sum_p \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{pair}_{ip}$ are assumed

[29] "Stacking" is an attractive approach to estimate DiD regressions that are deliberate about which units form the control group, avoiding the problems that have been recently highlighted with two-way fixed effect models when previously-treated units contribute to the control group (see Roth et al., forthcoming, for a review). By construction, our control groups are "never treated" by the marginal SRA increase that they identify.

[30] Here, "stacking" duplicates the observations of focal workers in Cohorts 2–4, with one observation allocated to the treatment group (with Cohorts 1–3 forming the control groups) and one to the control group (with Cohorts 3–5 forming the treatment groups). Standard errors are clustered by firm, which accounts for duplication and the fact that firm or coworker outcomes may be counted multiple times if the firm has multiple focal workers. Standard errors are similar if we also cluster by the focal worker's month-year of birth (which dictates the SRA).

to be pair-specific). The model also includes calendar month-year fixed effects τ_t (which account for seasonality and business cycle fluctuations), and an error term ϵ_{ipt}.

In terms of the observation period, the DiD model leverages the full span of longitudinal records defined in Section 4.1 (i.e., the 48 months when focal workers are aged 63–66 years old). This means that our baseline model estimates the coefficients β_j over a long pre-treatment period, which allows us to evaluate whether the parallel-trends assumption is likely to hold. Upon confirming that this is the case, we switch to a more parsimonious model specification that replaces the coefficients β_j corresponding to the interval $j \in [-27, -7]$ by a single coefficient for the pooled pre-treatment reference period. This allows us to obtain more precise and reliable estimates of the reform effects, and it is reflected in a slightly-adjusted functional form:

$$\begin{aligned} y_{ipt} =& \xi_i \left(\left(\sum_j \sum_p \alpha_{jp} \mathbf{1}(\text{ev\_age}_{ipt} = j) + \sum_p \psi_p \text{treat}_{ip} \right) \text{pair}_{ip} + \sum_{\substack{j \\ j \neq \text{ref.}}} \beta_j \mathbf{1}(\text{ev\_age}_{ipt} = j) \times \text{treat}_{ip} \right) \\ &+ \tau_t + \epsilon_{ipt} \end{aligned} \tag{5.3}$$

Equation (5.3) modifies equation (5.2) in two ways: (i) it omits the treatment by event-age interactions over the reference period and (ii) it includes a set of treatment-pair dummies $\sum_p \text{treat}_{ip} \times \text{pair}_{ip}$ (with coefficients ψ_p), which controls for any pair-specific differences between treatment and control groups over the reference period.

Analogous research designs are used to estimate the effects of larger increases in the SRA. We estimate modified versions of equations (5.2) and (5.3) with non-adjacent cohort-pairs: for example, to study SRA increases of 6–7 months, our sample consists of three cohort-pairs (Cohorts 1 & 3, 2 & 4, and 3 & 5), while for increases of 13 months we have just one cohort-pair (Cohorts 1 & 5). This means that the sample is smaller and the estimates are less precise when we study larger SRA increases. Despite this, these estimates complement our analysis of adjacent cohorts in two important ways. First, the estimates allow for a longer 'treatment period' (when focal workers in the control group have reached the SRA but those in the treatment group have not). This facilitates identification because the responses of treated and control firms to the SRA-induced retirements are less likely to overlap. Second, this analysis allows us to assess the robustness of our estimates to different definitions of the control group. The fact that we find quantitatively similar responses across the model specifications reinforces our findings.

5.3 Effects of SRA increases

In this section we discuss our DiD estimates of the effects of the SRA increases on firm-level outcomes. In contrast to the event-study estimates presented in Section 5.1, here we estimate treatment effects at a quarterly frequency (e.g., the effect at event-age 0 corresponds to the total effect over months 0, 1 and 2).[31] The result of this adjustment is that the presented estimates are more precise, and their quarterly frequency matches the policy variation, which shifts the SRA by at least one quarter per cohort-pair.

We present most of our DiD estimates graphically with 95% confidence intervals (conventional and adjusted for multiple-hypothesis testing). We present our estimates of equation (5.2) in the Appendix; this specification allows us to assess the parallel-trends assumption over a long pre-period. We present our estimates of equation (5.3) in the main set of results; this is our preferred specification for hypothesis testing and inference, since it accounts for any difference in the outcomes of the treatment and control group well before focal workers reach the SRA.

We begin with the estimated effects of SRA increases on the hours worked and earnings of focal workers (Figures 4 and A4). Each figure is split into four panels based on the size of the assessed SRA increase (3–4 months; 6–7 months; 9–10 months; and 13 months), with the estimates for hours worked on the left and the estimates for earnings on the right. Figure A4 provides support for the parallel-trends assumption; while the coefficient estimates corresponding to the pre-treatment period are consistently positive, which suggests that focal workers in the treatment group have slightly higher earnings and hours than those in the control group, there is no evidence of differential trends. We thus focus on the estimates in Figure 4, which adjust for differences in the outcomes of the treatment and control group well before the SRA. The estimates show a large increase in the hours worked and earnings of treated focal workers starting in quarter 0. At this point, focal workers in the control group have reached the SRA threshold, but those in the treatment group have not. The positive effects on hours and earnings continue

[31]Strictly speaking, our regressions estimate average monthly effects over the relevant quarter. However, we multiply the estimates and standard errors by three in the tables and figures to give the total effects in each quarter. This causes an approximation error for our earnings growth measures, since earnings growth is multiplicative rather than additive, but the error is minute given the size of the regression estimates. We adjust for this error when we calculate the implied cumulative effects on coworkers' earnings in Section 6.

until all treated workers have reached the SRA.[32] In line with our event-study analysis, the estimates peak at around 240 hours per quarter (0.46 full-time equivalents) and €5,000.

Next, we examine the effects on the hiring and separation rates of coworkers. We first verify that there is no evidence of pre-trends for either outcome (Figure A5), and then focus on our preferred estimates (Figure 5). Starting with hiring, the estimates are negative and statistically significant in quarter -1 across all panels, indicating that treated firms hire fewer workers in this period than control firms (the estimates range from 0.069 to 0.098 fewer hires per focal worker). A negative effect in this period conforms with the predictions of our theoretical model, and the findings of our event-study analysis: in quarter -1, control firms begin hiring replacements for focal workers who are about to retire (this is the preemptive response of firms that value handover periods). Treated firms do not start hiring yet, because their focal workers remain attached to the firm for an additional 1–4 quarters (Figure 4). The hiring response of treated firms is delayed proportionally to the SRA-induced retirements of focal workers, which gives rise to a wave-like pattern of treatment effects that is widening with the length of the retirement delays. Interestingly, the initial negative effects do not appear to be fully compensated by the subsequent positive effects, which suggests that the reform may have caused treated firms to hire fewer workers overall. These cumulative effects are discussed in detail in Section 6.

For coworker separations, none of the estimates are statistically significant, which suggests that there are either no effects on both dismissals and quits, or that the two effects offset each other. While we cannot disentangle the two effects in our data, we can examine the separation rates of workers who differ in terms of their job security. Specifically, we consider separations by coworkers with (i) insecure work contracts (7.6% of the sample), who are relatively easy to dismiss and (ii) secure work contracts (92.4%), who are very difficult to dismiss due to the strong employment protection in the Netherlands. We find null effects for both groups (Table A4), which suggests that the SRA increases had little to no effects on both margins.

Turning to the effects on coworkers' career progression, Figure 6 shows the effects on the average earnings growth of coworkers on the left and coworkers' promotion rates on the right. (Figure A6 confirms that there is no evidence of pre-trends). For coworkers' earnings growth,

[32] The positive effects extend beyond the 'treatment period' denoted by the vertical lines in Figure 4. This is because (i) focal workers born in year 1953 are subject to a delay that is one-month longer than the rest, and (ii) some workers retire midway through the SRA month, which leads to mechanical spillovers into the subsequent month (this also explains why the Q0 treatment effects in panels b–d are smaller than in subsequent quarters).

the estimates in quarter 0 are consistently negative, and the effects are highly significant for the 3–4 month and 6–7 month treatments ($p < 0.01$).[33] As discussed in Section 5.1, a simple way to interpret the magnitude of these estimates is to assume the impact falls on a single coworker. For this 'affected' coworker, the point estimates imply a negative effect on their earnings growth ranging from -2.8 to -5.5 percentage points in quarter 0. In some panels, there is suggestive evidence of a compensating positive effect after treated focal workers reach their SRA, but the associated estimates are not statistically significant.

For coworker promotions, the estimates show a similar pattern and tighter confidence bounds. The effects in quarter 0 are negative and statistically significant for all four treatments (with a smaller negative effect in quarter -1 for some treatments). The point estimates in quarter 0 are large, ranging from -0.077 to -0.150 promotions per focal worker. These negative effects extend across the affected quarters, and there is little evidence of a compensating positive effect after treated focal workers reach the SRA. This indicates that the SRA-induced retirement delays reduce coworkers' earnings by limiting their promotion opportunities.

To decompose the promotion effects, Table A5 shows the estimates for 10% sustained increases in hours worked and 10% sustained increases of hourly wages. (Figure A7 shows that there is no evidence of pre-trends for these outcomes). Table A5 shows that the effects on hours worked mimic the pattern of effects on earnings, showing a significant drop in "hours promotions" in quarter 0. Table A5 shows that negative effects on wage growth are also relevant, but the individual estimates are typically smaller and more dispersed. For the 3–4 month treatment, we observe a negative estimate on wage promotions in quarter 0 of -0.019, and the estimates are mostly negative in subsequent quarters. For the 6–7 month treatment, we observe a similar pattern, but the negative estimates in quarters 0 and 1 are larger, and both are individually significant at the 5% level (i.e., without the Bonferroni adjustment). For the larger treatments, the estimates are consistently negative during and after the affected quarters, although none are statistically significant at the 5% level if we adjust for multiple-hypothesis testing.

5.4 Robustness and effect heterogeneity

In this section, we examine the robustness of our results and explore heterogeneity in the effects.

[33] The sign of these estimates is the same as for the hiring effects in quarter -1, which verifies a prediction of our theoretical model (see the discussion under Channel 4 in Section 2.1).

Robustness. First, we investigate the robustness of our earnings estimates. Instead of investigating the effects on the average earnings growth rate across all coworkers, here we focus on the growth rate in the combined earnings of "stable coworkers" (Table A6), who worked at the firm for at least two months prior to the point of observation. This measure places less weight on changes among lower-paid, seasonal and temporary workers, and more weight on changes affecting the core workforce. The estimates are of similar size and show the same dynamic pattern as the estimates in Table A5. Changes in the combined hours worked of coworkers explain most but not all of these dynamics (columns 5–8), which indicates that changes in wages also contribute. Second, we show that our results are robust to more stringent definitions for promotions (Table A7). While the estimated effects shrink in absolute terms under more stringent thresholds, the pattern in the estimates remains similar. We also verified that there was no evidence of an effect on coworker demotions (sustained 10% earnings decreases), which suggests that the promotion effects we identify reflect persistent changes in workers' earnings. Third, we show in Table A8 that our results are robust to other model adjustments: (i) to clustering by the focal worker's firm *and* month-year of birth (column 2) and (ii) with alternative samples that (a) allow firms to move outside the 5–200 worker range (column 3) or (b) drop observations linked to firms with multiple focal workers in a given cohort-pair (column 4).[34] Finally, we note that a potential limitation of our identification strategy is that treated and control units within a cohort-pair reach a given event age in different months. To assess the importance of time-by-age confounders, we estimate placebo regressions by defining a group of focal workers born ten years later (1960–63). We assume that these workers reach a 'fake SRA' threshold in the same calendar month as someone who is exactly ten years older. We find no evidence of any effects of increases in the 'fake SRA' on our main outcomes (Figure A8).

Effect heterogeneity. To assess effect heterogeneity, we focus on hiring and coworker promotions (because these are the two most strongly affected outcomes) and we evaluate the effects of the 3–4 month treatment (which yields the most precise estimates). In Figure 7, we show the treatment effect estimates corresponding to the most affected period for each outcome: quarter -1 for hiring and quarter 0 for promotions. This exposition allows us to compare the treatment effects for many subgroups in a single figure, and we can confirm that the comparison

[34]For sample (a), we allow firms to shut down, and code outcomes as zeros if that occurs. We find no consistent evidence that the SRA increments affect firm survival (Table A9).

of the full set of effect estimates yields similar patterns. Note that we present the implied *percentage* effects for a median-sized firm, which accounts for differences in the baseline hiring and promotion rates across subgroups. (Figure A9 contains the untransformed estimates.)

We start by showing how the effects depend on the relative position of focal workers and coworkers within the firm (Figure 7, top panel). We divide both focal workers and coworkers into "above-median earners" and "below-median earners" based on the median earnings observed in their firm. This allows us to test whether there is stronger substitution away from coworkers who earn a similar amount to the focal worker. The estimates show that the hiring dynamics are driven by substitution between similar types of workers. We observe strong treatment effects when both focal and non-focal workers are ranked either above or below the median, but no detectable effects for other earnings combinations. For promotions, the estimates show a similar pattern, but we also observe a negative effect of high-ranking focal workers on the promotions of lower-ranked coworkers (possibly reflecting downstream effects of promotions within the firm).

Next, we show how the effects vary based on coworkers' characteristics (Figure 7, middle panel). The relative impacts on hiring are larger among women and they are increasing in the age of new hires, although these differences are less pronounced in absolute terms (Figure A9). For promotions, the estimates show evidence of a decline among all groups except older workers, for whom the estimate is similar in magnitude but not statistically significant. The relative impacts are largest among female and middle-aged coworkers.

Finally, we show how the effects vary based on firm characteristics (Figure 7, bottom panel). The effects on hiring are similar irrespective of the firm's size, level of productivity (proxied by average wages), and whether the firm is growing or shrinking.[35] The effects on promotions are driven by smaller and less productive firms, which is consistent with Bianchi et al.'s (2022) finding of larger effects on coworkers in firms with fewer high-wage jobs. We also find a larger dynamic effect on promotions in growing firms, possibly because these firms have a stronger need to replace retirees to maintain their current operations.[36]

[35] We define firms that are growing/shrinking based on the change in the number of workers at the firm in the 18 months after the focal worker turns 63. We define high- and low-wage firms based on the median of the average wage rate (total earnings divided by total hours) at firms over the same period.

[36] This is supported by the event-study estimates, which show a much stronger spike in promotions around focal workers' SRA for growing firms (not shown).

6 Magnitude and importance of within-firm spillovers

In the previous section, we summarized the dynamic impacts of the SRA reform on firms' personnel decisions and coworkers' outcomes. Here, we highlight the overall magnitude and practical importance of these spillover effects. In Section 6.1, we present the cumulative effects of the reform on the outcomes of interest, and compare them to the effects of other reforms analyzed in the literature. In Section 6.2, we assess the distributional implications of firms' responses, and we discuss the implications for welfare analysis in Section 6.3.

6.1 Cumulative effects of retirement delays

We calculate cumulative reform effects by estimating a version of equation (5.3) with a single treatment effect for the 21-month period from quarter -2 to 4.[37] We present the results in Panel A of Table 1. To make the estimated effects more generalizable, we re-scale the estimates to represent the causal effects of one delayed retirement by a full-time worker (rather than one SRA increase).[38] This presentation of the effects provides a useful benchmark for future studies since it accounts for the intensity of the first-stage effect on focal workers (which is context specific).

For hiring, the cumulative effects are consistently negative, and the size of these effects increases with the length of the retirement delays. For example, the estimated hiring decline in response to a single retirement delay is 0.126 workers if the delay is 3–4 months ($p = 0.078$) and 0.554 workers if the delay is 13 months ($p = 0.072$). This suggests that raising the SRA not only delays hiring but also decreases its overall incidence. For separations, the cumulative effects are consistently close to zero and highly insignificant.

For coworker promotions, we see pronounced negative effects that grow with the length of the retirement delays. For example, the firm promotes 0.292 fewer coworkers in response to a 3–4 month retirement delay and 1.340 fewer coworkers if the delay is 13 months (both $p < 0.01$). These changes reflect declines in both hours and wage promotions. Interestingly, the relative importance of the two channels depends on the length of the retirement delays: the overall earnings effect is driven primarily by hours promotions when the delays are short, but it

[37]We obtain similar but slightly less precise estimates if we add up the quarterly treatment effects from equation (5.3) and use the Delta rule to construct standard errors.

[38]Specifically, we divide the estimates and standard errors by the estimated change in the number of full-time equivalent focal workers over the period of the SRA increase (which ranges from 0.475 for the 3–4 month increase to 0.434 for the 13 month increase). We show the untransformed estimates in Table A10.

is split more evenly between hours and wage effects when the delays are longer. This matches our observation that coworkers' hours spike immediately following the focal workers' retirements (Figure 3g) whereas the wage responses are smaller and less concentrated within the months immediately following the SRA (Figure 3h). Accordingly, longer retirement delays will cover a larger share of post-retirement wage promotions than short delays.

To illustrate the cumulative effects of delayed retirements on coworkers' earnings, we use our point estimates from equation (5.3) and assume that the effects are borne by a single incumbent with average earnings. Our point estimates imply that, for each additional €1 paid to focal workers, the substitute coworker loses around €0.21.[39] This translates to an earnings loss of €4,700 for a single retirement delay of six months.

