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Longer careers: A barrier to hiring and coworker advancement?

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

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Abstract

In response to the increasing fiscal burden imposed by public-pension systems, many countries have successfully encouraged older workers to delay retirement. These career extensions may significantly affect both the hiring and firing decisions of firms and the career progression of younger workers. To study these effects, we leverage reforms in the Netherlands in 2011/12 that gradually increased the eligibility age for public-pension benefits across birth cohorts. Using administrative linked employer-employee data, we first show that the reforms have significantly extended careers, doubling employment rates at ages that were directly affected. Next, we show that firms respond by delaying hiring, and hiring fewer workers overall. Co-workers also experience slower earnings growth over the period of career extensions, which is mainly attributable to a reduction in hours worked rather than lower hourly wages, but their separation rates from the firm are not affected. We support these findings with a descriptive analysis of an earlier Dutch reform in 2006, which reduced the share of older workers taking up early retirement and reveals similar dynamics.

Keywords: retirement reform; firm responses; coworker spillovers

JEL Classification: H34, J26, J32, J63, J88, L23

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1 Introduction

In response to population aging and steady increases in life expectancy, governments in many developed countries are raising eligibility ages for retirement pensions. By doing so, they aim to reduce national pension expenditures and encourage older workers to delay retirement. While the literature shows that these policies generally have the desired effects on employment,[1] there are concerns about the broader consequences of longer careers. For example, many younger workers feel that their careers are held back by their older co-workers (Bianchi et al., 2021), which may lower their effort levels and allegiance to the firm. These pressures, which are already mounting due to the significant presence of Baby Boomers in the workforce, are likely to intensify further as statutory retirement ages continue to increase.

Despite these pressures, we know surprisingly little about the impacts of longer careers on (i) the hiring-and-firing decisions of firms that employ older workers and (ii) the earnings and promotion opportunities of their younger colleagues. The dearth of research on these questions reflects a twin challenge in studying these questions: researchers require both a sharp and exogenous source of variation in retirement behavior and the ability to link a large and representative sample of older workers to the outcomes of their firms and coworkers. Existing studies are mostly focused on a single pension reform in Italy in 2011 (Boeri, Garibaldi and Moen, 2021; Bertoni and Brunello, 2020; Bianchi et al., 2021; Carta, D'Amuri and Von Wachter, 2021) and offer somewhat contradictory findings. This reform, which was was implemented in response to a severe public debt crisis, has attracted considerable research in large part due to its severity and immediacy.[2] These features created an interesting natural experiment, with minimal opportunity for anticipation, but it is not clear whether the lessons from the reform may readily translate to other contexts and countries, especially considering the cultural- and economic-specificity of the southern-European region and Italy's deep recession during implementation.

In this paper, we use high-quality administrative data from the Netherlands to estimate the effects of more gradual and representative statutory retirement age reforms on the economic outcomes of firms and workers. The Netherlands is an ideal setting for our paper. First, the

[1] E.g., Atav, Jongen and Rabaté (2021); Lindeboom and Montizaan (2020); Staubli and Zweimüller (2013); Geyer and Welteke (2021).

[2] As an example of its severity and immediacy, the reform raised the full retirement age of individuals who were weeks from retirement by up to six years.

retirement hazard of Dutch workers at the Statutory Retirement Age (SRA) is among the largest in the world; Atav, Jongen and Rabaté (2021) show that around two-thirds of remaining workers retire in the month of reaching the SRA. This reflects a feature of Dutch employment contracts, which are automatically terminated at the SRA unless explicitly renewed, and suggests that changes in the SRA will have large effects on career length. Second, we have access to monthly administrative records on the universe of firms and employees since 2010. Our linked employer-employee data allows us to identify the firms and co-workers of older workers and precisely measure their outcomes. Third, the SRA was progressively increased across birth cohorts over the sample period due to reforms in 2011 and 2012. These reforms allow us to study four incremental increases in the SRA, which have shifted the pension eligibility threshold from 65 years and 3 months to 66 years and 4 months.[3] Fourth, like most pension reforms, the reform was announced well in advance and phased in gradually across birth cohorts, which allows us to study potential anticipation effects. Fifth, the reform was implemented during a period when the Dutch economy was growing and unemployment rates were falling, contrasting with previous studies.

We focus our attention on older workers born between January 1950 and September 1953 who work in small-to-medium firms and have a strong attachment to the labor market and to their firm ("focal workers"). We divide these focal workers into five cohorts based on the variation in the SRA and estimate the effects of an SRA increment of 3–4 months using a cohort-pairs differences-in-differences strategy, where focal workers in the previous nine-month cohort form the control group. Our approach estimates the differences in labor supply between treatment and control groups in event time or by *relative age*, which is equal to age minus the SRA of the control group. We show that the SRA increments have large, positive and highly statistically significant effects on focal workers' labor supply in affected months, with focal workers working an additional 1.58 months on average in response to each SRA increment.

We use a similar approach to study the effects of SRA reforms on the outcomes of firms and coworkers. More specifically, the unit of observation remains the focal worker, but we estimate how the monthly outcomes of their firms or coworkers depend on their treatment status and age. We find that firms of treated focal workers respond by temporarily decreasing hiring, relative

[3]The SRA increased in three increments of three months, from 65 years, 3 months to 66 years, and then by four months to 66 years, 4 months.

to the control group, in the three months before focal workers reach the SRA. These effects are only partially offset by an increase in hiring in subsequent months, resulting in a net decrease in hiring that is equivalent to around 0.5 months worth of hiring. For affected coworkers, we do not find any effect on their separation rates from firms, but there is evidence of a negative effect on earnings growth. These effects operate primarily through fewer opportunities for growth in hours worked, rather than effects on wage growth. This mechanism is unsurprisingly strong in the Netherlands, which has one of the highest rates of part-time work in the OECD.

We examine heterogeneity in the effects on hiring and earnings growth. In line with Bianchi et al. (2021), we find different dynamics for firms that are contracting, in terms of workforce size, and firms that are growing. Contracting firms respond to the SRA increments of older workers by reducing their demand for other sources of labor — both in terms of new hires and the amount of hours worked by existing coworkers. In contrast, firms that are growing respond predominately by *delaying* hiring and the earnings growth of coworkers, in line with the delayed retirements of its older workers. This suggests that contracting firms use the retirement of older workers as an opportunity to become smaller — and the SRA increments force these firms to make other permanent adjustments to offset the added wage burden imposed by older workers — while growing firms want to replace the labor supplied by retiring older workers through new hires and an increase in coworkers' hours, with the SRA increments simply delaying this process by a few months.[4] These new insights about the responses of growing firms are only possible due to the higher frequency of our data (monthly) than previous studies (annual). We also examine heterogeneity by firm size. We find similar patterns in hiring for firms of different sizes, but the dynamic effects on earnings are driven by very small firms of 5–14 workers, which are more likely to be credit-constrained (Hut, 2019) and may be more reliant on the labor supplied by individual older workers.