To quantify the overall amount of substitution within firms, we estimate the cumulative effects on the total hours and earnings of non-focal workers and divide them by the estimates for focal workers. For non-focal workers overall, we estimate negative and statistically significant effects on hours worked and earnings that increase with the length of the retirement delay (Table A10). Our point estimates imply that these effects offset 66–90% (31–70%) of the effects on the hours (earnings) of focal workers (Table 1, Panel B).[40] For hours worked, we can reject the null hypothesis of zero crowding out for all four treatments at the 1% level, but we cannot reject the null of complete crowding out at even the 10% level.

Next, we demonstrate the importance of firms' labor demand responses across different margins and over time using our estimates of equation (5.3). Figure 8a shows the implied changes in the hours worked of focal workers and non-focal workers in event time in response to a 3–4 month retirement delay.[41] The estimates are normalized by the change in focal workers' hours recorded in quarter 0, and we decompose the change in non-focal hours into two components: changes due to hiring adjustments and changes in coworkers' hours.[42] Echoing the results presented in Figure 4a, focal workers' hours are subject to a spike in quarter 0 that partly extends

[39]€0.21 is the average effect implied by our estimates of the four treatments (which imply a range of €0.16–0.32).

[40]The estimated range for earnings is closer to zero than for hours worked, but this pattern is not inconsistent with the presence of modest negative effects on coworkers' wages. The average hourly wages of incumbent coworkers (€20.30) and new hires (€14.94) are lower than the focal workers who delay retirement (€21.90), which means that spillovers on hours worked mechanically have a stronger offsetting effect on work hours than earnings.

[41]We show the estimates for all treatments, along with 95% confidence intervals, in Figure A10.

[42]We use our point estimates of the effects of a 3–4 month increase in the SRA on the hiring rate and the combined hours growth of coworkers at the firm. Our calculations also assume that the affected workers work the average number of hours (for a new hire or an incumbent) and have average separation probabilities.

to quarter 1 and then disappears. This increase is countered by decreases in hours worked by new hires and incumbent coworkers, which are less acute but more persistent. Figure 8b shows the resulting cumulative changes in hours worked from each source, along with the net effect. The estimates in this panel are normalized by the cumulative change in focal workers' hours by the end of quarter 4. We see that the net effect on hours worked is initially negative, reflecting the preemptive hiring response. In quarters 0 and 1, the additional hours worked by focal workers more than offset this decline, rendering the net effect positive. By quarter 2, all affected focal workers have retired and so the net effects start to fall, reflecting the persistent declines in hours worked by new hires and incumbents. Changes in hiring explain most of the decrease (61%).

Overall, our results suggest that Dutch firms responded to the SRA reform on different margins than the similarly sized Italian firms studied by Bianchi et al. (2022). In terms of the effects on job flows, Bianchi et al. estimate that a one-standard-deviation increase in retirement delays causes a 10% increase in layoffs ($p < 0.001$) and a 1% decrease in hiring ($p > 0.1$). In contrast, we find no evidence of an effect on layoffs and a delay and decrease in hiring, which is consistent with other studies analyzing firms' hiring responses.[43] Several factors may explain the different impacts on Dutch and Italian firms, including (i) the suddenness of the Italian reform, (ii) the economic downturn experienced by Italian firms over the implementation period, and (iii) the higher levels of employment protection in the Netherlands. In terms of the effects on coworkers' earnings, our results match Bianchi et al.'s findings: we find strong negative effects on coworkers' earnings, and these effects are concentrated among workers who miss out on promotions. An important distinction is that we do not restrict our sample to full-time coworkers, which allows us to demonstrate that the overall earnings effects is driven by changes to both hours worked and hourly wages.

Despite these differences, the responses from small-to-medium-sized Dutch and Italian firms have an obvious thing in common: they have negative consequences on the careers of substitute workers. In the Dutch context, we estimate negative effects on coworkers' earnings, and we show that firms' hiring responses mainly affect job seekers who would have increased their earnings by at least 10% (Table A11). Most of this effect occurs among individuals moving between jobs, but there are also negative effects on individuals seeking to move into employment (Table A12).

[43] Negative effects on hiring are found in several studies (Martins et al., 2009; Hut, 2019; Eckrote-Nordland, 2021; Hernæs et al., 2023). The exception is Carta et al. (2021), which examines larger firms and finds a positive effect.

6.2 Distributional implications within firms

In this section, we assess how the reform affected existing within-firm earnings disparities between low and high earners, young and old workers, and women and men. We do this by estimating the reform's impact on the number of employees of each type within affected firms at different parts of the population earnings distribution. We then use these estimates to assess the reform effects on the total earnings of each group (see Appendix B for technical details).

The results are presented graphically in Figure A11. The estimates show the within-firm consequences of a single retirement delay of six months on the average number of workers per ventile of the distribution of earnings over quarters -2 to 4. These estimates are based on the model parameterization corresponding to a 6–7 month increase in the SRA.

Figure A11a shows that the net impact of the reform is an increase in the number of high-paid employees at affected firms (since the positive direct effects on focal workers outweigh the negative spillover effects on non-focal workers) and a decrease in the number of low-paid employees (since the spillovers dominate the direct effects). To understand the magnitude of these effects, Table 2 shows that this delay increases the amount earned by individuals in the top 50% of the population by around €17,000, and decreases the amount earned by the bottom 50% by ∼€4,000. These results imply a growing gap in the earnings share of the two groups within affected firms. For a median-sized firm (21 workers), the estimates imply a 2.7% increase in the top-50 to bottom-50 earnings ratio within the firm.[44] Half of this divergence is mechanical (due to the retention of focal workers), and half is attributable to spillovers on other workers.

The remaining panels in Figure A11 document the net impact of the reform on the earnings distribution by age and gender. The reform increases the number of older workers and male workers employed at affected firms, since the direct effects of delayed retirements on these groups are larger than the spillover effects. This increase is particularly pronounced among high-paid employees, who are disproportionately older and male (Figure A12). In contrast, the number of younger and female workers falls across the earnings distribution, particularly at lower-earning ventiles where such workers are over-represented. Table 2 shows that a six-month retirement delay induces older workers to earn ∼€21,000 more and the young to earn ∼€6,000 less. For men, the net gain is ∼€15,000 and for women the net loss is ∼€2,000. For a median-sized firm,

[44] We define the top-50 to bottom-50 earnings ratio within the firm as the total earnings of individuals in the top half of the population earnings distribution divided by the total earnings of individuals in the bottom half.

the estimates imply a 5.6% increase in the young-old earnings ratio and a 4.0% increase in the male-female earnings ratio, with spillovers explaining around 45% of these effects.

These results demonstrate that the retention of older workers within affected firms disproportionately crowded out career opportunities for lower earners, young workers and women, in each case widening disparities in earnings. As discussed in Section 6.1, most of the spillovers to non-focal workers are driven by decreases in hiring. While these individuals may have gained or retained employment elsewhere, it is likely that most would have earned less, since the majority of the hiring decline is among workers moving up the career ladder (Table A11).

6.3 Implications for welfare analysis

In this section, we highlight the importance of accounting for within-firm spillovers when assessing the welfare implications of retirement policies. Specifically, we present estimates of the marginal value of public funds (MVPF) of a small change in the SRA. The MVPF is the ratio of the benefits to society of a policy divided by its net fiscal cost (Hendren and Sprung-Keyser, 2020). Estimates of the MVPF allow policymakers to compare different policy options that target the same group of individuals and choose the policy with the most 'bang for buck'.

We present our calculations in Appendix C. If within-firm spillovers are ignored, our estimates of the MVPF range from 0.239 to 0.560, depending on how leisure is valued by older workers.[45] These estimates are well below 1, since the estimated social welfare costs of the policy are much smaller than the fiscal savings.[46] This suggests that raising the SRA may be an efficient way for the government to reduce expenditure on older individuals. However, these calculations do not consider within-firm spillovers, which can significantly change estimates of the MVPF (Paradisi, 2021). Indeed, our estimates of the MVPF increase considerably (to 0.465 and 0.837 respectively) when we account for the spillovers on new hires and incumbent coworkers. Thus, increases in the SRA start to look less attractive as a policy option once we consider spillovers on other groups, particularly if the government places higher welfare weights on the groups that are disproportionately crowded out by firms' responses (lower earners, young people, women).

[45] The first estimate ignores any value of leisure to older workers (and any insurance or consumption-smoothing benefits of pension income). The second assumes that €1 of earnings is worth €0.50 in public pension income.

[46] Interestingly, the welfare costs are concentrated among the non-working older population, who face a significant income decline if they are not eligible for other forms of social insurance, while workers significantly increase their income and may benefit from a welfare perspective, depending on how they value leisure/work from a utility perspective (Appendix C).

More generally, these calculations highlight the importance of understanding the spillover effects of retirement policies on other agents in the economy. For instance, another effect to consider is the impact on firm profits. Estimates of the MVPF would rise further if profits decline (Appendix C), which could occur if workers are "overpaid" near retirement due to backloaded compensation profiles (Lazear, 1979), downward wage rigidities or strict employment protection. This "overpayment" of older workers may be common in the Dutch setting; wages are often linked directly to job tenure (Mulders, 2018), and firms retain relatively few workers beyond the SRA.

7 Conclusion

We document important spillover effects of retirement timing within firms. We exploit cohort variation in Dutch workers' Statutory Retirement Age (SRA) that was induced by a national reform. The reform raised the SRA by 13 months from 2015 to 2019, and these SRA increases had sharp effects on retirement timing, nearly tripling employment rates at targeted ages. We use high-frequency linked employer-employee data to study how small-to-medium-sized firms respond to these retirement delays. We show that affected firms delay and decrease hiring, which limits opportunities for career progression among job seekers. Coworkers' earnings also decline, with the effects concentrated among workers who miss out on promotions, and changes in hours worked and hourly wages both contribute to these effects.

These spillovers are large and have important distributional and welfare effects. Over the analysis period, our point estimates indicate that firms offset between 66% and 90% of the increase in hours worked through labor demand responses on both the extensive and intensive margins, and we cannot rule out a complete offset. We show that these spillovers reduce earnings among lower-earning workers and exacerbate within-firm earnings disparities by age and gender. They also significantly increase the policy's ratio of welfare costs to fiscal savings.

When considering the implications of these results, we emphasize two caveats. First, since our analysis is at the firm level, our estimates will not capture general equilibrium effects of the reform, such as effects on employment due to changes in consumption behavior. Relatedly, much of the decrease in hiring from affected firms may have reallocated workers across firms rather than caused employment losses. However, our analysis suggests that many of the affected workers may have ended up in worse jobs, since the hiring decline mainly blocks movements either into work or a higher-paying job. This is consistent with causal evidence for the US (Mohnen, forthcoming) and descriptive evidence for many countries (Bianchi and Paradisi, 2022) of the effects of an aging society on youth labor market outcomes. Second, our analysis focuses on private-sector workers in small-to-medium-sized firms. Thus, our estimates are not informative about the overall effects in larger organizations or in the public sector, but our prior is that similar spillovers may exist within elements of these organizations (e.g., teams or plants). Testing this conjecture using detailed data on the internal structure of large organizations is an attractive area for future research.

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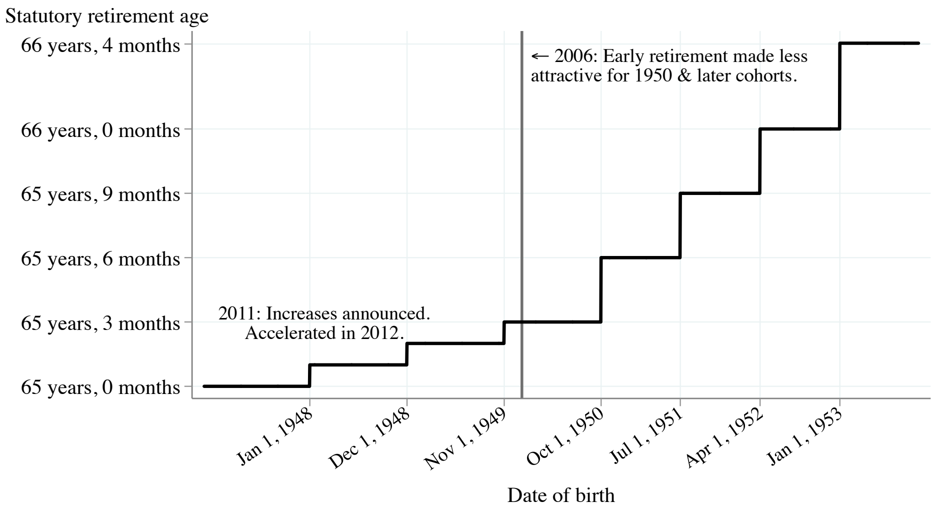
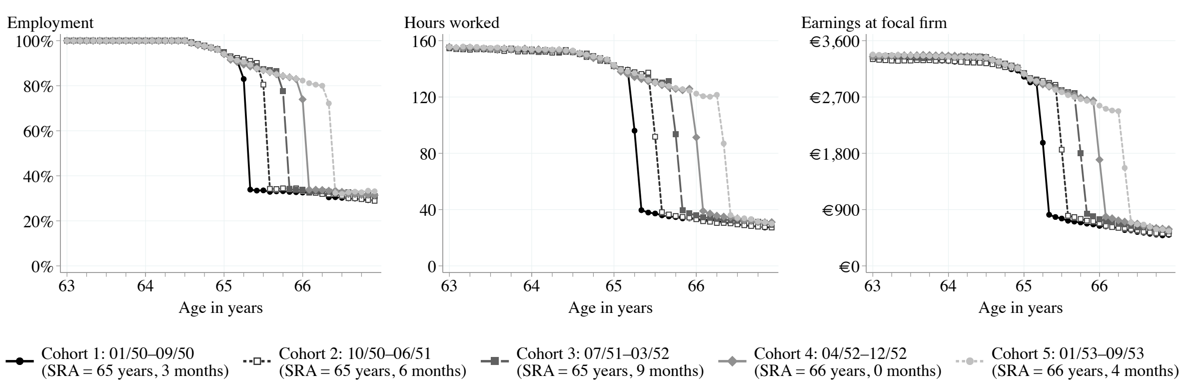
Figure 1: Retirement rules by cohort — Statutory retirement age and early retirement generosity



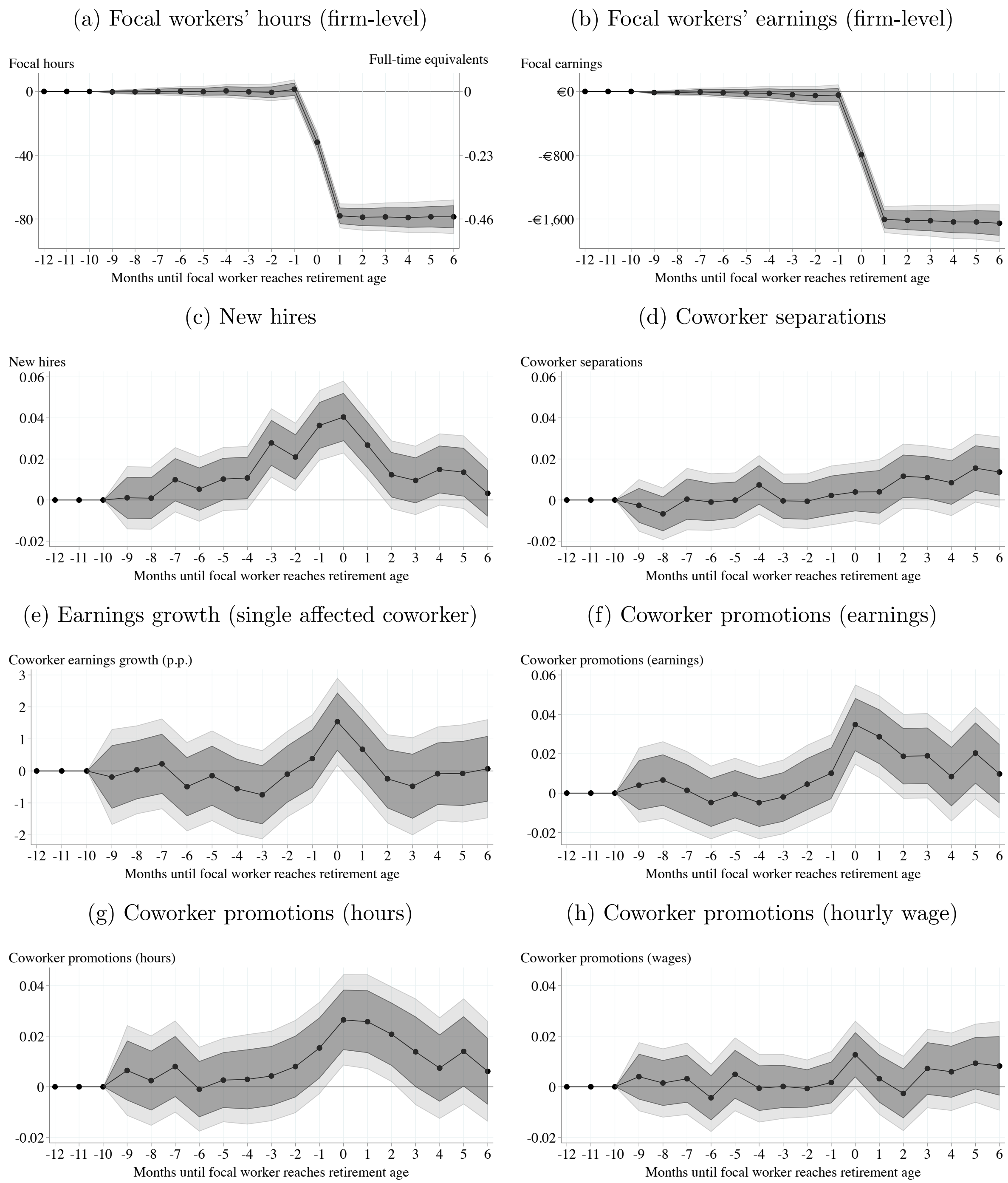
Notes: This figure shows the variation in the Statutory Retirement Age (SRA) in the Netherlands across birth cohorts. Our analysis focuses on increases in the SRA from 65 years and 3 months to 66 years and 4 months. These step-wise increases in the SRA of 3–4 months occurred as a result of reforms in 2011 and 2012. We focus on older workers born between January 1950 and September 1953. This allows us to avoid confounding the effects of the SRA increases with a 2006 reform that reduced the generosity of early retirement programs for individuals born after 1950.

Figure 2: Monthly labor supply trends of focal workers by age and cohort



Notes: These figures show the labor supply trends with age of older workers in the Netherlands in different birth cohorts. We divide individuals born between January 1950 and September 1953 into five nine-month birth cohorts, matching the variation in the Statutory Retirement Age (SRA). We show the labor supply trends of focal workers in each cohort, who are workers with a strong attachment to a particular small-to-medium-sized firm at ages 63 to 64.5 years. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands. See Section 4 for more details on the sample.

Figure 3: Event study around a focal worker's statutory retirement age



(a) Focal workers' hours (firm-level)

(b) Focal workers' earnings (firm-level)

(c) New hires

(d) Coworker separations

(e) Earnings growth (single affected coworker)

(f) Coworker promotions (earnings)

(g) Coworker promotions (hours)

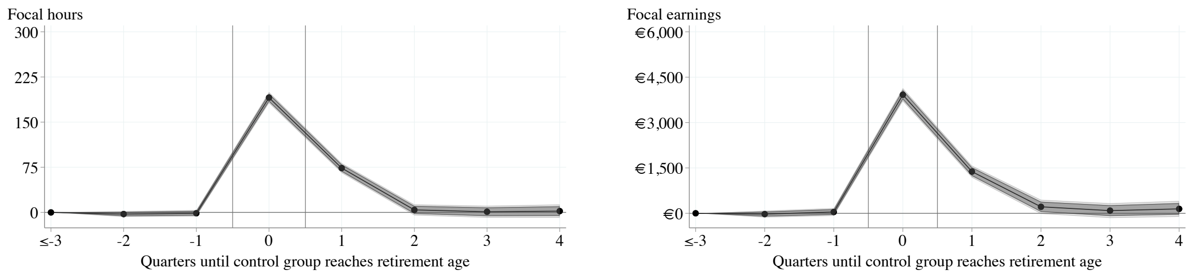
(h) Coworker promotions (hourly wage)

Notes: These figures show the effects on various outcomes of a focal worker's proximity to the SRA. We plot estimates of the γ coefficients from equation (5.1). Panels (a) and (b) augment equation (5.1) with a linear trend-time in event time, which controls for the effect of the focal worker's age on their own outcomes. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The sample is constructed using monthly linked employer-employee register data on the universe of employment spells from Statistics Netherlands.

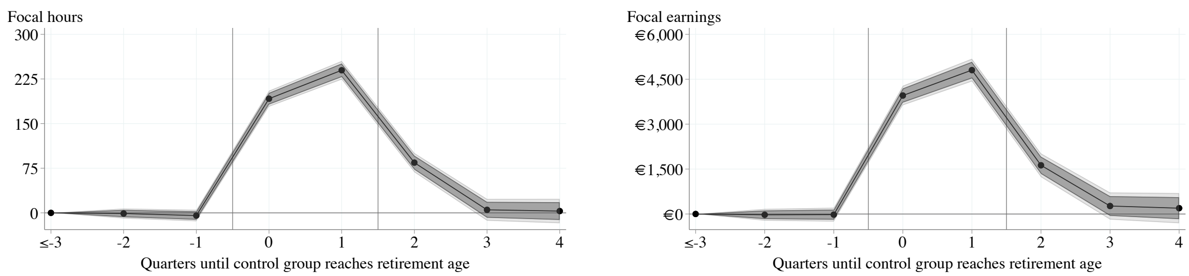
Figure 4: Effects of raising the Statutory Retirement Age on firm-level focal worker outcomes

Hours worked | Earnings

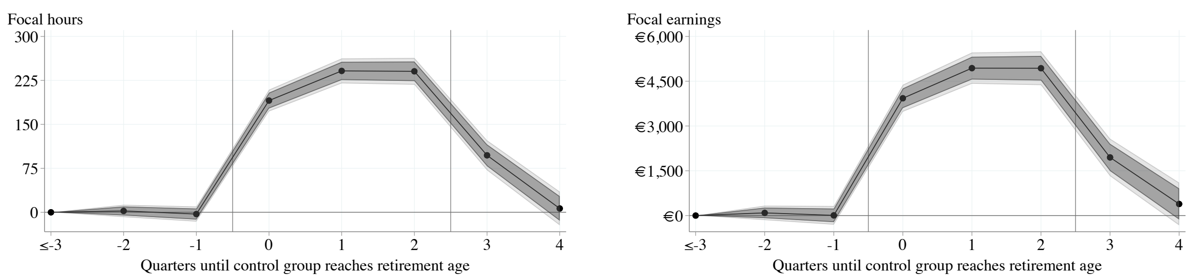
(a) 3 or 4 month treatment



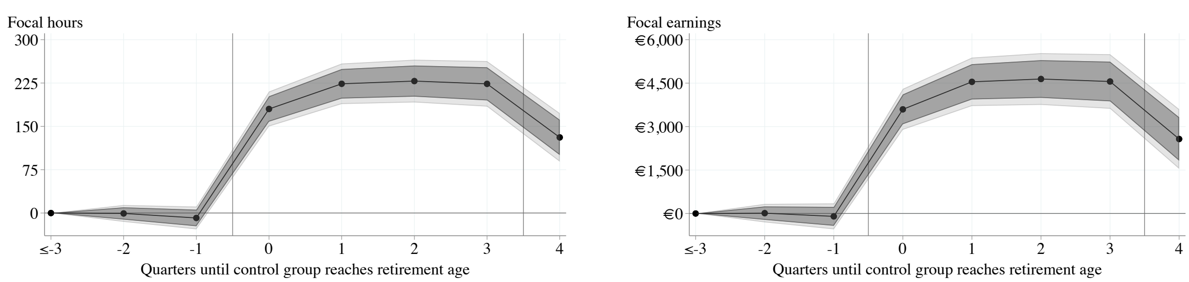
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



(d) 13 month treatment



Notes: These figures plot estimates of the β coefficients from equation (5.3) on the total firm-level hours and earnings of focal workers. Equation (5.3) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm, relative to any difference in the outcomes of treatment and control groups over the reference period. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

Figure 5: Effects of raising the Statutory Retirement Age on hiring and coworker separations

New hires | Coworker separations

(a) 3 or 4 month treatment

(b) 6 or 7 month treatment

(c) 9 or 10 month treatment

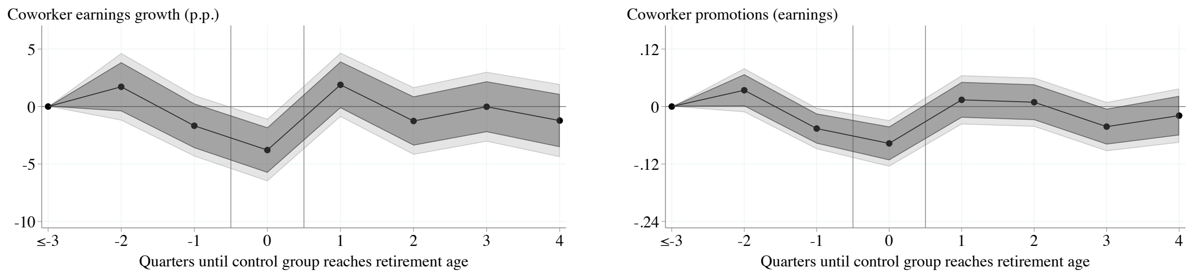
(d) 13 month treatment

Notes: These figures plot estimates of the β coefficients from equation (5.3) on hiring and coworker separations within firms. Equation (5.3) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm, relative to any difference in the outcomes of treatment and control groups over the reference period. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

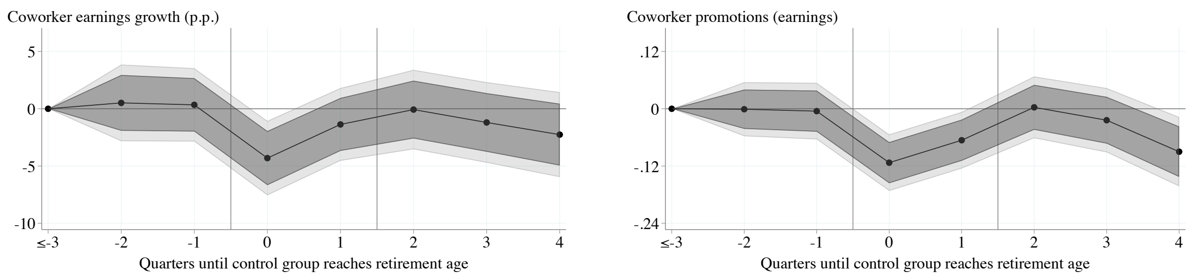
Figure 6: Effects of raising the Statutory Retirement Age on coworkers' career progression

Earnings growth | Promotions

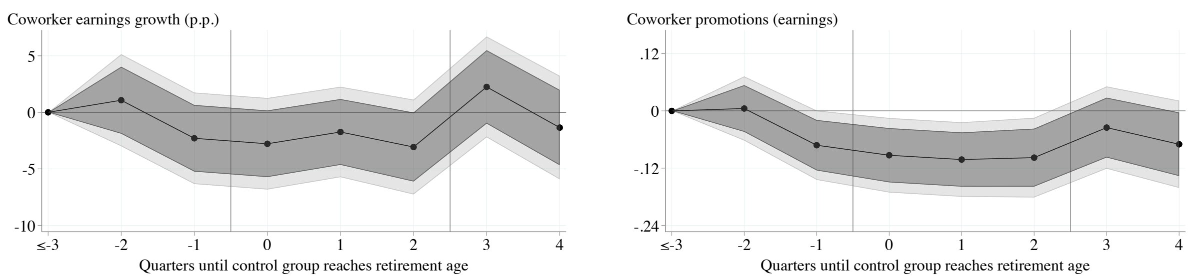
(a) 3 or 4 month treatment



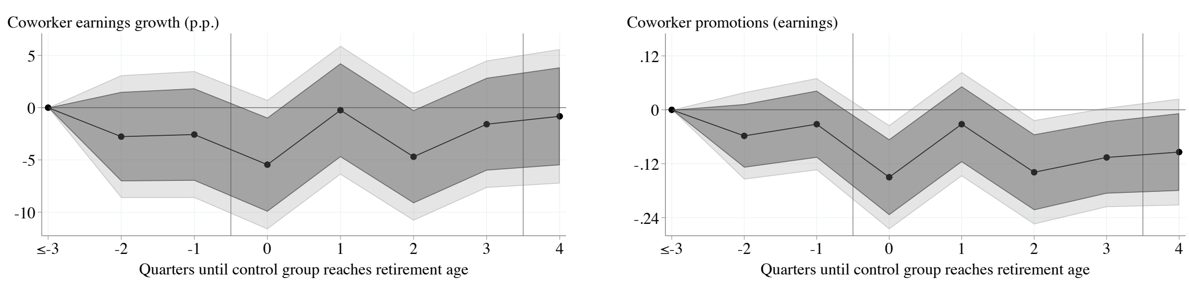
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment

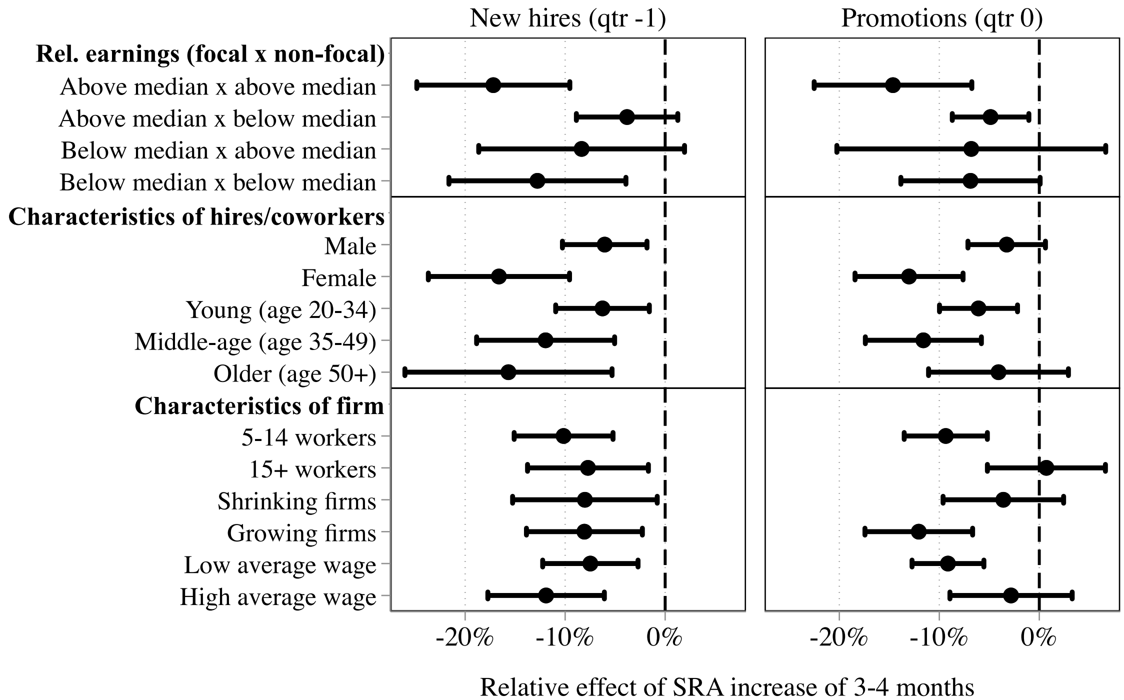


(d) 13 month treatment



Notes: These figures plot estimates of the β coefficients from a version of equation (5.3) on coworkers' average earnings growth and promotions. Equation (5.3) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm, relative to any difference in the outcomes of treatment and control groups over the reference period. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

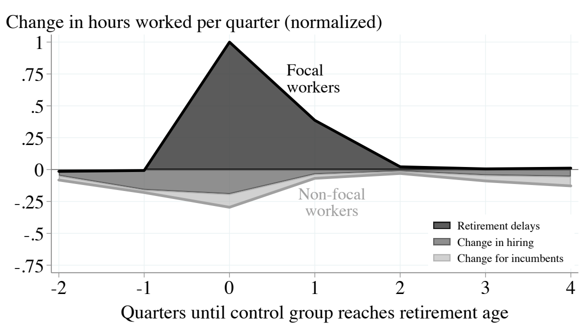
Figure 7: Heterogeneity in the effects on hiring and promotions in the most affected quarter



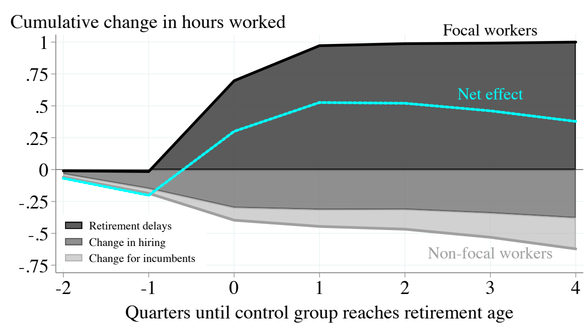
Notes: These figures show estimates from equation (5.3) of a 3–4 month increase in the Statutory Retirement Age (SRA) for a single focal worker, with standard 95% confidence intervals. We present estimates for various subgroups, including different combinations of the earnings of the focal worker and coworkers/hires based on their earnings relative to the median at the firm. We transform the estimates into the implied relative changes in hiring/promotions for each subgroup within a median-sized firm (see Figure A9 for the untransformed estimates, which show the absolute changes in the number of hires/promotions for each group).

Figure 8: Relative changes in hours worked from different sources under 3–4 month treatment

(a) Hours worked per quarter



(b) Cumulative hours worked



Notes: These figures show the implied changes in hours worked for focal and non-focal workers within firms in response to a 3–4 month increase in the SRA for a single focal worker. Panel (a) shows the implied changes in each quarter, with the estimates normalized by the increase in focal workers' hours in quarter 0. Panel (b) shows the cumulative changes, with the estimates normalized by the increase among focal workers by quarter 4. We decompose the change in non-focal hours into changes due to hiring responses and effects on incumbent coworkers.