In addition to our main analysis of the SRA increments, we conduct a descriptive analysis of an earlier national reform in 2006 that made early retirement more costly for workers born in or after 1950. The estimated effects are less precise but support the findings from our principal analysis. We find that this reform raised the employment rates of affected older workers —

[4]The Netherlands is rated as having the strongest employment protection legislation in the OECD, which makes it difficult for firms to fire younger workers. Choosing to not renew an older workers' contract at the SRA is thus one of the few ways a Dutch firm can unilaterally downsize its workforce. This may explain why, in contrast to Bianchi et al. (2021), we find a negative effect on hiring but no effect on layoffs (as proxied by separations) — Bianchi et al. (2021) find that the Italian reform led to an increase in layoffs but had no effect on hiring.

consistent with Lindeboom and Montizaan (2020) — and there is suggestive evidence that firms respond by limiting hiring and the earnings of coworkers, with the latter effects operating through effects on work hours.

This paper is structured as follows. In Section 2, we summarize the literature that is related to our study. Section 3 outlines the Dutch pension system and its recent reforms. Section 4 describes the data. Section 5 introduces the research design used to estimate the effects of the SRA reform on focal workers, and presents the corresponding findings. Section 6 presents the analysis of the effects of the SRA reform on firm and coworker outcomes. Section 7 summarizes our descriptive analysis of the 2006 reform. Section 8 discusses the external validity of our findings, and Section 9 concludes.

2 Related literature

Our study contributes to several strands of the literature. First, our work is related to studies of the effects of older workers' employment on the job opportunities of younger workers. This literature aims at testing the so called lump-of-labor argument. This argument, which implicitly assumes that the total number of jobs and working hours is fixed, posits that an increase in the retirement age would crowd out job opportunities for younger workers. Most of this earlier literature, based either on cross-country comparisons (Gruber, Milligan and Wise, 2010; Dorn and Sousa-Poza, 2010; Munnell and Wu, 2013) or on the analysis of single countries (Börsch-Supan and Schnabel, 1998; Gruber and Wise, 2010), finds no evidence that younger workers are crowded out. However, the reliance on aggregate data is a limitation of these studies.

More recently, several studies have examined the lump-of-labor argument in the context of pension reforms, which provide plausibly exogenous changes in the employment rates of older workers. For instance, Bertoni and Brunello (2020) exploit the different intensity of treatment across local labor markets induced by a 2011 Italian reform that abruptly increased minimum retirement ages, while Vestad (2013) and Martins, Novo and Portugal (2009) exploit changes in pension regulation in Denmark and Portugal. In contrast to the earlier estimates, these studies do tend to find evidence of substitution between old and young workers.

Second, our study is linked to a small but growing literature on career spillovers, which stems from relaxing the assumption that workers' careers are independent. In particular, career

spillovers might be present if it is difficult for firms to create new positions and for workers to switch firms (Bianchi et al., 2021). Previous studies have exploited plausibly exogenous shocks to worker separations driven by worker deaths or parental leave. For instance, Jäger and Heining (2019) analyze the effects of unexpected worker deaths on firms' demand for incumbent workers and new hires, finding a positive wage effect on coworkers in the same occupation as the deceased, and a negative wage effect for deaths of managers or workers in high-skilled occupations. This evidence is consistent with models in which firms face frictions in replacing workers externally. Ginja, Karimi and Xiao (2020) study the exogenous variation provided by a parental leave expansion in Sweden and find that firms responded by hiring new permanent workers and by increasing the work hours of incumbents, which came at a cost, as firms' total wage bill increased. Brenøe et al. (2020) estimate the effect of a female employee giving birth and taking parental leave on small firms and coworkers in Denmark. They find that most firms compensate for the lost labor supply by making adjustments both at the extensive and intensive margins, and that this does not affect the overall firm performance, measured in terms of output or firm survival.

Finally, our work is also related to the literature analyzing the effects of an aging workforce on firms' performance. If firms' pay structures follow Lazear's model of deferred compensation (Lazear, 1979) — whereby wages are below productivity in early phases of workers' career, and then increase at a faster rate than productivity — then an increase in the share of older workers in the firm could reduce firms' profits, especially in presence of wage rigidities or strict employment protection legislation (Martins, Novo and Portugal, 2009). Most of the research in this field is either based on cross sectional analysis or on panel data: while the former usually find a negative association between older workers presence in the firm and firm productivity, the latter do not confirm this finding (see Carta, D'Amuri and Von Wachter (2021) for a review).

To date, relatively little is known about the impact of statutory retirement age reforms on the hiring-and-firing decisions of firms that employ older workers, and the economic opportunities of their younger colleagues. However, the growing availability of longitudinal linked employer-employee datasets has sparked a considerable research interest in these questions. The empirical evidence so far has focused primarily on a pension reform that took place in Italy in 2011, which led to a sizable increase in the statutory eligibility age for public pension.

Several recent studies analyze the reform effects by exploiting firm-level differences in ex-

posure to the reform (leveraging the fact that firms with older workers born prior to the cutoff year were less exposed to the reform than firms with workers born after the cutoff year). Boeri, Garibaldi and Moen (2021) study how the pension reform affected employment opportunities of workers in different age groups, using a sample of firms with more than 15 employees. Their results point to crowding out of middle-aged workers and, to a lesser extent, young workers as well. Bianchi et al. (2021) use a sample of firms with 10 to 200 employees and find that an increase in older workers' presence at the firm leads to adverse effects on youth employment prospects and younger workers' careers, consistent with older workers blocking the careers of their younger colleagues. On the other hand, Carta, D'Amuri and Von Wachter (2021) analyze larger firms and find an increase in employment of young and middle-aged workers, consistent with older and younger workers being complements. In terms of firm productivity, they find that total labor costs and value-added increase in line with employment, leaving labor productivity and unit labor costs unchanged.

Outside of Italy, two other unpublished studies have exploited pension reforms in Portugal and the Netherlands (Martins, Novo and Portugal, 2009; Hut, 2019). Martins, Novo and Portugal (2009) study a Portuguese reform that increased women's legal retirement age (Martins, Novo and Portugal, 2009); they examine firms with less than 100 employees and find a negative effect on the hiring of younger workers despite no effects on the careers of older women. Meanwhile, Hut (2019) examines the same 2006 reform in the Netherlands as Section 7 of our paper; the author finds that a decrease in the generosity of the early retirement pathway led to a reduction in affected firms' investment and employment of younger workers, with the effects concentrated in cash-constrained firms.