Table 1: Cumulative effects of retirement delays on firm and coworker outcomes

	(1)	(2)	(3)	(4)
	Length of retirement delay			
	3–4 months	6–7 months	9–10 months	13 months
Panel A: Cumulative change in main outcomes from a single retirement delay				
New hires	-0.126*	-0.175	-0.241	-0.554*
	(0.072)	(0.127)	(0.177)	(0.308)
Coworker separations	-0.020	0.022	-0.046	0.033
	(0.071)	(0.123)	(0.175)	(0.293)
Coworker promotions	-0.292***	-0.593***	-0.934***	-1.340***
	(0.103)	(0.198)	(0.283)	(0.420)
Hours promotions	-0.192*	-0.256	-0.448*	-0.569
	(0.098)	(0.182)	(0.261)	(0.395)
Wage promotions	-0.113	-0.285**	-0.359*	-0.393
	(0.075)	(0.131)	(0.191)	(0.322)
Observations	1,466,233	1,094,089	739,273	367,129
Panel B: Overall substitutability between focal and non-focal workers				
Relative change in non-focal hours	-0.664***	-0.738***	-0.901***	-0.746***
	(0.228)	(0.213)	(0.197)	(0.233)
Relative change in non-focal earnings	-0.313	-0.501**	-0.634***	-0.698***
	(0.258)	(0.245)	(0.210)	(0.239)
Observations	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Standard errors in parentheses are clustered by firm.

Notes: The table shows the estimated cumulative effects from a modified version of equation (5.3), with a single treatment effect for the period from quarter -2 to quarter 4, of the effects of raising the Statutory Retirement Age (SRA). In Panel A, we re-scale the estimates and standard errors to represent the effect of a single retirement delay among a full-time worker within the firm. To do so, we divide by the change in the number of full-time equivalent focal workers over the period of the SRA increase (≈ 0.47). See Table A10 for the untransformed estimates. In Panel B, we present the estimated effects on the total hours worked and earnings of non-focal workers. The estimates are re-scaled into relative changes by dividing by the estimated increase in focal hours/earnings. See Table A10 for the untransformed estimates.

Table 2: Distributional implications of a six-month retirement delay within the firm

	(1) Top 50%	(2) Bottom 50%	(3) Old	(4) Young	(5) Men	(6) Women
Estimated change in total earnings (€)	17,248	-4,021	21,091	-6,046	15,131	-1,904
Change in between-group earnings ratio	2.7%		5.6%		4.0%	
Mechanical component	50%		58%		53%	
Share explained by spillovers	50%		42%		47%	

Notes: This table shows the estimated net effects on the total earnings of various groups (in 2019 €) within affected firms in response to a single retirement delay of six months. The estimates are based on estimates of equation (5.3) on the number of workers of different types in each ventile of the population earnings distribution. See Section 6.2 and Appendix B for more details.

Web Appendix for "Longer careers: A barrier to hiring and coworker advancement?"

Irene Ferrari Jan Kabátek Todd Morris

Contents

List of Figures

List of Tables

A Employment effects of SRA increases below the existing SRA

In Figure A1, we present regression discontinuity estimates and 95% confidence intervals of the effects of each of the four SRA increments on labor supply in 2011–18. Our outcome variable is the number of months in each year that the worker worked at least 20 hours per week and our sample consists of (i) individuals who worked more than 20 hours per week in every month of 2010, the year before the reform was announced, and (ii) those born within nine months of the given SRA-threshold. The bias-corrected estimates — using a uniform kernel and a 9 month bandwidth — are nearly all statistically indistinguishable from zero before the policies take effect. In the key year for each treatment (denoted by the vertical lines on the figures), the estimates are positive and highly statistically significant, implying that treated individuals work approximately one additional month on average.

Figure A1: Regression discontinuity estimates of the employment effects of each SRA increase

(a) 65 years, 3 months ⇒ 65 years, 6 months

(b) 65 years, 6 months ⇒ 65 years, 9 months

(c) 65 years, 9 months ⇒ 66 years, 0 months

(d) 66 years, 0 months ⇒ 66 years, 4 months

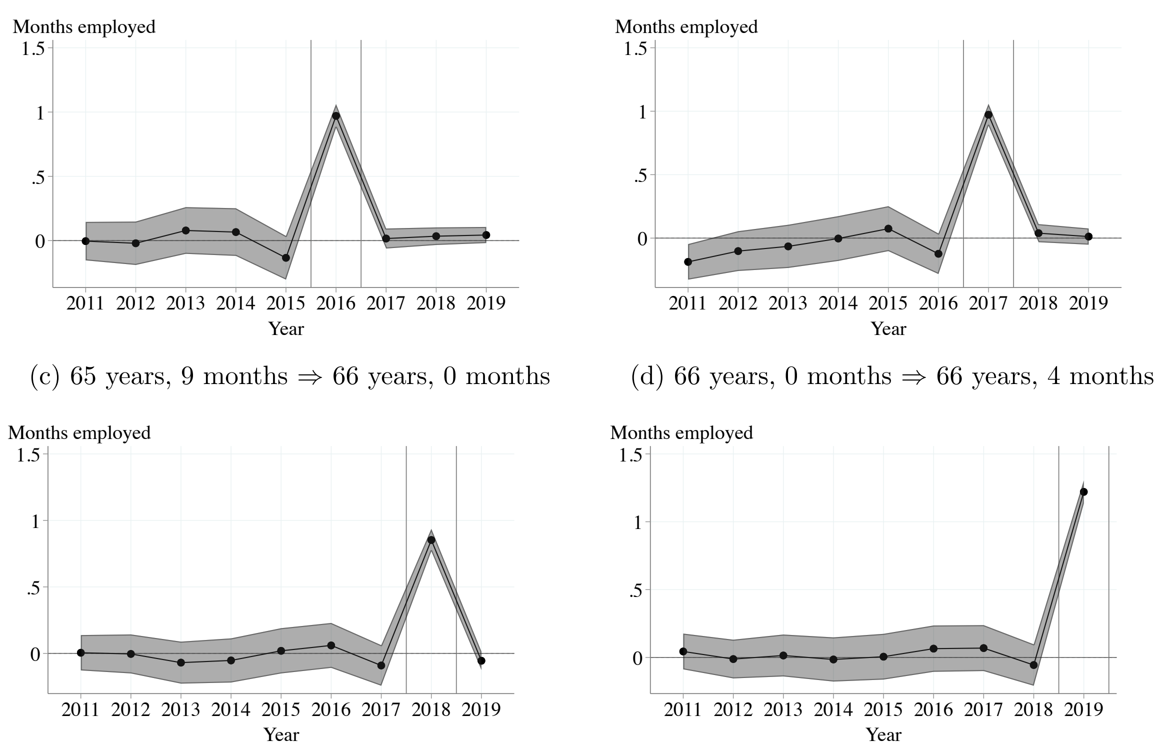
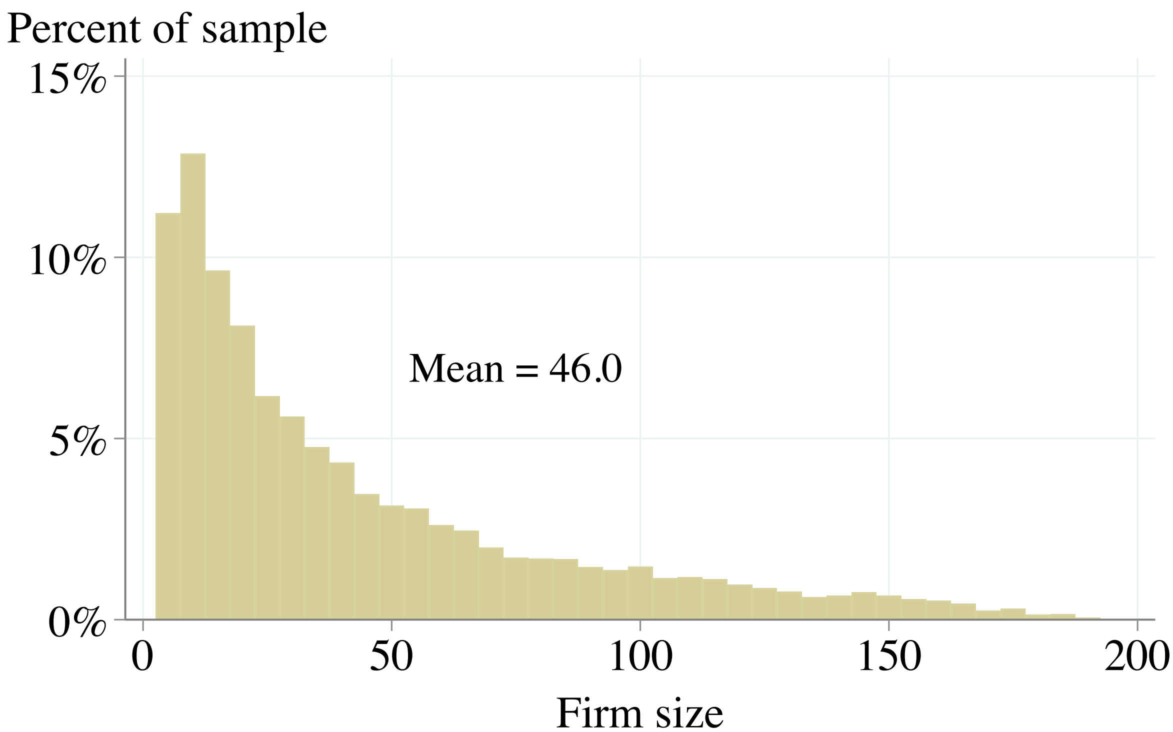


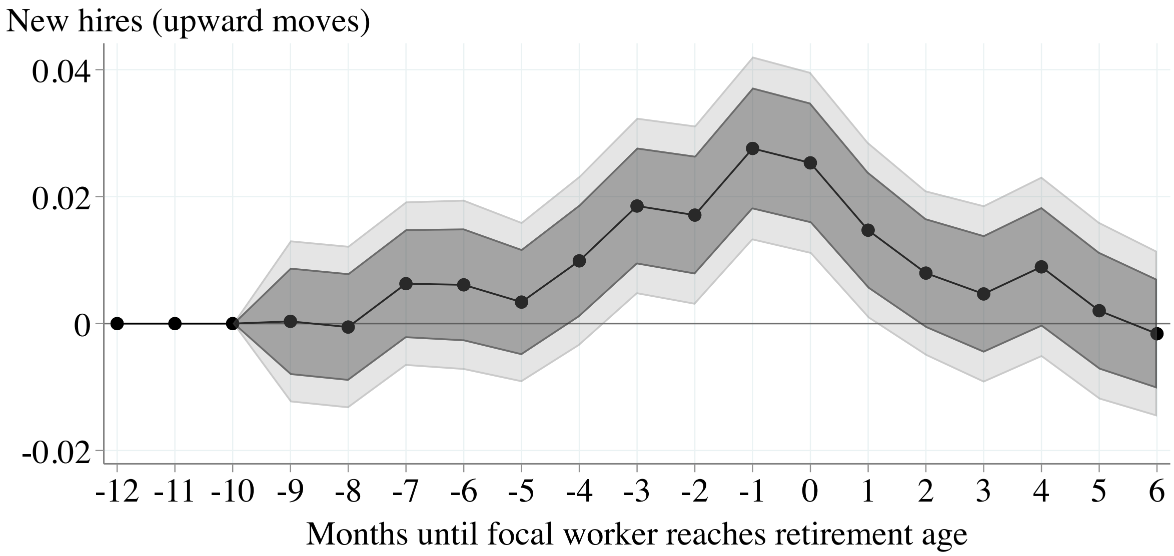
Figure A2: Distribution of the size of focal workers' firms



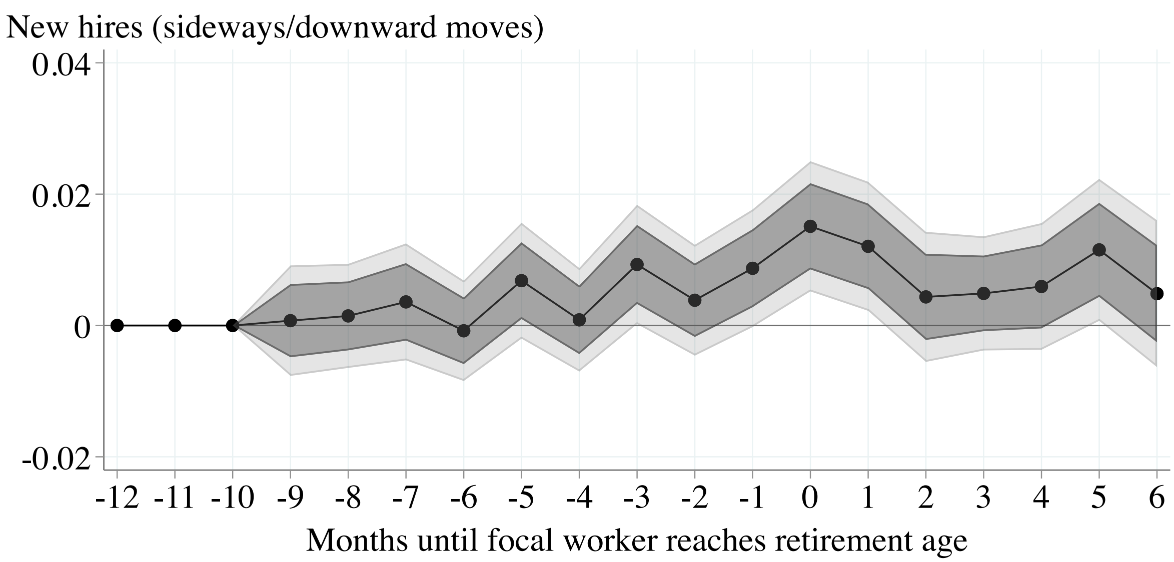
Notes: This figure shows the distribution of firm size among the sample of focal workers. The unit of observation is the focal worker, as in the regressions. On average, focal workers work in firms with 46 workers (the median is 31). The average firm is smaller (mean of 33 and median of 21), since larger firms are more likely to have multiple focal workers. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A3: Event study around a focal worker's statutory retirement age: Hiring patterns

(a) Upward moving new hires



(b) Sideways/downward moving new hires

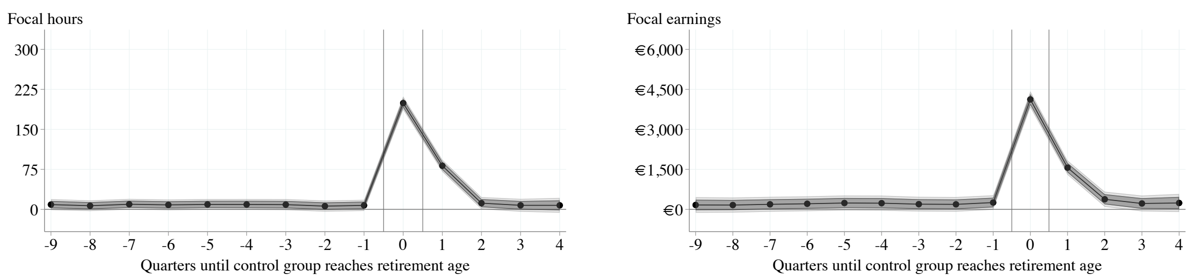


Notes: These figures plot estimates of the γ coefficients from equation (5.1). The estimates show the effects on the hiring of various groups of a focal worker's proximity to the SRA. We define new hires as "upward movers" if they have a 10% increase in their monthly earnings upon being hired, which includes individuals moving into employment. The remainder of new hires are defined as "sideways/downward movers". We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The sample is constructed using monthly linked employer-employee register data on the universe of employment spells from Statistics Netherlands.

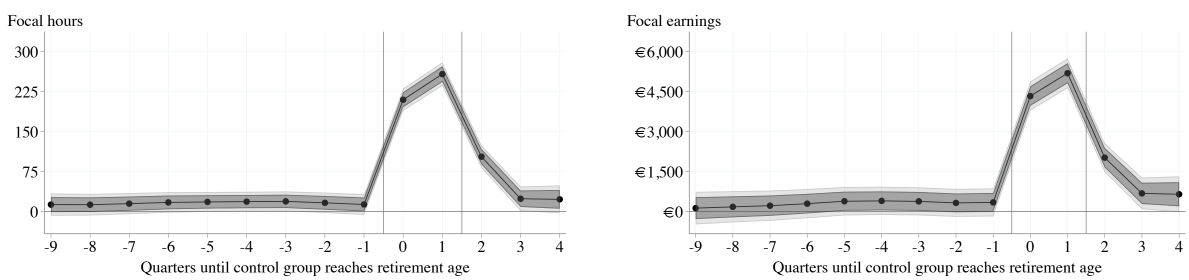
Figure A4: Effects of raising the SRA on focal workers' outcomes (no ref. period)

Hours worked | Earnings

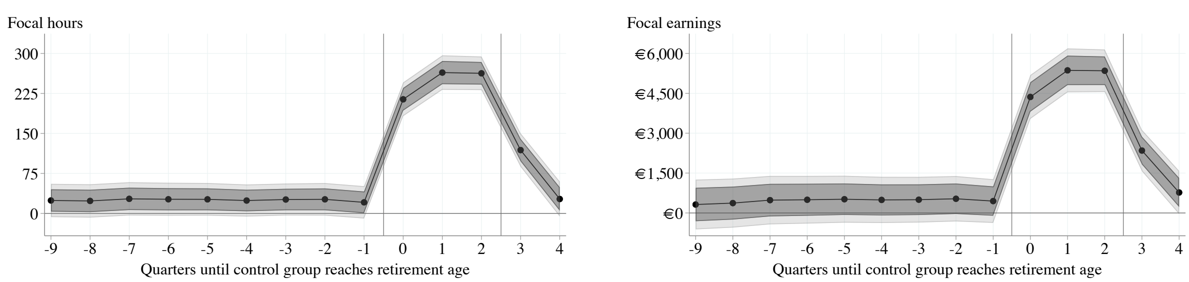
(a) 3 or 4 month treatment



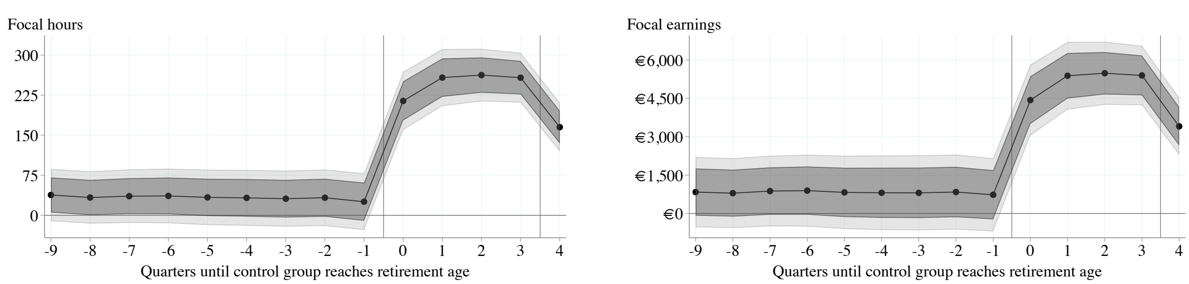
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



(d) 13 month treatment

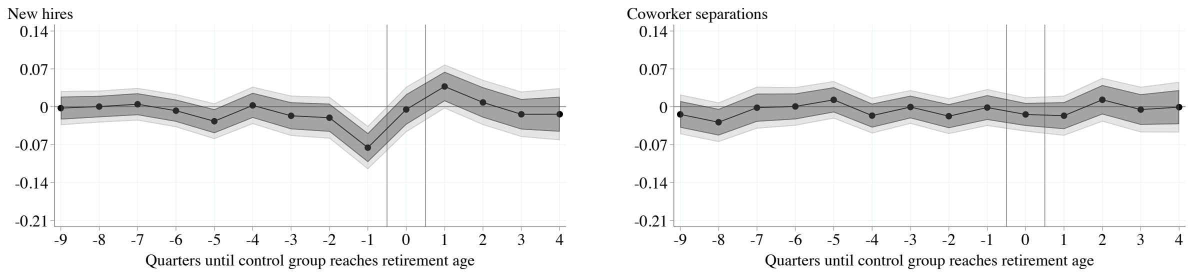


Notes: These figures plot estimates of the β coefficients from equation (5.2) on the total firm-level hours and earnings of focal workers. Equation (5.2) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm over a long period, allowing us to assess the parallel-trends assumption before the SRA. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

Figure A5: Effects of raising the SRA on hiring and coworker separations (no ref. period)

<u>New hires</u> <u>Coworker separations</u>

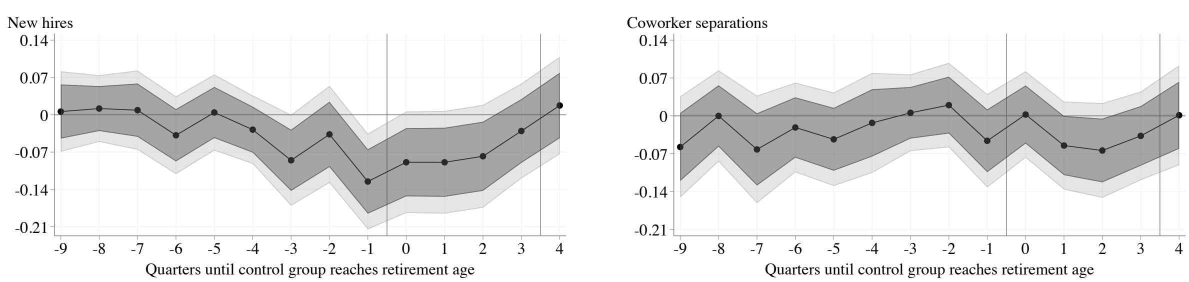
(a) 3 or 4 month treatment



(b) 6 or 7 month treatment

(c) 9 or 10 month treatment

(d) 13 month treatment

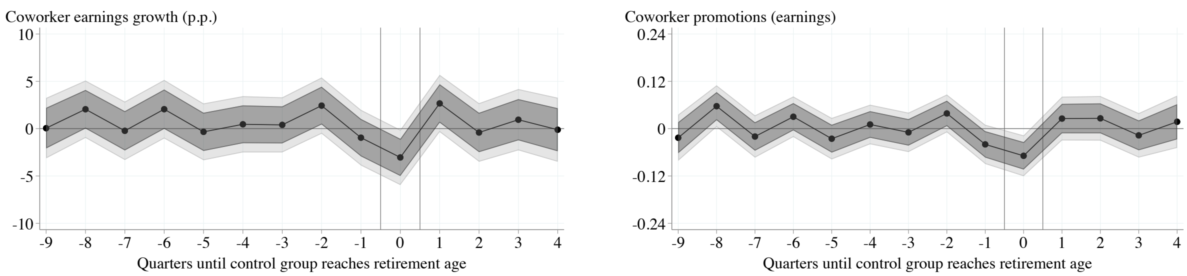


<u>Notes</u>: These figures plot estimates of the β coefficients from equation (5.2) on hiring and coworker separations within firms. Equation (5.2) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm over a long period, allowing us to assess the parallel-trends assumption before the SRA. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

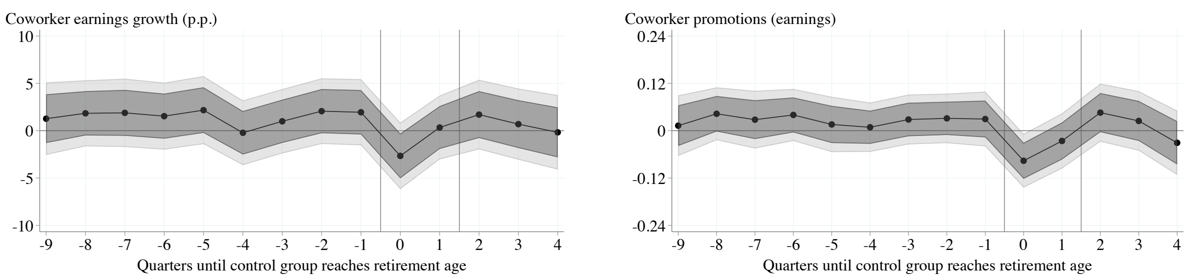
Figure A6: Effects of raising the SRA on coworkers' career progression (no ref. period)

Earnings growth | Promotions

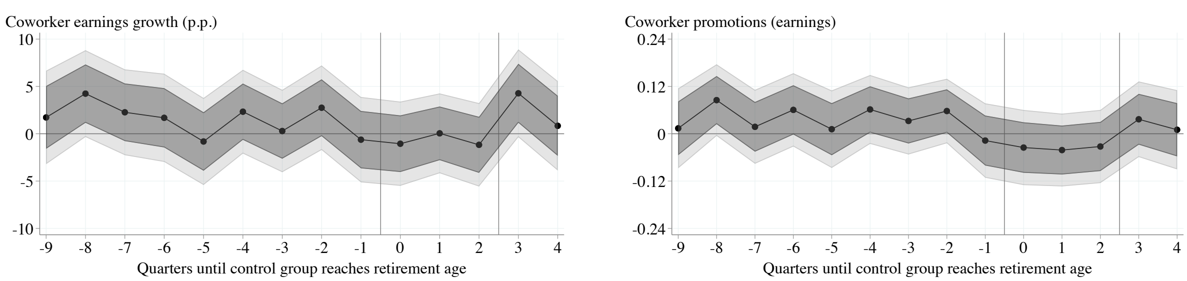
(a) 3 or 4 month treatment



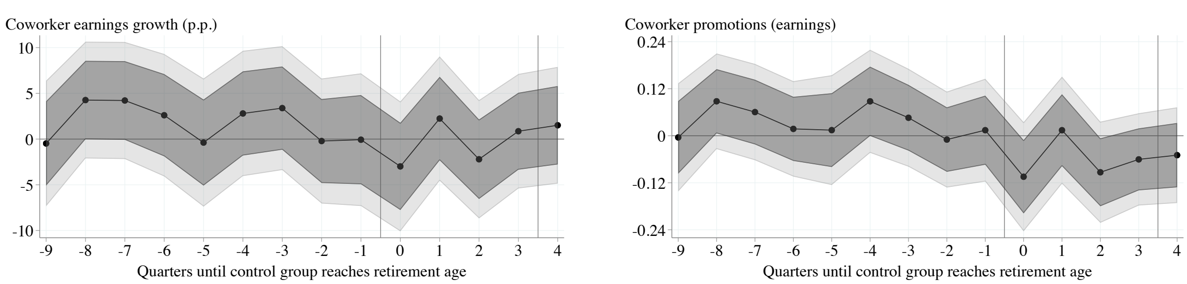
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



(d) 13 month treatment

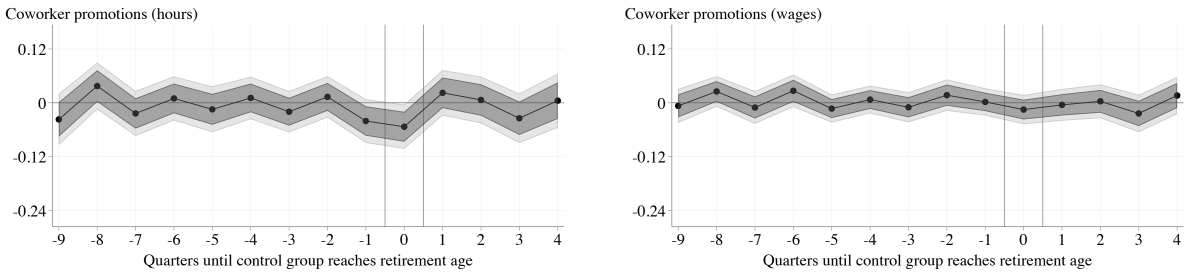


Notes: These figures plot estimates of the β coefficients from equation (5.2) on coworkers' average earnings growth and promotions. Equation (5.2) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm over a long period, allowing us to assess the parallel-trends assumption before the SRA. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

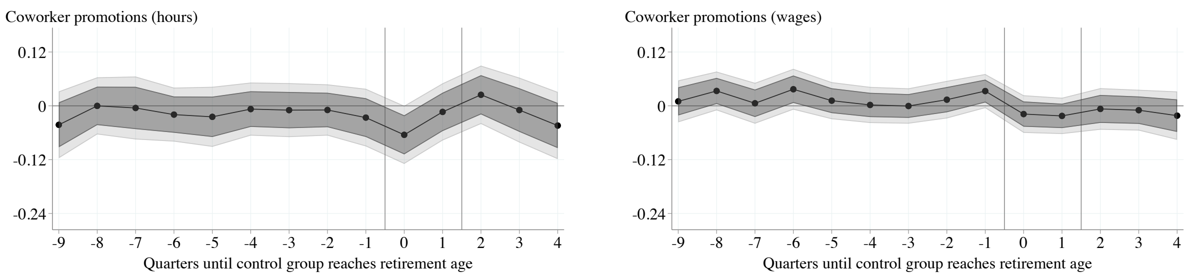
Figure A7: Assessing pre-trends in hours and wage "promotions"

Hours worked — Hourly wages

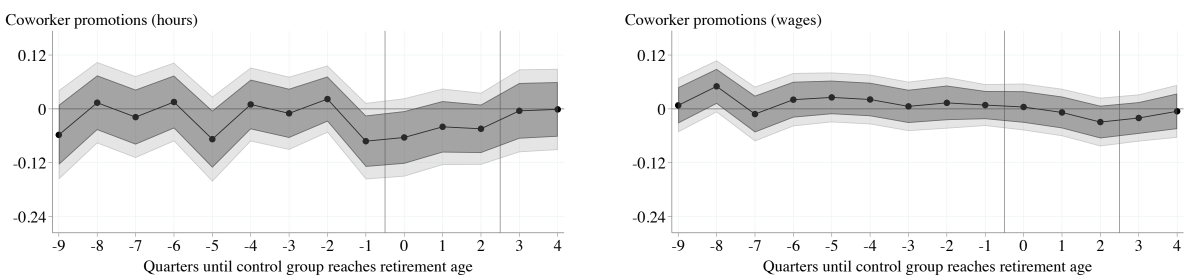
(a) 3 or 4 month treatment



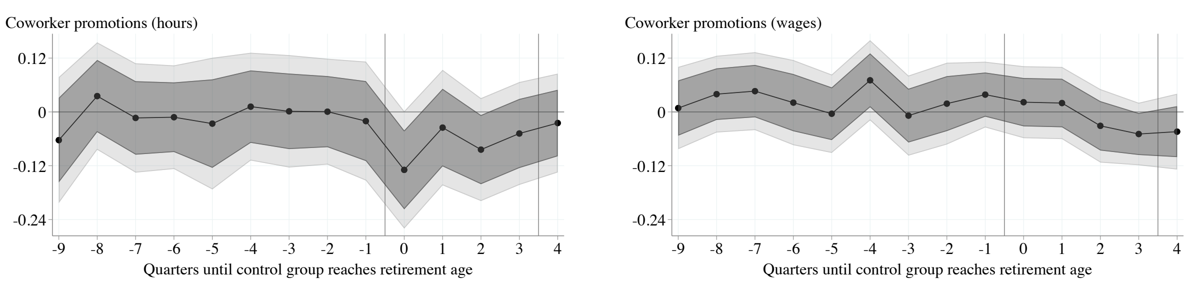
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



(d) 13 month treatment

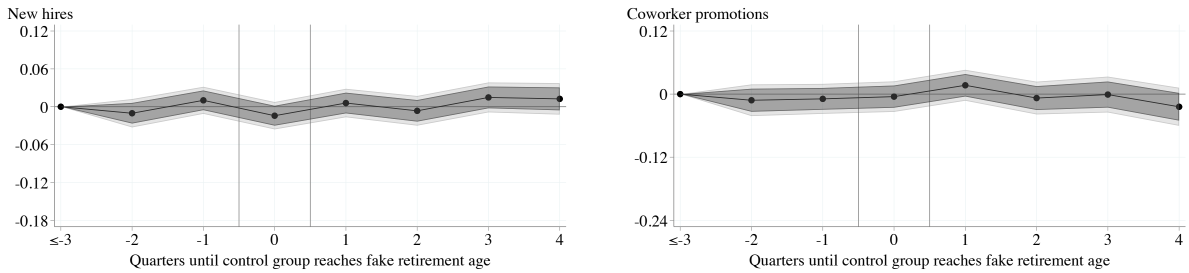


Notes: These figures plot estimates of the β coefficients from equation (5.2) for coworkers' hours and wage 'promotions' (sustained increases of at least 10% in monthly contractual hours/wages). The estimates show the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm over a long period, allowing us to assess the parallel-trends assumption. We present two sets of 95% confidence intervals (CIs): standard CIs (dark gray) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light gray). The vertical lines show the main quarters in which retirement is delayed for focal workers. The sample is constructed using monthly linked employer-employee data from Statistics Netherlands.