3 Institutional Background

3.1 Three pillars of the Dutch Pension System

The Dutch pension system consists of three pillars. The first pillar (*AOW, Algemene Ouderdomswet*) was introduced in 1957 as a flat-rate pension financed by pay-as-you-go social insurance contributions. Since 1974, the pension has been indexed by the after-tax minimum wage, and subsequent revisions also made it dependent on partnership status (de Vos, Kapteyn and Kalwij,

2018; Atav, Jongen and Rabaté, 2021).[5] Individuals start receiving this pension once they have reached their statutory retirement age (SRA), which was fixed at 65 years until 2013. Individuals cannot bring the receipt of this pension forward, and their employment contracts are terminated upon reaching the SRA, unless they are explicitly renewed (Atav, Jongen and Rabaté, 2021).[6] The ease of employment termination at the SRA is an important feature of Dutch labor markets, since it contrasts with the extreme difficulty of pursuing forced redundancies at earlier ages.As such, the employment terminations enabled at the SRA remain one of the few tools that can be used by a firm that wants to downsize or restructure its workforce. This, combined with the fact that SRA-aged individuals lose eligibility for unemployment or disability insurance benefits, helps explain why the employment rate falls sharply at the SRA threshold.

The second pillar consists of firm- and sector-specific funded pension schemes that supplement the first-pillar benefits. These pension schemes are negotiated between unions and employer organisations and are officially set forth in collective agreements (De Grip, Lindeboom and Montizaan, 2012). Because of these agreements, most employees are entitled to supplementary defined-benefit pensions (employees' participation in these schemes is mandatory). Individuals can decide to claim the second-pillar pensions before or after reaching the SRA, with an associated reduction or increase in monthly benefits (de Vos, Kapteyn and Kalwij, 2018).[7] Until 2006, sectoral pension schemes were subject to a preferential tax treatment whereby pension premiums were deductible from the worker's gross salary. In addition, early retirement benefits from the second pillar (available from age 55 onwards) were taxed as if they were a regular source of income (Euwals, Van Vuuren and Wolthoff, 2010).

A third pillar also exists, consisting of individual savings for retirement. However, due to the mandatory supplementary pensions in the second pillar, the third pillar is a less important part of the Dutch pension system than in other developed countries (De Grip, Lindeboom and Montizaan, 2012).

[5]In 2021, the net pension amounts to €1,226.60 per month for singles and €838.55 for partnered individuals entitled to the full pension (Sociale Verzekeringsbank, n.d.). In 2021, €1=$1.16US.

[6]A few months before reaching the SRA, an application is sent home to be filled in and returned to the social insurance bank ("Sociale Verzekeringsbank", SVB). Individuals can continue working after reaching the SRA, but this will not affect the amount of pension received (European Commission, n.d.).

[7]Partial retirement, whereby individuals claim the occupational pension while they continue working a reduced number of hours, is also possible (OECD, 2017).

3.2 Two reforms of the Dutch Pension System

Recently, two major reforms have reduced the generosity of the first and second pillars. The first reform, in 2006, abolished the favorable tax treatment of the second pillar early retirement schemes for all workers born after 1949. Workers born after 1949 were subject to the new and less generous system. This consisted of a drop in pension benefits, accompanied by an increase in pension contributions to partially offset the drop in pension wealth.[8] Importantly, penalties (supplements) on pension income when retiring before (after) the SRA were introduced, which created strong incentives to continue working. Moreover, the eligibility age for second-pillar benefits was increased to 60 years (De Grip, Lindeboom and Montizaan, 2012; Lindeboom and Montizaan, 2020).[9]

Overall, De Grip, Lindeboom and Montizaan (2012) calculate that the replacement rate for retirement at age 62 years and three months yields a pension benefit at a replacement rate of 70% of average yearly earnings since 2004 for workers born before 1950. On the other hand, the replacement rate drops to 64% for workers born after 1949, and they must work an additional 13 months to qualify for a pension at a replacement rate of 70%.

The second reform took place in 2011, and it delayed access to the first pillar. In an attempt to improve the long-term sustainability of the pension system, it introduced a series of stepwise increases in the SRA. The first of these increases came into effect in 2013 and it raised the SRA of workers retiring within that year by one month. In 2012, the reform was amended to accelerate later increases in the SRA (Atav, Jongen and Rabaté, 2021), which meant that the SRA increased by one month per year in 2013–15, three months per year in 2016–18 and four months in 2019.[10]

[8] While younger workers might have benefited from this compensation, workers born just after 1949 did not have enough time to compensate for the drop in pension benefits (De Grip, Lindeboom and Montizaan, 2012).

[9] In 2006, the Dutch government also introduced the Life Course Savings programme (Levensloopregeling), which allows tax-free saving of up to 12% of annual earnings in a fund that can be used to finance periods of non-employment, including early retirement. The total amount that can be saved is capped at 210% of annual earnings, and can be used to finance about two years of early retirement. A worker would need to save for 17.5 years to reach the cumulative maximum of 210%. Special arrangements were made for older workers who were the most affected by the pension reform: those born in 1950–1954 could save more than 12% of their annual earnings, so long as the cumulative maximum did not exceed 210% of annual earnings. Workers of the 1950 cohort had to save approximately 14% of their annual earnings for seven years to finance an early retirement at age 62; probably only a small fraction of these workers were willing or capable of saving such a high proportion of their earnings each year before retirement.

[10] In 2020 and 2021, the SRA was frozen at age 66 and four months, but it is expected to rise further to age 67 by 2024. After that, the SRA will be linked to life expectancy.

Figure 1 illustrates this reform graphically, showing that the SRA was raised by 16 months, from 65 years for workers born before January 1948 to 66 years and 4 months for workers born in or after January 1953. The figure also highlights that workers born after 1949 were affected by both reforms, which creates a potential identification issue. To ensure that our analysis of the SRA increases is not contaminated by differences attributable to the 2006 reform of the second pillar, we follow Atav, Jongen and Rabaté (2021) and focus that analysis only on individuals born after 1949 (who were subject to the same second-pillar incentives). This means that we evaluate the 13-month shift of the SRA from 65 years and 3 months to 66 years and 4 months.

Of note, we are not expecting any anticipation effects that could invalidate our empirical strategy. While the reforms of the Dutch pension system may have been to some extent anticipated (due to the obvious fiscal pressures imposed on the pension system by population aging), the timing of the reforms, as well as the discontinuous assignment rules and differential treatment of adjacent cohorts came as a surprise (De Grip, Lindeboom and Montizaan, 2012). Selection into treatment is also unlikely to be a concern, since pre-retiree workers had their SRAs assigned strictly based on their birth cohorts, and the employers were unlikely to act on this assignment (by dismissing the treated pre-retiree workers) because of the strictness of the Dutch employment protection laws.