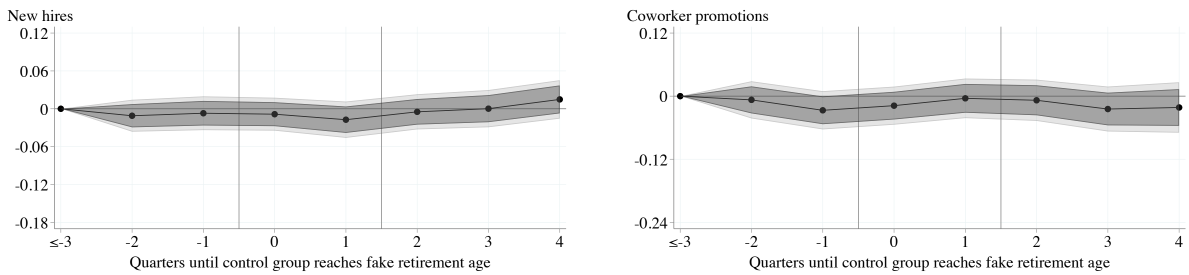
Figure A8: Placebo regression estimates of the effects of raising a fake retirement threshold

New hires | Coworker promotions

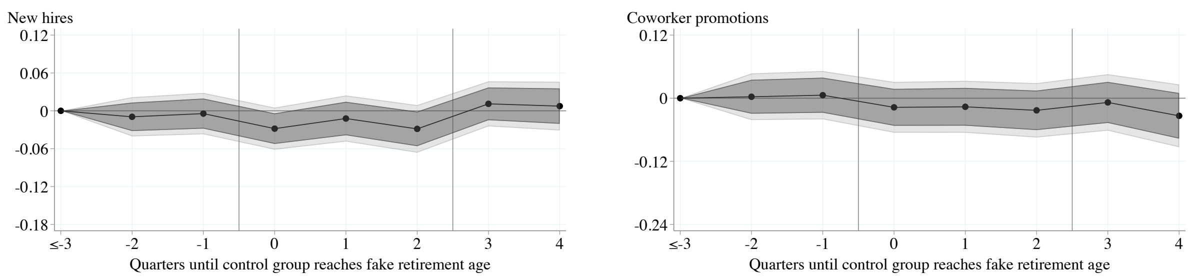
(a) 3 or 4 month treatment



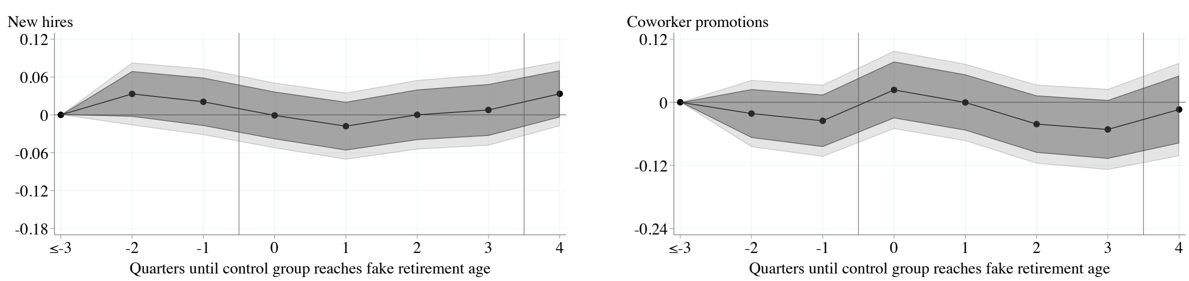
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



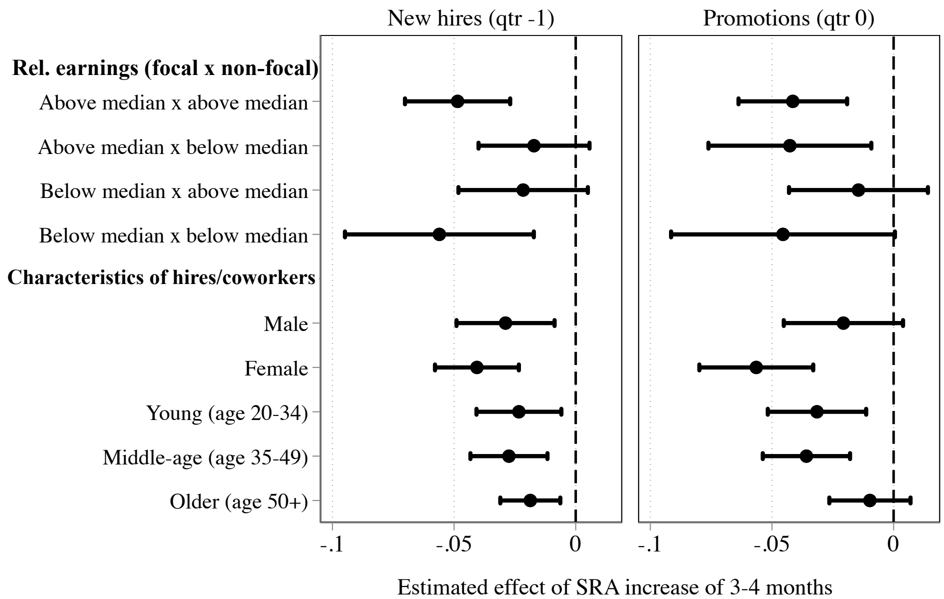
(d) 13 month treatment



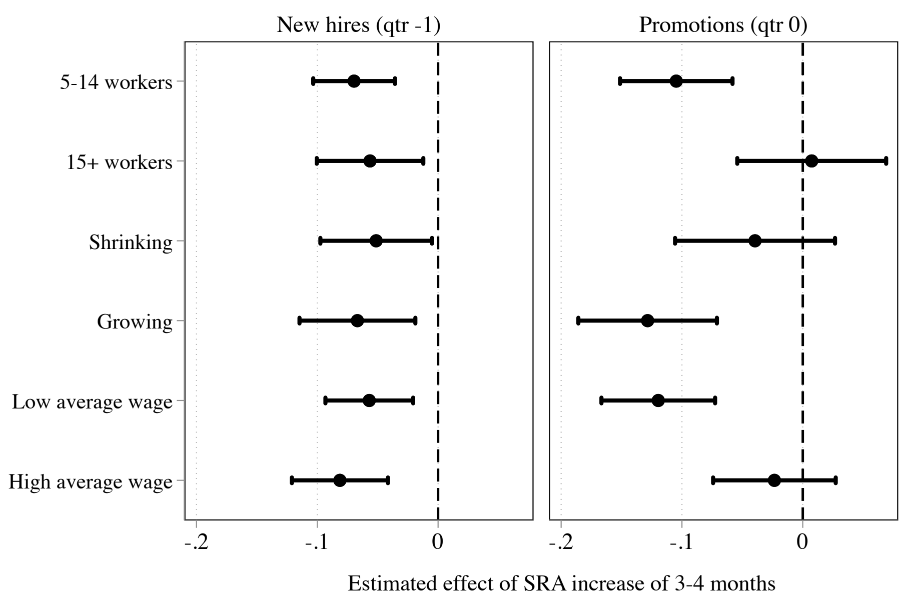
Notes: These figures plot placebo regression estimates and 95% confidence intervals of the β coefficients from equation (5.3) on hiring and coworker promotions. We define a sample of focal workers who are born in 1960–63 — ten years later than our main sample — and assume they reach a 'fake Statutory Retirement Age' threshold in the same calendar month as someone who is exactly ten years older. The estimates show the effects of an increase in the 'fake SRA' in event time for the treatment group, relative to any difference in the outcomes of treatment and control groups over the reference period. The vertical lines show the main quarters in which retirement would have been delayed for focal workers if they responded to the fake SRA threshold.

Figure A9: Heterogeneity in the effects on hiring and promotions (untransformed estimates)

(a) Worker characteristics



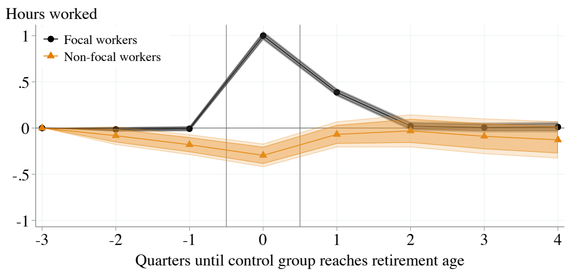
(b) Firm characteristics



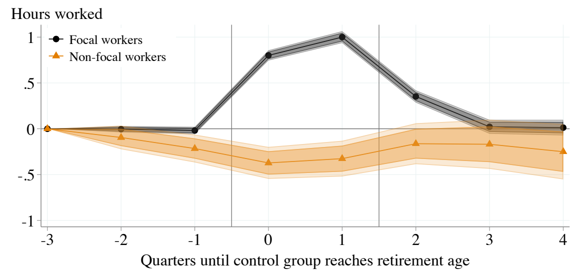
Notes: These figures plot the untransformed estimates of the β coefficients from equation (5.3) with standard 95% confidence intervals (the relative impacts implied by these estimates are shown in Figure 7). The estimates show the effects of a 3–4 month increase in the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the number of hires/promotions within different groups. We show the effects for different combinations of the earnings of the focal worker and coworkers/new hires, dividing both sets of workers into above- and below-median earners based on the median earnings at the firm. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

Figure A10: Effects of raising the SRA on hours worked by focal and non-focal workers

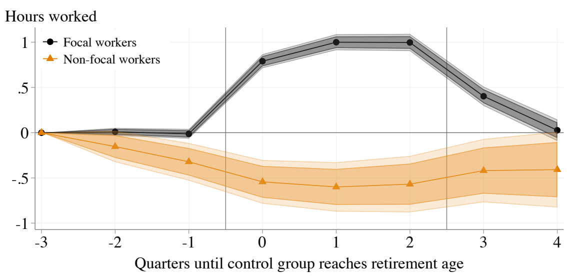
(a) 3 or 4 month treatment



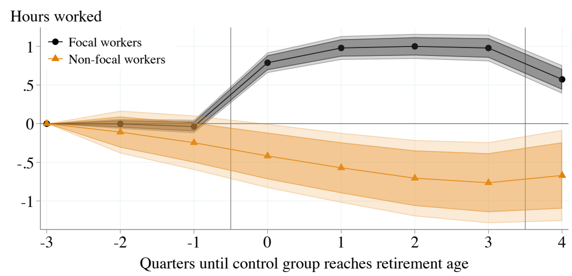
(b) 6 or 7 month treatment



(c) 9 or 10 month treatment



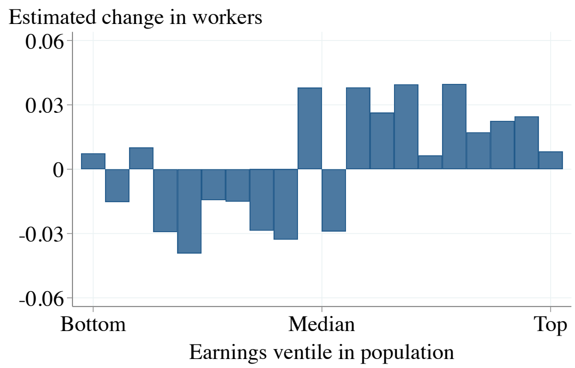
(d) 13 month treatment



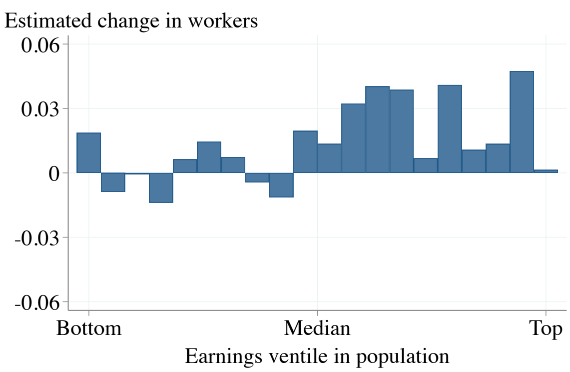
Notes: These figures plot estimates of the β coefficients from equation (5.3) on the total firm-level hours worked by focal workers and non-focal workers. We normalize the estimates by dividing by the estimated increase in hours worked among focal workers in the most affected quarter (e.g., quarter 0 in panel a). Equation (5.3) estimates the treatment effects of raising the Statutory Retirement Age (SRA) for a single focal worker within the firm, relative to any difference in the outcomes of treatment and control groups over the reference period. We present two sets of 95% confidence intervals (CIs): standard CIs (dark) and CIs that account for the multiple hypothesis tests in each regression via a Bonferroni correction (light). The vertical lines show the main quarters in which retirement is delayed for treated focal workers. The sample is constructed using monthly linked employer-employee register data from Statistics Netherlands.

Figure A11: Estimated cumulative effects of a six-month retirement delay on the number of workers in affected firms by earnings ventile and group

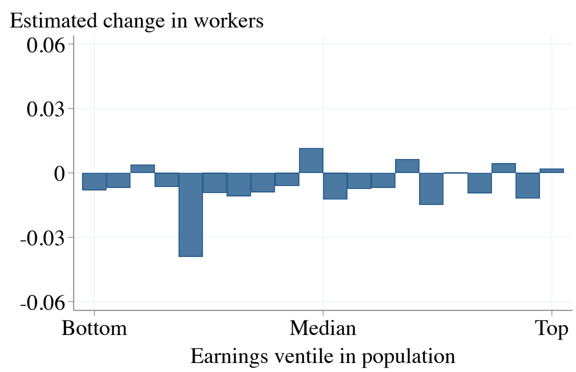
(a) All workers



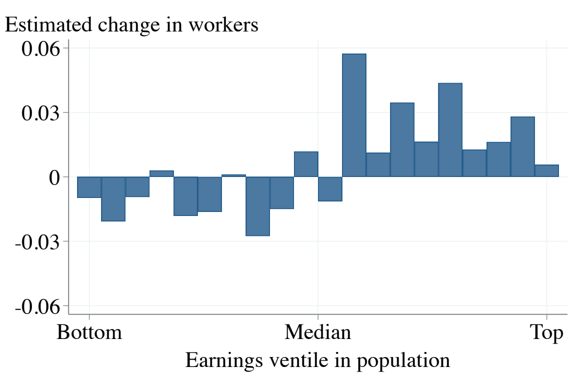
(b) Older workers (age 50+)



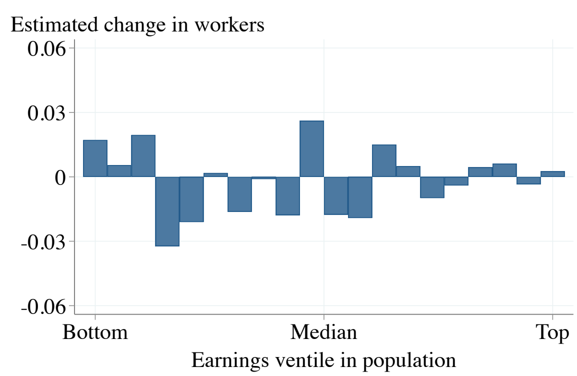
(c) Young workers (age 20–34)



(d) Male workers



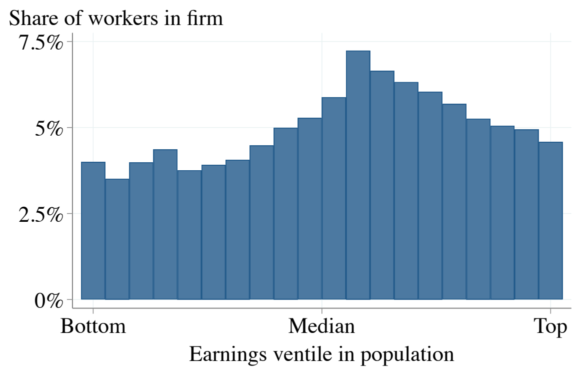
(e) Female workers



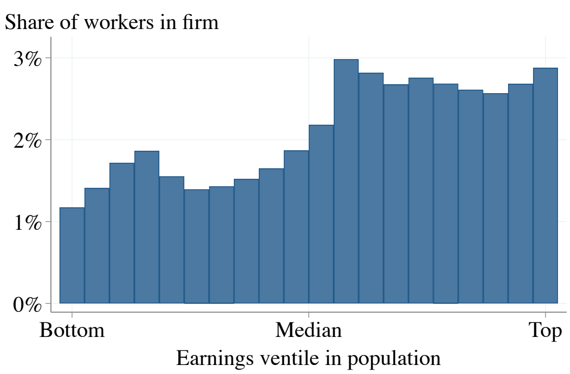
Notes: These figures show the estimated effects of a single six-month retirement delay on the number of workers in affected firms in different ventiles of the population earnings distribution. The estimates effects are based on estimates of equation (5.3) for the 6–7 month treatment. (Other treatments show qualitatively similar effects, with the magnitudes growing with the length of the retirement delay.) We use the point estimates to calculate the average change in the number of workers over the 21-month period around the older worker's Statutory Retirement Age (SRA). See Section 6.2 and Appendix B for more details.

Figure A12: Distribution of the share of workers in different population earnings ventiles by type of worker

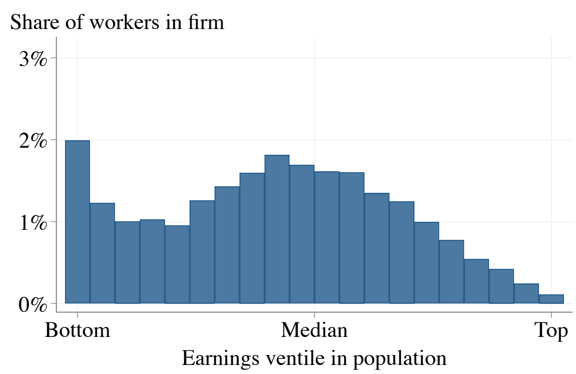
(a) All workers



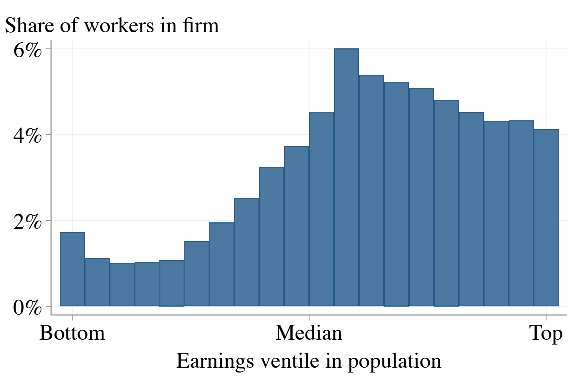
(b) Older workers (age 50+)



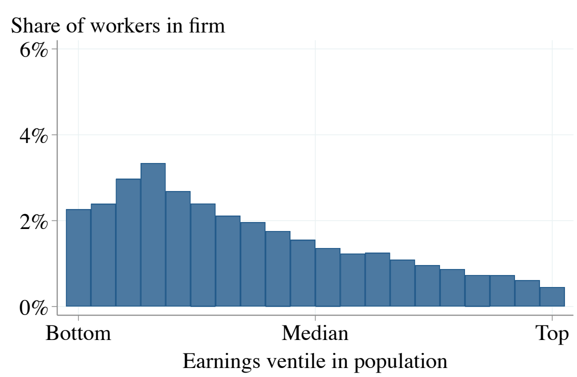
(c) Young workers (age 20–34)



(d) Male workers



(e) Female workers



Notes: These figures show how workers in different parts of the population earnings distribution are distributed within firms. On average, affected firms employ slightly more workers in the top half of the earnings distribution. High earners are much more likely to be an older worker (age 50+) than a young worker (age 20–34), and much more likely to be a man than a woman.

Table A1: Impact of sample restrictions on sample size

Sample/restriction	Focal workers	Firms
Working population at age 64 years and 6 months (cohorts January 1950 – September 1953)	248,003	62,289
Restricting to private sector	138,768	56,321
Restrictions on their firms		
5–200 workers in business ID at ages 63-66.99	53,088	27,311
Excluding firms with multiple establishments	39,961	21,629
Excluding firms with extreme month-to-month changes in workforce size	33,885	19,746
Excluding firms ever in public/subsidized sectors	29,606	18,070
Restrictions on focal workers		
Requiring consistent employment at same firm at ages 63–64.5	26,578	16,440
Requiring work hours of at least 20 per week at ages 63–64.5	21,646	13,557
Excluding directors/major shareholders	19,505	12,159

Notes: This table shows how the various sample restrictions affect the number of focal workers and focal firms in the sample.

Table A2: Descriptive statistics on focal workers and their firms/coworkers

	Mean	Std. Dev.
Focal worker characteristics		
Age in years	64.5	0
Employed	100%	0
Monthly contractual work hours	152	31
Monthly contractual earnings	€3,306	€1,652
Contractual hourly wage	€21.6	€9.9
Share male	79.4%	
Firm/coworker characteristics		
Number of workers	46.0	40.7
Young workers (age 20–34)	11.2	12.1
Middle-age workers (age 35–49)	16.9	17.0
Older workers (age 50+)	17.8	17.2
Focal workers	2.3	1.9
Total monthly contractual wage costs	€144,340	€150,069
No. of coworker separations per month per 100 workers	1.07	2.45
No. of new hires per month per 100 workers	1.05	2.42
Average p.p. coworker earnings increase	0.87	4.15
Average p.p. coworker hours increase	0.85	5.73
Average p.p. coworker wage increase	0.46	4.21
No. of coworkers with 10% earnings increases per 100 workers	1.69	4.07
No. of coworkers with 10% hours increases per 100 workers	1.38	3.95
No. of coworkers with 10% wage increases per 100 workers	0.96	3.17
Percent change in combined earnings of coworkers	0.13	4.53
Percent change in combined hours of coworkers	0.11	6.44
Mean earnings of coworkers in $t-1$	€3,003	951
Mean hours of coworkers in $t-1$	148	24
Combined earnings of stable coworkers in $t-1$	€127,357	€136,945
Combined hours of stable coworkers in $t-1$	6,032	5,915
Individuals (focal workers)	19,505	
Firms	12,159	

Notes: This table summarizes the characteristics of focal workers and their firms in the month when the focal worker is aged 64.5 years old. For all statistics, the unit of observation is the focal worker. All incomes are in 2019 €. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A3: Assessing balance across adjacent cohorts at ages 63 to 64.5

	Difference: Treatment *minus* control	Control mean
Focal worker labor supply		
Focal worker hours per 10 workers	4.1	119.2
	(2.5)	
Focal worker earnings per 10 workers	65	2,587
	(62)	
Firm size, labor costs and job flows		
Number of workers	1.9	44.5
	(1.0)	
Total labor costs per month	7,191	138,145
	(3,682)	
Monthly hires per 10 workers	0.003	0.096
	(0.002)	
Monthly coworker separations per 10 workers	0.001	0.105
	(0.003)	
Monthly growth in coworkers' earnings, hours and wages		
Average earnings growth (p.p.)	-0.003	0.878
	(0.032)	
Average hours growth (p.p.)	0.034	0.879
	(0.035)	
Average wage growth (p.p.)	-0.013	0.477
	(0.014)	
Coworker promotions: Sustained 10% increases per 10 workers		
Earnings	-0.003	0.172
	(0.006)	
Hours	-0.002	0.144
	(0.006)	
Wages	-0.001	0.098
	(0.003)	
Percent change in combined coworker earnings/hours		
Earnings	0.026	0.129
	(0.031)	
Hours	0.014	0.111
	(0.020)	
Observations	588,126	

Notes: This table compares the characteristics of focal workers and their firms in adjacent treatment and control cohorts when focal workers are aged 63 to 64.5 years old. For each outcome, we estimate a stacked regression with a single treatment dummy, cohort-pair fixed effects and other controls (age-in-month and month-year fixed effects). Standard errors in parentheses for the treatment dummy are clustered by firm. The estimates show no statistically significant differences between treated and control workers in adjacent cohorts if we account for multiple hypothesis testing. The estimates for focal work hours, number of workers and total labor costs are significant at the 10% level without this adjustment. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A4: Estimated effects of raising the Statutory Retirement Age (SRA) on the separation rates of coworkers with secure and insecure work contracts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Separations: Secure contracts				Separations: Insecure contracts			
	Treatment: SRA increase (months)				Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13	3 or 4	6 or 7	9 or 10	13
Quarter								
-2	-0.013	-0.004	0.008	0.031	0.002	0.006	-0.002	0.001
	(0.011)	(0.013)	(0.015)	(0.022)	(0.005)	(0.006)	(0.008)	(0.011)
-1	0.001	-0.017	-0.014	-0.017	0.002	0.002	0.005	-0.009
	(0.010)	(0.013)	(0.016)	(0.025)	(0.005)	(0.005)	(0.008)	(0.012)
0	-0.002	0.024	0.007	0.021	-0.007	-0.008	-0.014	0.008
	(0.010)	(0.013)	(0.016)	(0.024)	(0.005)	(0.006)	(0.008)	(0.012)
1	-0.019	-0.028	-0.023	-0.029	0.010	0.009	0.002	-0.002
	(0.012)	(0.014)	(0.018)	(0.027)	(0.005)	(0.005)	(0.007)	(0.010)
2	0.019	0.004	-0.023	-0.033	0.003	0.003	-0.004	-0.004
	(0.013)	(0.015)	(0.017)	(0.028)	(0.005)	(0.006)	(0.008)	(0.013)
3	-0.000	0.020	0.014	-0.007	-0.002	0.007	-0.004	-0.006
	(0.013)	(0.017)	(0.020)	(0.027)	(0.005)	(0.007)	(0.008)	(0.012)
4	0.011	0.019	0.026	0.026	-0.005	-0.004	0.011	0.001
	(0.015)	(0.017)	(0.023)	(0.031)	(0.006)	(0.007)	(0.010)	(0.013)
R-squared	0.007	0.007	0.007	0.008	0.004	0.004	0.004	0.004
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the separation rates of coworkers. We construct separate rates for coworkers who have secure job contracts, who are hard to dismiss, and those who have insecure job contracts. The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

Table A5: Effects of raising the Statutory Retirement Age (SRA) on coworkers' opportunities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coworker earnings growth (p.p.)				Coworker promotions			
	Treatment: SRA increase (months)				Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13	3 or 4	6 or 7	9 or 10	13
Quarter								
-2	1.719	0.513	1.071	-2.772	0.034	-0.001	0.005	-0.058
	(1.089)	(1.242)	(1.512)	(2.178)	(0.017)	(0.021)	(0.025)	(0.036)
-1	-1.674	0.342	-2.295	-2.574	-0.046**	-0.005	-0.072*	-0.032
	(0.990)	(1.188)	(1.503)	(2.250)	(0.016)	(0.022)	(0.027)	(0.038)
0	-3.789***	-4.311***	-2.781	-5.454	-0.077***	-0.113***	-0.093**	-0.150***
	(1.008)	(1.206)	(1.503)	(2.295)	(0.018)	(0.022)	(0.029)	(0.043)
1	1.890	-1.368	-1.737	-0.243	0.014	-0.066**	-0.102***	-0.032
	(1.035)	(1.179)	(1.485)	(2.286)	(0.019)	(0.022)	(0.029)	(0.043)
2	-1.260	-0.072	-3.069	-4.698	0.009	0.003	-0.098**	-0.139***
	(1.089)	(1.287)	(1.557)	(2.268)	(0.019)	(0.024)	(0.031)	(0.043)
3	-0.018	-1.197	2.250	-1.584	-0.042	-0.024	-0.035	-0.106*
	(1.125)	(1.305)	(1.656)	(2.259)	(0.019)	(0.025)	(0.032)	(0.041)
4	-1.224	-2.259	-1.350	-0.828	-0.019	-0.090***	-0.070	-0.094
	(1.179)	(1.377)	(1.701)	(2.385)	(0.021)	(0.027)	(0.034)	(0.044)
R-squared	0.037	0.037	0.036	0.036	0.010	0.010	0.010	0.009
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129
	Hours promotions				Wage promotions			
Quarter								
-2	0.016	-0.001	0.028	0.006	0.014	-0.001	-0.006	-0.003
	(0.017)	(0.020)	(0.023)	(0.034)	(0.013)	(0.015)	(0.019)	(0.030)
-1	-0.038	-0.019	-0.067**	-0.013	-0.001	0.018	-0.011	0.017
	(0.016)	(0.020)	(0.025)	(0.038)	(0.011)	(0.014)	(0.017)	(0.027)
0	-0.052**	-0.059**	-0.061	-0.122**	-0.019	-0.034	-0.016	-0.002
	(0.017)	(0.021)	(0.027)	(0.040)	(0.012)	(0.015)	(0.018)	(0.028)
1	0.022	-0.010	-0.040	-0.028	-0.009	-0.039*	-0.029	-0.004
	(0.017)	(0.021)	(0.027)	(0.040)	(0.013)	(0.015)	(0.020)	(0.031)
2	0.003	0.026	-0.047	-0.077	-0.002	-0.024	-0.052	-0.055
	(0.018)	(0.022)	(0.028)	(0.038)	(0.013)	(0.017)	(0.022)	(0.033)
3	-0.042	-0.011	-0.011	-0.041	-0.031	-0.028	-0.044	-0.073*
	(0.019)	(0.024)	(0.032)	(0.040)	(0.015)	(0.017)	(0.021)	(0.030)
4	-0.009	-0.053	-0.015	-0.016	0.007	-0.042	-0.031	-0.067
	(0.020)	(0.026)	(0.032)	(0.040)	(0.015)	(0.020)	(0.023)	(0.034)
R-squared	0.005	0.006	0.006	0.006	0.046	0.046	0.048	0.049
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors are clustered by firm.

Notes: The table shows estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) on coworkers' earnings and promotion outcomes. The estimates for promotions should be interpreted as the effects of raising the SRA for a single focal worker on the number of promotions at the firm in the respective quarter. For the earnings growth measure, the simplest way to interpret the estimates is to assume the effects are borne by a single coworker (see the text for more details).

Table A6: Effects of raising the Statutory Retirement Age (SRA) on the combined earnings and hours of stable coworkers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Change in earnings (p.p.)				Change in hours (p.p.)			
	Treatment: SRA increase (months)				Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13	3 or 4	6 or 7	9 or 10	13
Quarter								
-2	2.781	-0.189	1.854	-1.449	-1.053	-3.915	-0.117	-0.675
	(1.359)	(1.566)	(1.980)	(2.385)	(1.827)	(2.052)	(1.890)	(3.060)
-1	-2.088	0.153	-2.943	-2.025	-0.126	-1.845	-2.736	-1.260
	(1.215)	(1.386)	(1.854)	(2.349)	(1.476)	(1.539)	(1.683)	(2.529)
0	-3.861***	-6.255***	-4.986*	-4.455	-3.294*	-3.528	-2.907	-3.123
	(1.206)	(1.494)	(1.944)	(2.475)	(1.242)	(1.728)	(1.845)	(2.790)
1	2.736	-0.288	-4.383	0.117	1.206	-0.477	-1.953	-0.684
	(1.260)	(1.377)	(1.863)	(2.664)	(1.332)	(1.449)	(1.764)	(2.7)
2	0.774	0.765	-1.944	-3.888	0.288	0.504	-0.657	-1.206
	(1.287)	(1.431)	(1.935)	(2.448)	(1.404)	(1.449)	(1.746)	(2.529)
3	-1.467	-1.143	-0.279	-4.392	-0.360	0.144	0.774	-0.576
	(1.305)	(1.467)	(1.971)	(2.358)	(1.449)	(1.548)	(1.737)	(2.619)
4	-2.538	-4.167*	-1.467	-2.565	-2.466	-2.871	1.116	-1.485
	(1.395)	(1.566)	(2.124)	(2.169)	(1.494)	(1.710)	(1.917)	(2.565)
R-squared	0.034	0.034	0.034	0.035	0.187	0.186	0.186	0.182
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the monthly percentage point change in the combined hours and earnings of stable coworkers at their firm. The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

Table A7: Sensitivity of estimated effects on promotion rates to earnings threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Coworker promotions, based on monthly earnings increase of at least						
	10%	20%	40%	€250	€500	€750	€1000
	Treatment: SRA increase of 3–4 months						
Quarter							
-2	0.034	0.027	0.011	0.032	0.019	0.008	0.003
	(0.017)	(0.012)	(0.008)	(0.015)	(0.009)	(0.007)	(0.005)
-1	-0.046**	-0.027	-0.015	-0.047***	-0.014	-0.009	-0.004
	(0.016)	(0.013)	(0.008)	(0.014)	(0.008)	(0.006)	(0.004)
0	-0.077***	-0.060***	-0.031***	-0.042**	-0.026**	-0.012	-0.003
	(0.018)	(0.013)	(0.008)	(0.015)	(0.010)	(0.007)	(0.004)
1	0.014	0.023	0.016	0.012	0.009	0.002	-0.005
	(0.019)	(0.013)	(0.009)	(0.016)	(0.010)	(0.007)	(0.005)
2	0.009	0.007	-0.008	0.017	0.006	-0.004	-0.002
	(0.019)	(0.014)	(0.009)	(0.017)	(0.010)	(0.007)	(0.005)
3	-0.042	-0.015	-0.003	-0.044**	-0.007	0.002	-0.004
	(0.019)	(0.014)	(0.010)	(0.016)	(0.010)	(0.007)	(0.005)
4	-0.019	-0.014	-0.003	-0.006	-0.009	-0.007	-0.002
	(0.021)	(0.016)	(0.011)	(0.018)	(0.011)	(0.008)	(0.006)
R-squared	0.010	0.005	0.004	0.023	0.009	0.006	0.004
Observations	1,466,233	1,466,233	1,466,233	1,466,233	1,466,233	1,466,233	1,466,233

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the promotions of coworkers. We show the sensitivity of the estimates for the 3–4 month treatment to different definitions of promotions. The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

Table A8: Robustness of estimates on hiring and promotion rates

	(1) Main estimate	(2) Twoway clustering	(3) Relaxing size restriction	(4) One focal worker per firm-pair
	New hires			
Quarter				
-2	-0.014	-0.014	-0.015	-0.005
	(0.014)	(0.015)	(0.011)	(0.015)
-1	-0.069***	-0.069***	-0.053***	-0.075***
	(0.014)	(0.014)	(0.011)	(0.015)
0	0.001	0.001	-0.005	-0.002
	(0.014)	(0.013)	(0.012)	(0.016)
1	0.043**	0.043**	0.036***	0.041*
	(0.014)	(0.014)	(0.011)	(0.015)
2	0.013	0.013	0.008	0.011
	(0.014)	(0.017)	(0.012)	(0.016)
3	-0.010	-0.010	0.000	-0.014
	(0.014)	(0.013)	(0.011)	(0.016)
4	-0.011	-0.011	-0.019	0.003
	(0.016)	(0.014)	(0.013)	(0.018)
R-squared	0.015	0.015	0.011	0.013
Observations	1,466,233	1,466,233	1,760,373	898,943
	Coworker promotions			
Quarter				
-2	0.034	0.034	0.017	0.033
	(0.017)	(0.017)	(0.014)	(0.019)
-1	-0.046**	-0.046***	-0.023	-0.050**
	(0.016)	(0.013)	(0.013)	(0.018)
0	-0.077***	-0.077***	-0.060***	-0.081***
	(0.018)	(0.017)	(0.014)	(0.019)
1	0.014	0.014	0.031	0.013
	(0.019)	(0.019)	(0.015)	(0.021)
2	0.009	0.009	0.016	0.005
	(0.019)	(0.017)	(0.015)	(0.022)
3	-0.042	-0.042**	-0.034	-0.045
	(0.019)	(0.015)	(0.015)	(0.021)
4	-0.019	-0.019	0.001	-0.007
	(0.021)	(0.019)	(0.016)	(0.024)
R-squared	0.010	0.010	0.008	0.008
Observations	1,466,233	1,466,233	1,760,373	898,943

Notes: * denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for multiple hypothesis testing. Standard errors in parentheses. The table examines robustness of the estimates from equation (5.3) for the 3–4 month treatment: column 2 shows estimates if we cluster by both focal worker's firm and their month-year of birth; column 3 allows firms to grow/shrink beyond the 5–200 worker range; column 4 drops firms with multiple focal workers per cohort-pair. See Section 5.4 for more information.