4 Data

Our data comes from several linked population registers maintained by the Dutch national statistical agency, Statistics Netherlands. The cornerstone of our dataset is the SPOLISBUS register, which is a tax-based register that tracks the full population of workers living in the Netherlands between years 2010 and 2020. The register consists of monthly records containing workers' earnings (regular and overtime), hours of work (regular and overtime), sector of employment, and a set of hashed IDs (worker ID, employer ID, and job ID). Worker and employer IDs are used to link relevant information from other registers, such as worker's age at observation, gender, and immigration background.

Our data has several advantages over those used in previous studies. First, the data is available at a monthly frequency, which allows us to better understand the nature and the timing of workforce dynamics surrounding workers' retirements. It also facilitates our analysis

of the 2011/12 SRA reforms, because the reforms created monthly variation in the effective SRAs. Second, the data contains both monthly contractual earnings and monthly contractual hours of work, which allows us to determine whether any potential changes in workers' earnings are attributable to changes in wages or work hours. Third, the data covers all workers in the Netherlands, without any restrictions on their firm size or the sector of employment.

In the remainder of this section, we focus on the construction and description of the dataset used to evaluate the 2011/12 SRA reforms (as this is our principal empirical analysis). Information about the dataset used to evaluate the 2006 reform can be found in the beginning of Section 7.

We focus our attention on workers in the private sector who were likely to be directly affected by the reforms ("focal workers"). These are workers who have fairly strong labor market attachment, stable work histories, and who were employed shortly prior to the ages targeted by the reform. Having isolated these focal workers, we then employ the firm IDs to identify their firms and coworkers, construct their outcomes of interest, and follow these outcomes over the observation period.

This means that we select workers who were continuously employed by the same firm between ages 63 and 64.5, and worked at least 20 hours per week on average over this period. Above age 64.5, we make no restrictions on focal workers changing firms or retiring, and we follow the outcomes of their original 'focal firms' regardless of these choices. We follow focal workers for the 48 months that they are aged 63–66 years old. This covers the years 2013–2020, but we drop 2020 from the sample to avoid possible confounding effects of the COVID-19 pandemic. We define the set of coworkers for a given focal worker i in month t as anyone born in 1955–1999 who is aged at least 20 years old in month t and who works at the same firm. This includes workers who have no overlap in their employment spells with the focal worker (i.e., workers who join the firm after focal workers have retired).

As discussed in the previous section, we restrict focal workers to those born between January 1950 and September 1953. We then divide them into five nine-month birth cohorts based on their effective SRAs:

We also impose a sample restriction on firm size, focusing on focal workers in firms with 5–200

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

employees.[11] We do not consider very small firms, as a considerable proportion may be owned by the focal workers and cease existing or experience ownership transfer and restructuring after the retirement event. We also avoid large firms, because they are likely to employ a number of workers from multiple SRA birth cohorts (which complicates identification), and because the impacts of retirement delays in large firms are likely to be obfuscated by a large amount of noise (stemming from unrelated firm-level processes). These selections yield a sample of 28,779 focal workers, working in 17,211 firms with approximately 800,000 coworkers.

4.1 Key outcome variables

We use several outcome variables in our analyses. For focal workers themselves, we examine their monthly contractual earnings at the focal firm, monthly contractual hours at all firms, and employment (defined as having positive earnings at any firm).

For focal firms, we examine their monthly job flows, firm size, and overall labor costs. In terms of job flows, we use three measures: (i) the overall separation rate per 100 workers, defined as the monthly number of worker separations multiplied by $^{100}/_{\text{firmsize}_{i,t}}$, where $\text{firmsize}_{i,t}$ is the size of the firm in month t (before any separations); (ii) the separation rate of coworkers, which is the same as (i) but excludes focal worker separations in the numerator; and (iii) the overall hiring rate per 100 workers, defined as the monthly number of new hires multiplied by $^{100}/_{\text{firmsize}_{i,t-1}}$.[12] We examine changes in the relative size and labor costs of a firm by taking the natural logarithm of its number of workers and total contractual labor costs.

For coworkers, we focus on various measures capturing the growth of their earnings. Following Bianchi et al. (2021), we analyze the average monthly percentage growth of coworker earnings.[13] We also examine effects on the percentage change in the combined earnings of "sta-

[11] To harmonize the dataset further, we also drop firms that ever grow or shrink by more than 10 workers, or ever double in size, from one month to the next. Our results are robust to these restrictions.

[12] We show that our results are robust to using a fixed measure of firm size for each focal worker (i.e., firmsize_i) in the denominators to define rates of promotions, hires and separations (Section 6.4).

[13] This measure is sensitive to extreme percentage changes in individuals' earnings, so we winsorize this measure

ble" coworkers, which places more weight on changes among higher-earning coworkers.[14] We further define rates of promotions that denote the monthly number of coworkers experiencing a large and sustained increase in earnings per 100 workers (i.e., the number of promotions are multiplied by $^{100}/_{\text{firmsize}_{i,t}}$).[15] Our main measure of promotions is a sustained 20% increase in earnings, although we show qualitatively similar patterns in the estimates with other thresholds. For each of the above measures, we construct similar measures for hours worked and wages, which allows us to decompose the effects on earnings growth into changes driven by hours worked and wages.

4.2 Descriptive statistics

Table 1 summarizes the characteristics of our sample of focal workers at age 64.5, as well as the outcomes of their firms and coworkers measured at the same time. At age 64.5, focal workers worked on average 153 hours per month and earned €3,418, with a mean wage rate of €22.30 per hour. This means that focal workers earn on average 16% more than their coworkers, while working 3% more hours (with 12% higher wages). Focal workers are mainly men (79.3%), which is partly attributable to lower rates of female employment at these ages, partly to our focus on the private sector (women are more likely to work in the public sector), and partly to our requirement that focal workers must be working at least 20 hours per week.

The average focal worker works in a firm with 49.8 employees — of which 12.5 are classified as young (age 20–34), 18.3 as middle-aged (age 35–49) and 19.0 as older (age 50+).[16] On average, each focal worker has 1.4 colleagues that are also focal workers. The average monthly labor costs per focal firm are €152,874. The average monthly separation and hiring rates are 1.19 separations and 1.13 hires per 100 workers.[17] In any given month, large and sustained increases

at the 5^{th} and 95^{th} percentiles.

[14] We define stable coworkers at time t as those who worked at the firm in the two adjacent months and were not hired in either month. This measure has fewer extreme values, so we winsorize at the 1^{st} and 99^{th} percentiles.