Table A9: Estimated effects of raising the Statutory Retirement Age (SRA) on firm survival

	(1)	(2)	(3)	(4)
	Firm is operating			
	Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13
Quarter				
-2	-0.000	-0.000	-0.000	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
-1	-0.001	-0.001	-0.002	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)
0	-0.000	-0.001	-0.003	-0.002
	(0.001)	(0.002)	(0.002)	(0.003)
1	-0.000	-0.001	-0.004	-0.005
	(0.001)	(0.002)	(0.003)	(0.003)
2	-0.000	-0.002	-0.005	-0.007
	(0.001)	(0.002)	(0.003)	(0.004)
3	-0.002	-0.003	-0.007	-0.011
	(0.001)	(0.003)	(0.004)	(0.005)
4	-0.004	-0.006	-0.007	-0.013*
	(0.001)	(0.003)	(0.004)	(0.005)
R-squared	0.015	0.014	0.015	0.016
Observations	1,054,542	784,552	525,292	255,302

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker in a median-sized firm — on firm survival. The estimates provide suggestive evidence that treated firms are slightly less likely to be operating in quarter 4. The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

Table A10: Cumulative reform effects on firm and coworker outcomes by size of SRA increase

	(1)	(2)	(3)	(4)
	Size of SRA increase			
	3–4 months	6–7 months	9–10 months	13 months
Focal hours	267.8***	518.2***	775.2***	977.6***
	(16.6)	(32.2)	(46.5)	(72.9)
Implied increase in full-time equivalents	0.475	0.472	0.471	0.434
Non-focal hours	-177.9***	-382.4***	-698.5***	-729.7***
	(61.0)	(110.3)	(153.0)	(228.1)
Focal earnings	5,763***	10,799***	16,235***	19,826***
	(411)	(798)	(1,159)	(1,726)
Non-focal earnings	-1,803	-5,414**	-10,286***	-13,831***
	(1,487)	(2,641)	(3,413)	(4,738)
New hires	-0.060*	-0.083	-0.113	-0.240*
	(0.034)	(0.060)	(0.083)	(0.134)
Coworker separations	-0.009	0.010	-0.022	0.014
	(0.034)	(0.058)	(0.082)	(0.127)
Coworker promotions	-0.139***	-0.280***	-0.440***	-0.581***
	(0.049)	(0.093)	(0.133)	(0.182)
Hours promotions	-0.091*	-0.121	-0.211*	-0.247
	(0.047)	(0.086)	(0.123)	(0.171)
Wage promotions	-0.054	-0.135**	-0.169*	-0.171
	(0.036)	(0.062)	(0.090)	(0.140)
Observations	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$. Standard errors in parentheses are clustered by firm.

Notes: The table shows the estimated cumulative effects from a modified version of equation (5.3), with a single treatment effect for the period from quarter -2 to quarter 4, of the effects of raising the Statutory Retirement Age (SRA). Earnings estimates are in 2019 €. Table 1 re-scales these estimates in various ways.

Table A11: Heterogeneity in the estimated effects on hiring by the earnings change of the new hires

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New hires: Upward movers				New hires: Sideways/downward movers			
	Treatment: SRA increase (months)				Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13	3 or 4	6 or 7	9 or 10	13
Quarter								
-2	-0.011	-0.006	0.020	-0.015	-0.003	-0.003	0.002	0.009
	(0.011)	(0.012)	(0.015)	(0.025)	(0.007)	(0.008)	(0.010)	(0.015)
-1	-0.056***	-0.064***	-0.055***	-0.061	-0.013	-0.022*	-0.031**	-0.037
	(0.012)	(0.014)	(0.017)	(0.026)	(0.007)	(0.009)	(0.011)	(0.016)
0	0.010	-0.026	-0.065***	-0.044	-0.009	-0.011	-0.026	-0.022
	(0.012)	(0.014)	(0.017)	(0.026)	(0.008)	(0.009)	(0.011)	(0.020)
1	0.033**	0.035	0.000	-0.031	0.010	-0.002	-0.012	-0.036
	(0.012)	(0.014)	(0.018)	(0.027)	(0.008)	(0.009)	(0.012)	(0.016)
2	0.006	0.023	0.033	-0.034	0.007	0.016	-0.012	-0.021
	(0.011)	(0.013)	(0.017)	(0.026)	(0.009)	(0.010)	(0.013)	(0.018)
3	-0.018	-0.011	0.019	-0.013	0.009	0.023	0.030	0.005
	(0.012)	(0.015)	(0.018)	(0.025)	(0.008)	(0.010)	(0.012)	(0.016)
4	-0.007	-0.020	-0.008	-0.001	-0.004	0.008	0.031	0.041*
	(0.013)	(0.015)	(0.019)	(0.028)	(0.009)	(0.010)	(0.013)	(0.017)
R-squared	0.011	0.011	0.011	0.010	0.007	0.007	0.007	0.007
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the hiring of different groups. We define new hires as "upward movers" if they have a 10% increase in their monthly earnings upon being hired, which includes individuals moving into employment. The remainder of new hires are defined as "sideways/downward movers". The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

Table A12: Estimated effects of raising the Statutory Retirement Age (SRA) on hiring rates by recent employment history of the new hires

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New hires: Recently employed				New hires: Recently non-employed			
	Treatment: SRA increase (months)				Treatment: SRA increase (months)			
	3 or 4	6 or 7	9 or 10	13	3 or 4	6 or 7	9 or 10	13
Quarter								
-2	-0.005	-0.012	0.013	-0.008	-0.010	0.003	0.009	0.002
	(0.011)	(0.013)	(0.015)	(0.024)	(0.007)	(0.008)	(0.010)	(0.015)
-1	-0.050***	-0.058***	-0.056***	-0.062	-0.020*	-0.028**	-0.030*	-0.036
	(0.011)	(0.014)	(0.017)	(0.025)	(0.008)	(0.009)	(0.012)	(0.016)
0	-0.001	-0.031	-0.060***	-0.051	0.002	-0.005	-0.032**	-0.015
	(0.012)	(0.014)	(0.017)	(0.029)	(0.008)	(0.009)	(0.011)	(0.017)
1	0.029*	0.024	-0.013	-0.046	0.014	0.009	0.002	-0.021
	(0.012)	(0.014)	(0.018)	(0.025)	(0.007)	(0.009)	(0.011)	(0.016)
2	0.010	0.028	0.007	-0.049	0.003	0.010	0.015	-0.006
	(0.012)	(0.015)	(0.019)	(0.028)	(0.007)	(0.009)	(0.010)	(0.015)
3	-0.000	0.016	0.037	-0.015	-0.009	-0.004	0.012	0.007
	(0.012)	(0.015)	(0.019)	(0.026)	(0.008)	(0.010)	(0.012)	(0.016)
4	-0.004	0.002	0.027	0.058	-0.007	-0.014	-0.004	-0.018
	(0.013)	(0.016)	(0.019)	(0.027)	(0.009)	(0.010)	(0.013)	(0.018)
R-squared	0.012	0.012	0.012	0.012	0.009	0.009	0.009	0.008
Observations	1,466,233	1,094,089	739,273	367,129	1,466,233	1,094,089	739,273	367,129

* denotes $p < 0.1$, ** denotes $p < 0.05$ and *** $p < 0.01$, accounting for the multiple hypothesis tests in each regression using a Bonferroni correction. Standard errors in parentheses are clustered by firm.

Notes: This table presents estimates from equation (5.3) of the effects of raising the Statutory Retirement Age (SRA) — for a single focal worker within the firm — on the hiring of different groups. We define new hires as "recently employed" if they were employed at some point in the preceding three months, while the remainder are categorized as "recently non-employed". The sample is constructed using monthly register data on the universe of employment spells from Statistics Netherlands.

B Distributional implications of retirement delays within firms

We estimate the distributional implications of increases in the SRA in a number of steps. First, we construct earnings ventiles in each month-year over the sample period for the entire population of Dutch workers. We allocate worker-month observations in our sample to ventiles, and calculate the mean earnings in each ventile over the sample period (in 2019 €). Second, we assess the effects of increases in the SRA on the number of workers employed in different earnings ventiles. We construct eight distinct counts for each firm, earnings ventile and month-year: (i) focal workers, (ii) non-focal workers, (iii) older non-focal workers (age 50+), (iv) young workers (age 20–34), (v) male focal workers, (vi) female focal workers, (vii) male non-focal workers, and (viii) female non-focal workers. Third, we normalize these counts by the size of the firm when the focal worker is aged 64.5 years old and then estimate equation (5.3) for each group-ventile combination. The normalization on the left- and right-hand side of this regression allows us to interpret the regression estimates as the effects of a single retirement delay within the firm on the number of workers employed in the respective earnings ventile. Fourth, we estimate the average impact of an SRA increase over the 21-month period of observation around the SRA (quarters -2 to 4) by taking the mean of the estimated treatment effects. Fifth, we calculate net effects on different groups: (a) we calculate the net effects overall for each ventile by adding effects (i) and (ii); (b) we calculate the net effects on older workers by adding the effects (i) and (iii); (c) the net effects on young workers is estimated by (iv); (d) the net effects on male workers is estimated by adding the effects (v) and (vii); and (e) the net effects on female workers is estimated by adding the effects (vi) and (viii). Sixth, we re-scale all of the estimated effects by the size of the first stage (i.e., the fraction of focal workers that delay retirement in response to the SRA increase).

Upon completing these steps, we plot in Figure A11 the estimated effects of an SRA increase on the number of workers in groups (a) to (e). We also calculate the net effects on the total earnings of each group by multiplying the estimated effects on the number of workers in each ventile by the mean monthly earnings within each ventile over the sample period, and then summing across ventiles. We then multiply these (monthly) effects by 21 to get the total effects on earnings over the 21-month period around the SRA. We use these estimates to calculate the relative changes in earnings for different groups in a median-sized firm (21 workers). For example, we calculate the change in the old-young earnings ratio. We then distinguish changes in these ratios that are purely mechanical (i.e., driven by the retention of focal workers) and those that result from spillovers. To calculate the mechanical component, we set the estimated effects on non-focal workers to zero and re-estimate the change in our earnings ratio of interest. The spillover component is the residual effect.

We present estimates of the distributional implications of a six-month retirement delay in Figure A11 and Table 2. To avoid presenting four sets of estimates, we use our estimates of a 6–7 month increase in the SRA for these calculations. We found qualitatively similar effects when we examined smaller (3–4 month) and larger (13 month) increases in the SRA, with the reduced-form effects growing with the length of the retirement delay.

C Estimates of the marginal value of public funds

In this section, we estimate the marginal value of public funds (MVPF) of a policy that raises the SRA by a small amount and demonstrate the importance of accounting for within-firm spillovers. The MVPF is the ratio of society's willingness to pay (WTP) for a policy divided by the net cost to the government (Hendren and Sprung-Keyser, 2020).

For individuals directly affected by an increase in the SRA, the forgone public pension income enters their WTP with a negative sign, while the additional earnings and other welfare benefits (Atav et al., forthcoming) enter their WTP with a positive sign. Our baseline assumption is that individuals are indifferent between €1 of income from the three sources. This is defensible for the population who receives welfare prior to the SRA, as their labor supply decisions are unlikely to be strongly affected if they have to transition onto the public pension slightly later. However, for individuals who are working near the SRA, it is more difficult to compare the value of €1 in earnings and €1 from the pension. Many individuals retire immediately at the SRA, and this increase in leisure may be valuable. In addition, consistent income from the public pension may alleviate liquidity constraints or reduce income volatility. Thus, as a robustness check, we consider the case where €1 in earnings are worth just €0.50 in pension income.

Our estimates are based on the assumptions and parameters in Table C1 (on page C3), which draw on estimates of other Dutch studies (De Koning et al., 2017; Atav et al., forthcoming). We calculate the implications of raising the SRA by one month. On average, each person that is directly affected by this policy loses €717 in income from the public pension (after tax), but gains €275 from other welfare payments and €322 in earnings. Thus, the average WTP for this policy among older individuals ranges from -€282 to -€120, depending on the relative value of earnings and pension income.[A1] The net fiscal cost is -€502, which comprises the change in costs on the public pension of -€717, the additional costs on other welfare payments and administration costs of €306, minus the change in revenue from additional income taxes of €91. Thus, ignoring within-firm spillovers, we calculate a MVPF of 0.239 (0.560 if earnings are less valuable).

[A1] These welfare costs are concentrated among non-workers, whose WTP is -€316 on average. Workers' WTP ranges from -€188 to €308, depending on the relative value of earnings and pension income.

These estimates for the MVPF are low, which suggests that raising the SRA may be an efficient way for the government to reduce expenditure on older individuals. Although this would result in welfare losses for older individuals, the Dutch government could compensate retirees in other ways that have a higher MVPF, resulting in a welfare improvement. For example, a hypothetical transfer to retirees that has no labor supply effects would have a MVPF of 1.

These calculations do not consider spillovers onto coworkers and firms, which can significantly affect calculations of the MVPF (Paradisi, 2021). Thus, we consider the importance of these effects in our setting. Across incumbent coworkers, we estimate an earnings loss of €88 (€45 after tax) for each older person that is affected by the policy. While part of this spillover results from fewer hours worked, these income losses may extend beyond our period of analysis, so we assume that coworkers' WTP is -€45. Considering these spillovers to coworkers' increases the MVPF from 0.239 to 0.360 (or from 0.560 to 0.710). Across new hires, we estimate an earnings loss of €63 (€36 after tax).[A2] Accounting for these spillovers to new hires, our estimates of the MVPF increase further, from 0.360 to 0.465 (or 0.710 to 0.837). These calculations show that the estimates of the MVPF increase considerably when we consider the spillover effects on the careers of younger workers. Thus, increases in the SRA start to look less attractive as a policy option once we consider spillovers on other groups, particularly if policymakers place higher welfare weights on groups that are less affluent (lower earners, young people, women), who are disproportionately affected by the policy.

More generally, these calculations highlight the importance of understanding the broader implications of raising the SRA on other agents in the economy. Beyond coworkers and potential coworkers, another important group to consider is firms. Although we cannot directly estimate the incidence of changes in the SRA on firms' profits due to data limitations, we can explore how sensitive the calculations of the MVPF are to possible effects on firms' profits. We consider a specific case where firms set back-loaded compensation profiles to induce effort (Lazear, 1979). Specifically, we assume that workers close to retirement are paid 10% more than the value of their marginal product, while younger workers are paid 10% less.[A3] Under these assumptions,

[A2]These calculations allow for the fact that new hires may work more in other jobs if they are not hired due to firms' responses. Specifically, we use the change in their total earnings upon being hired as an estimate of the net earnings effects of being hired by a focal firm. This is considerably smaller than their earnings at the focal firm.

[A3]These compensation profiles are plausible in the Dutch setting. Wages are often linked directly to job tenure (Mulders, 2018), and firms retain only a minority of older workers beyond the SRA, suggesting that older workers are paid more than their value to the firm on average.

raising the SRA reduces firms' profits by €46, which further increases the MVPF, from 0.465 to 0.585 (or 0.837 to 0.967). These hypothetical calculations suggest that understanding the incidence of retirement delays on firms is an important area for future research.

Table C1: Parameters and assumptions for calculations of the marginal value of public funds of a small increase in the Statutory Retirement Age

Statistic	Value	Source
Δ income from public pension (after tax)	-€717	2019 net pension amounts. Assumes 9.5% of older individuals are single (Atav et al., forthcoming). Values include the holiday allowance paid in May but assume the payroll tax credit is used by another income source.
Δ income from other payments (after tax)	€275	De Koning et al. (2017) estimate that 38.3% of the reduction in public pension benefits are offset by an increase in other benefits.
Δ administration costs	€31	De Koning et al. (2017) estimate that 4.3% of the reduction in public pension benefits is offset by increased costs of administering other payments.
Share of population working at age 65	31.4%	Own calculations for people turning 65 in 2015–2018.
Δ gross earnings among older individuals	€413	Own calculations based on share of population working at 65 and change in their earnings at the SRA.
Δ gross earnings of coworkers	-€88	Estimates in Table A5 imply that ∼21.3% of the increase in older workers' earnings are offset by effects on coworkers.
Δ gross earnings of new hires	-€63	Estimates in Table A12 imply that ∼15.2% of the increase in older workers' earnings are offset by effects on new hires. Uses the increase in total earnings upon being hired for workers hired from other jobs (€548) and workers hired from non-employment (€2,148).
Average tax rate of older workers	22%	Dutch Income Tax Calculator (`https://thetax.nl/`) for 2019 based on average earnings of older workers at age 65.
Marginal tax rate of incumbent coworkers	49%	Dutch Income Tax Calculator (`https://thetax.nl/`) for 2019 based on average earnings of incumbent coworkers.
Marginal tax rate of new hires	43%	Dutch Income Tax Calculator (`https://thetax.nl/`) for 2019 based on average earnings of new hires.
Corporate tax rate	19%	Rate for income up to €200,000 in 2019.