[15] 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 earnings increases to be sustained over a three month period (i.e., we not only compare an individual's earnings in months t and $t-1$ but also in months $t+1$ and $t-2$). We also exclude coworkers that were hired in the last two months to avoid capturing increases in monthly earnings that result from workers starting midway through a month.

[16] Figure A2a shows the distribution of firm size in our sample when focal workers were aged 64.5 years old. For our analysis of spillovers on firms and coworkers, we normalize by firm size, which places more weight on focal workers in smaller firms. When we weight observations by the inverse of firm size, the average focal worker works in a firm with 22.6 employees.

[17] The slightly higher rate of separations is partly due to our definition. We define a separation as an absence

in monthly earnings are relatively uncommon: 1.0% of workers experience a 20% increase, our main threshold used to define a promotion. We observe similar but slightly lower rates of "hours promotions" (15% lower), but "wage promotions" are considerably rarer (65% lower).

5 Effects of the 2011/12 reforms on older workers' labor supply

5.1 Descriptive evidence

Figure 2 displays the labor supply trajectories of focal workers in Cohorts 1 to 5 over the period of observation. Between ages 63 and 64.5, all three outcomes — employment, hours worked and earnings — are roughly or exactly constant across cohorts. This is unremarkable as we restrict the sample to workers who are working continuously over this period. Beyond age 64.5, though, focal workers are free to retire and so labor supply begins to decline. Of note, we continue to see similar labor supply patterns across cohorts until age 65 years and 3 months, when the differences in pension eligibility start to take effect. This suggests that the labor supply trajectories of focal workers would have been very similar if not for differences in the SRA. For each cohort, we see a small drop in employment in the month in which individuals reach the SRA and a large drop in the subsequent month. For monthly work hours and monthly contractual earnings, the declines are more evenly split across these two months. This is because some workers stop working midway through the month in which they reach the SRA. Over these two months, employment rates decline from 80–90% to 35–40%, average monthly hours worked declines from approximately 120–140 hours to 40–45 hours and average monthly earnings decline from approximately 2600–3000 euros to 800–1000 euros.[18]

5.2 Sketch of research design

Even though the consequences of the reform for focal workers' labor supply are fairly evident from Figure 2, here we would like to discuss the key elements of the research design that we use to quantify the reform effects on focal workers and, in extension, also on their firms.

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from the firm greater than three months, as contract terminations are not directly observed in the data. We cannot extend this period beyond three months without dropping observations in 2019 or using labor market records beyond March 2020, when government-imposed lockdowns started to affect the economy. Hires are defined as anyone who is observed at the firm for the first time since January 2010. Thus, hires are less likely to pick up workers taking an extended break, or choosing to work only in certain months of the year.

[18]Note that the decline of the employment rate presented here is not as large as the one found by Atav, Jongen and Rabaté (2021). This is attributable to differences in imposed sample selection criteria.

To obtain causal estimates of the reform effects, we would like to compare the outcomes of workers who differ in terms of their SRAs, but who are similar in terms of their background characteristics and experienced labor market conditions. To this end, we leverage a differences-in-differences pair-wise cohort design, in which we compare the age-specific labor supply outcomes of focal workers from adjacent nine-month SRA birth cohorts (e.g., Cohorts 1 & 2).[19] The five birth cohorts in our sample form four adjacent cohort-pairs, which are used to estimate the effects of each of the pair-wise increases in the SRA. Within each pair, the earlier cohort (Cohort 1) acts as a control group for the later cohort (Cohort 2) that is subject to the 3 or 4 month SRA increase.

We follow the monthly labor market outcomes of focal workers for four years (48 months), covering ages 63–66. Treatment effects are allowed to vary based on the age of the focal worker in quarters.[20] For each cohort-pair, we re-center this measure of age-in-quarters so that it is measured in event time, equal to zero in the month control workers reach their SRA (and subsequent two months) and when treated workers are the same age but remain below their SRA. This facilitates the exposition of our results across multiple cohort-pairs, and it allows us to better consolidate our findings.

For each pair, the basic regression structure is as follows:

$$y_{it} = \text{age}_{it} + \sum_{j=-12}^{6} \beta_j (\text{treated}_i \times \mathbf{1}(\text{ev\_age}_{it} = j)) + \epsilon_{it} \tag{1}$$

where i indexes a focal worker and t is the time (month-year) of observation. y_{it} is a labor market outcome (e.g., monthly contractual earnings). The regressions include fixed effects for focal worker's age in months, age_{it}, which flexibly control for differences in labor market outcomes with respect to the age of focal workers.[21] The $\text{treated}_i \times \mathbf{1}(\text{ev\_age}_{it} = j)$ terms correspond to a set of dummy variables equal to one for individuals who are in the treatment group and whose

[19] Adjacent cohorts of focal workers have similar observable characteristics and work in similar firms (see Appendix Table A1). While there are modest differences between treated and control groups in some outcomes when focal workers are aged 63 to 64.5, these differences are unlikely to be large enough to give rise to differential *trends* across treatment and control groups.

[20] For example, age 63 years and 0 quarters captures the calendar months when the individual is aged between 63 years and 0 months and 63 years and 2 months at the end of the month.

[21] We include fixed effects for age in months rather than age in quarters, since some focal workers in cohort 5 do not reach age 66 years, 11 months before the end of 2019, when the sample ends. Unsurprisingly, the results are similar with fixed effects for age in quarters.

event-age is equal to j. Their coefficients, the β_j terms, are the coefficients of interest, measuring the differences between treatment and control workers at event-age j. We cluster standard errors by firm, which accounts for potential serial correlation in individuals' outcomes and correlations between the outcomes of different focal workers within the same firm.

We do not expect any treatment effects well ahead of the SRA, which means that the coefficient estimates for $j << 0$ provide a test of our parallel-trends assumption. This assumption is that, if not for the increases in the SRA, the outcomes of treated and control workers would have followed similar patterns with respect to their age. The estimates for j closer to but below 0 may capture possible anticipation effects of the higher SRA for the treatment group. We believe that such effects are likely to occur mainly in quarter -1 (i.e., the three months before the higher SRA takes effect). The estimate in quarter 0 captures the effect of raising the SRA during the months when eligibility is directly affected (at this point, the control workers have reached the SRA, but the treated workers are yet to do so). The estimate in quarter 1 corresponds to a period when eligibility is partly affected.[22] The estimates in quarter 2 and above correspond to the ages at which both groups of workers have passed their SRAs, which means that these capture any longer-term consequences of the preceding variation in the SRA thresholds.

5.3 Stacking cohort-pairs into a single regression

Rather than presenting four sets of treatment coefficients corresponding to each cohort-pair, we aggregate our four models into a single "stacked regression" (Cengiz et al., 2019). The stacked regression yields a single set of treatment coefficients that is pooled across the four pairs, thus simplifying the exposition and increasing the precision of our results. In our context, this adjustment involves expanding the data. Specifically, we duplicate the observations of focal workers in Cohorts 2–4, allocating one observation to the treatment group (with Cohorts 1–3 forming the respective control groups) and one to the control group (with Cohorts 3–5 forming the respective treatment groups). The adjusted regression structure is as follows:

$$y_{ipt} = \text{pair}_{ip} \times \text{age}_{it} + \sum_{j=-12}^{6} \beta_j(\text{treated}_{ip} \times \mathbf{1}(\text{ev\_age}_{ipt} = j)) + \epsilon_{ipt} \tag{2}$$

[22] Since age is measured at the end of the month, individuals treated by a three-month increase in the SRA will reach the SRA within the first month of quarter 1. Furthermore, individuals in cohort 5 are treated by a four-month increase in the SRA, which means that they will reach the SRA within the second month of quarter 1.

where the indexation is the same as before, except for the addition of p, which indexes the pair that the observation belongs to. Pair p is not perfectly collinear with i because, as explained above, individuals in Cohorts 2–4 have a duplicate observation that is allocated to a different pair (e.g., an individual in Cohort 2 has a treatment observation that belongs to pair (i) and a control observation that belongs to pair (ii)). These regressions interact the age-in-months fixed effects in (1) with pair dummy variables, which means that we control age-specific trends in y that are specific to each pair. We focus on the estimated effects between quarters -9 and 3, where the sample is balanced (i.e., all cohorts are observed).

Results. Figure A1 shows the estimates of equation (2) on focal workers' labor supply, alongside 95% confidence intervals. We present two sets of confidence intervals, standard confidence intervals and more conservative confidence intervals that come from a Bonferroni correction, which accounts for the 13 hypothesis tests (quarters -9 to 3) in each regression.[23] As suggested by Figure 2, there are no substantive differences in the labor supply of treated and control workers in quarters before the SRA, which adds credibility to our parallel-trends assumption and suggests the absence of anticipatory responses. In quarter 0, we observe positive, large and highly statistically significant treatment effects on all labor supply outcomes. These positive effects are expected, since the control workers have already reached their SRA, whereas the treated workers are yet to do so. Accordingly, we see that the employment rate of treated workers at this age is 35 percentage points higher than the employment rate of control workers, which is also reflected in their higher earnings and work hours. The positive effects partly extend to quarter 1 and disappear from quarter 2 onwards. This pattern is once again expected since by quarter 2 all treated workers have reached their SRAs and are subject to the same retirement incentives (and potential involuntary job loss) as the control group. The absence of significant effects from quarter 2 onwards suggests that the SRA increases delayed the retirement of focal workers to the new SRA but did not have further dynamic effects.

Adding a reference period to increase precision. Our preferred model uses a reference period of quarters -9 to -3, which increases the precision and reliability of the estimates

[23] Specifically, the Bonferroni-adjusted confidence intervals are roughly equivalent to a 99.6% confidence interval, where $0.996 = 1 - \frac{0.05}{13}$. These confidence intervals may be too conservative, since they assume the hypothesis tests are independent.

close to the SRA. We estimate the following regressions:

$$y_{ipt} = \text{pair}_{ip}(\text{age}_{it} + \text{treated}_{ip}) + \sum_{j=-2}^{6} \beta_j(\text{treated}_{ip} \times \mathbf{1}(\text{ev\_age}_{ipt} = j)) + \epsilon_{ipt} \quad (3)$$

where the indexation is the same as equation (2). Here, the baseline treatment dummy (interacted with pair dummies) allows for within-pair differences between treated and control workers over the reference period, which we define as quarters -9 to -3.[24] Setting a reference period not only increases the precision of the estimates but also re-centers the estimates close to the SRA, adjusting for any differences between treatment and control groups in the reference period. We focus on the estimated effects over the following six quarters (-2 to 3), where the sample is balanced.

Table 2 shows the estimates. In quarter 0, we estimate an increase in employment of 34.5 percentage points, an increase in hours worked of 67.0 hours per month and an increase in earnings of 1,461 euros per month. In quarter 1, we estimate further increases in employment of 17.6 percentage points, an increase in hours worked of 26.7 hours per month and an increase in earnings of 500 euros per month. All of these estimates are highly statistically significant and consistent with Atav, Jongen and Rabaté's (2021) comprehensive evaluation of the effects of this reform on older workers.

We calculate the total labor supply response to an SRA increment of 3–4 months over quarters -2 to 3.[25] The estimates indicate that, on average, focal workers work an additional 1.58 months and 282.4 hours, earning an additional 5,890 euros at their original firms. Overall, these results show that the SRA increases provide sharp variation in the labor supply of older workers and, consequently, their wage burden on firms.

[24] Omitted from equation (3) is a set of three treatment effects corresponding to quarters -12 to -10. These are purely auxiliary covariates that ensure unbiased estimation of the set of age fixed effects.

[25] Throughout the paper, we add the effects in quarters -2 to 3 — to capture possible anticipatory and dynamic effects — and multiply by three, since the unit of observation is monthly and the estimates apply to quarters.

6 Effects on firms and co-workers

6.1 Modifications to the empirical strategy

Next, we study how firm and coworker outcomes are affected by the increasing SRAs of focal workers. We track the outcomes of firms and coworkers over the same observation period (corresponding to the 48 months that focal workers are 63–66 years old). With this sample, we start by estimating regressions that derive from our stacked regression equation (2):

$$y_{ipt} = \frac{1}{\text{firmsize}_i}\left(\text{pair}_{ip} \times \text{age}_{it} + \sum_{j=-12}^{6} \beta_j(\text{treated}_{ip} \times \mathbf{1}(\text{ev\_age}_{ipt} = j))\right) + \text{time}_t + \epsilon_{ipt} \quad (4)$$

where y_{ipt} is now an outcome of the coworkers or firm of focal worker i (e.g., the number of new hires or promotions of coworkers at the firm). We normalize most outcomes by firm size, either directly by defining a rate or indirectly (e.g., we consider relative changes in firm size by taking logs). The explanatory variables are the same as in equation (2), except for the addition of month-year fixed effects, time_t, which are important as firm outcomes such as hiring and separations display stronger seasonality and relation to the business cycle than focal workers' outcomes.[26] We also normalize the treatment dummies and baseline pair-by-age fixed effects by the size of the firm when the focal worker is 64.5 years old. This adjustment reflects our view that the retirement of a single focal worker is likely to have a larger effect on the (normalized) outcomes of smaller firms.[27] To facilitate interpretation, we further multiply this normalization factor by 10, so that the coefficients β_j can be interpreted as the average effect of an SRA increment on a focal worker who represents 10% of their firm's workforce.[28]

As before, the β coefficients for $j << 0$ allow us to assess the validity of our parallel-

[26] Due to the way the sample is defined, month-year fixed effects are highly collinear with some focal worker outcomes (e.g., employment), so we prefer to estimate equation (2) and (3) without month-year fixed effects.

[27] We prefer to use a fixed measure of firm size that is captured prior to the SRA, since firm size itself may change in response to the SRA increments.

[28] Figure A2b shows the distribution of the workforce share made up by treated focal workers (defined here as anyone in cohorts 2–5, who is exposed to at least one SRA increment). On average, treated focal workers comprise 6.9% of their firms' workforce (i.e., less than 10%). However, treated focal workers in smaller firms contribute more to the identification of the estimated effects, since the treatment dummies are normalized by firm size. When we weight observations by the inverse of firm size, treated focal workers make up 10.2% of their firms' workforce (i.e., approximately 10%).

trends assumption that, if not for the increases in the SRA of focal workers, average firm and coworker outcomes should follow similar trends with respect to the event-age of focal workers. The β coefficients for j close to and above 0 capture firm or coworker responses to changes in the SRA of focal workers, which may precede or lag the direct effects on focal workers given that retirements at the SRA are a relatively predictable event (and something that firms can mandate). We continue to cluster standard errors by firm. This accounts for serial correlation in firm outcomes and corrects inference for the fact that firm or coworker outcomes may be counted multiple times if the firm has multiple focal workers.

We use equation (4) to confirm the validity of our parallel-trends assumption and assess the dynamic changes in firm and coworker outcomes around focal workers' SRA. Then, as in Section 5, we set a reference period (quarters -9 to -3) to obtain more precise and reliable estimates of the dynamics around the SRA:

$$y_{ipt} = \frac{1}{\text{firmsize}_i}\left(\text{pair}_{ip}(\text{age}_{it} + \text{treated}_{ip}) + \sum_{j=-2}^{6} \beta_j(\text{treated}_{ip} \times \mathbf{1}(\text{ev\_age}_{ipt} = j))\right) + \text{time}_t + \text{focal\_worker}_i + \epsilon_{ipt} \quad (5)$$

where these regressions modify equation (4) by including a baseline treatment dummy (interacted with pair dummies), which controls for any within-pair differences between treatment and control groups over the omitted reference quarters. We also add focal worker fixed effects, which control for any time-invariant differences between focal workers' firms, although the results are similar without them. Again, we cluster standard errors by firm and calculate the total effect around the SRA by adding the six treatment dummies for quarters -2 to 3 and multiplying by 3.

6.2 Effects on firms' job flows, overall size and labor costs

To understand affected firms' responses to the SRA increments, we start by looking at the effects on their separation and hiring rates. Figure 3a shows the estimated effects from equation (4) on the overall separation rate of firms. As explained above, this measure includes the separations of focal workers. The estimates up to quarter -2 are close to zero and show little evidence of any differential trends in the separation rates of firms employing treated focal workers. In quarter -1, there is a small and statistically significant decrease in the separation rates of firms employing

treated focal workers (relative to firms employing control focal workers). This is to be expected, since around 8% of focal workers finish working in the month before they reach the SRA (see Figure 2a). In quarter 0, there is a much larger decrease in separation rates (relative to the control group), since control focal workers reach the SRA but treated focal workers are yet to reach their SRA. This dip in separation rates is temporary and offset in quarter 1 as treated focal workers reach their SRA and leave the labor force.

Figure 3b shows the effects on the separation rates of coworkers. None of the estimates are statistically significant at the 5% level using the more conservative Bonferroni-corrected confidence intervals. There is also little change in the pattern in the estimates around focal workers' SRA. Overall, the delayed separations of focal workers do not appear to lead to changes in the dismissals of or quits by their co-workers.

Figure 3c shows the effects on hiring rates. The estimates up to quarter -2 are close to zero and statistically indistinguishable from zero at the 5% level using the Bonferroni-corrected confidence intervals. In quarter -1, the estimate is strongly negative and statistically significant, indicating that treated firms are hiring less than control firms over this period. In quarter 0, the estimate moves closer to zero and the estimate in quarter 1 becomes positive and almost statistically significant, suggesting an increase in hiring rates among treated firms relative to the control firms. These patterns are consistent with descriptive trends in hiring rates, which show that firms start to hire replacements for their elderly workers in the three months before they reach the SRA.[29] Overall, the results suggest that treated firms respond to SRA increases by delaying hiring and, potentially, hiring fewer people overall.

The estimates in Figure 3 add credibility to our parallel-trends assumption and suggest that we can causally interpret estimates of our preferred specification, equation (5), which uses a reference period of quarters -9 to -3 and controls for focal-worker fixed effects. Table 3 shows these estimates. The estimates are similar to Figure 3 but, by setting a reference period, become more precise and likely more reliable (e.g., there is suggestive evidence of slightly lower rates of separations and hiring among the treated group throughout the pre-period that can now be controlled for). For overall separations (column 1), we estimate an effect of -0.16 separations per month per 100 workers in quarter -1, -0.94 separations per month per 100 workers in quarter 0,

[29]For example, among the control group, the average hire rate jumps from 1.08 hires per 100 workers in quarters -9 to -2 (ranging 1.00 to 1.17) to 1.28 in quarter 1, and then remains between 1.23 and 1.26 in quarters 0 to 3.

and an offsetting effect of 0.87 separations per month per 100 workers in quarter 1 (all $p < 0.001$). As suggested by Figure 3b, there is little evidence of an effect on coworker separations in any quarter around the SRA (column 2), which means that the effect on overall separation rates is driven by the delayed separations of focal workers.

In terms of magnitude, the estimated effects on total separation rates in quarters 0 and 1 are around 75% of the average separation rate among focal workers' firms over the observation period. Thus, the three-month extension of focal workers' SRA equates to around 2.25 months worth of firms' average number of separations.[30]

For hiring (column 3), we estimate a negative and statistically significant effect of -0.17 hires per month per 100 workers in quarter -1 ($p < 0.001$), and a partially-offsetting effect of 0.12 hires per month per 100 workers in quarter 1 ($p = 0.003$). These effects are around 10–15% of the average hiring rate among focal workers' firms over the observation period. Thus, the reform-induced delay in hiring equates to around 0.3 to 0.45 months worth of firms' average number of hires.

We calculate the overall effect on separation and hiring rates across quarters -2 to 3. For overall separation rates, the estimated overall effect is negative but insignificant (-0.47 fewer separations per 100 workers over 18 months), as is the estimated effect on coworker separations (-0.05 fewer separations per 100 workers). For hires, the overall effect is negative and statistically significant at the 5% level (-0.55 fewer hires per 100 workers over 18 months). This suggests that the SRA increments led to both a delay and a net decrease in hiring among affected firms. The net decrease in hiring equates to approximately 0.5 months worth of hiring.

Overall, these effects on hiring and separation rates imply dynamic effects on the size and labor costs of treated firms. Column 4 of Table 3 shows that firm size decreases by 0.7% in quarter -1 ($p < 0.001$), due to the decrease in hiring among firms employing focal workers in the treatment group, but then increases by 1.6% in quarter 0 due to the delayed retirement of older workers (both $p < 0.001$). The effects on firm size then start to fall back towards zero, becoming negative in quarter 2, and negative and statistically significant in quarter 3 (-0.5%, $p = 0.019$). This decline in firm size results from the net decrease in hiring in earlier quarters. Column 5 shows that the reduction in firm size in quarter -1 results in a decline in the total

[30] $2.25 = 0.75 \times 3$

labor costs of treated firms by an estimated 0.4% ($p < 0.001$). In quarter 0, there is an increase in the labor costs of treated firms due to the retention of its relatively expensive older workers. The estimates indicate an increase in treated firms' total wage costs of 3.3% over this three-month period ($p < 0.001$). The effect on labor costs falls in subsequent quarters and becomes statistically indistinguishable from zero in quarters 2 and 3.[31] Overall, the average size and labor costs of firms' is slightly higher around the SRA. Over the 18 month period of analysis, the total number of worker-months increases by 0.11% ($= \frac{1.9}{18}$) and the total contractual labor costs increase by 0.66% ($= \frac{11.9}{18}$).

6.3 Effects on coworkers' earnings growth

In the previous section, we found no evidence of any effect of the SRA increments on coworkers' propensity to separate from firms. Here, we examine whether there were other spillover effects on coworkers, such as effects on their earnings growth and promotion opportunities.

Based on equation (4), Figure 4a shows little evidence of any pre-trend in the average earnings growth of coworkers. In quarter 0, there is a clear drop in the earnings growth of coworkers of treated focal workers, relative to coworkers of control focal workers. The estimate is negative and significant at the 5% level with conventional confidence intervals. In quarter 1, the estimate becomes positive and statistically significant at the 5% level with both conventional and Bonferroni-corrected confidence intervals. This pattern in the estimates suggest that the earnings growth of coworkers is delayed in line with the delayed separations of focal workers. We see a similar pattern in coworker promotion rates (Figure 4b), where promotions are defined as a sustained 20% increase in monthly earnings. Again, there is no evidence of any pre-trend in promotions, and there is a clear and statistically significant drop in promotions in quarter 0 before an insignificant increase in promotions in quarter 1.

Table 4 shows the estimated effects on these outcomes from equation (5). Column 1 shows that, in quarter 0, the average earnings growth of coworkers falls by 0.105 percentage points ($p < 0.001$). This decrease in earnings growth is partially offset by an estimated increase in earnings growth of 0.070 percentage points in quarter 1 ($p = 0.030$). Column 4 confirms that these changes in earnings can be explained by delayed promotions. In quarter 0, the estimated

[31] Appendix Figure A3 shows the estimates from equation (4) for firm size and labor costs. The estimate show similar dynamics around the SRA and no evidence of any pre-trend in either outcome.

number of coworkers that experience a 20% earnings increase month falls by 0.19 per 100 workers ($p < 0.001$), before increasing by 0.06 per 100 workers in quarter 1 ($p = 0.137$). As expected, we do not find any evidence that the delayed earnings growth of coworkers is explained by an increase in demotions (Table A4).

In order to discuss the magnitude of the effect on average earnings growth of coworkers, we convert it to monetary value. Given that average coworker earnings amount to €2,952 and that we estimate a decrease in mean percent change in coworker earnings by 0.105 percentage points in quarter 0, we find a decrease in coworkers' earnings growth of €3.10 per coworker per month. Over three months, since effects on earnings growth accumulate, the total effect on lost earnings is €18.60. This corresponds to just 0.4% of the size of the increase in focal workers earnings at the firm over the three months.

Overall, the magnitude of these effects on earnings growth and promotions is relatively low. However, this is well in line with the relatively infrequent incidence of sustained increases of workers' earnings. The estimated effects on earnings growth and promotions in quarter 0 are 11% and 19% of the means over the observation period respectively, which is consistent with the notion that the retirement of focal worker is unlikely to influence the earnings profiles of all workers within the firm.

Crucially, these results provide evidence that the earnings growth of coworkers is delayed by the longer tenure of focal workers. On average, the three-month delay in monthly earnings growth is equivalent to 0.33 months worth of earnings growth and 0.57 months worth of promotions. Despite the delay in earnings growth, we cannot reject the null hypothesis that there is no net effect on earnings growth and promotions over the 18-month period of analysis. While the estimated total effect in quarters -2 to 3 is negative for both outcomes, the effect is imprecisely estimated.

6.4 Robustness

In order to ensure reliability of our findings, we conduct a battery of robustness checks. First, our results are robust to alternative measures of earnings growth and promotions. In Table A2, we show the estimated effects on the combined earnings growth of stable coworkers, rather than calculating the average individual-specific change. This measure is less sensitive to extreme changes among individual coworkers, placing more weight on changes among full-time

and higher-earning coworkers. The estimates in column 1 show that the combined earnings growth of stable coworkers falls by 0.077 percentage points in quarter 0 ($p = 0.004$), before increasing by 0.069 percentage points in quarter 1 ($p = 0.016$). These estimates are relatively larger, around 70% of the mean over the observation period. As total stable coworkers' earnings amount to €134,975, this effect is equivalent to a decrease in the combined earnings growth of €103.93 per month, or €623.58 over three months. Across workers, the decrease in earnings growth amounts to around 14% of the focal workers' salary in quarter 0.

In Table A3, we show the estimated effects on the rate of coworker promotions, using different thresholds to define a promotion. In column 1, we define promotions as a sustained earnings increase of at least 10%, while in column 5 we require a sustained earnings increase of at least 50%. While the estimates become smaller under more stringent thresholds, the pattern in the estimates is similar. There is a consistent decrease in promotions in quarter 0 ($p < 0.05$ for all thresholds), followed by a partially-offsetting and statistically insignificant increase in promotions in quarter 1. In Table A5, we show the estimated effects on the rate of coworker promotions using absolute earnings increases to define a promotion. We find evidence of delayed promotions for sustained monthly earnings increases of €250+, €500+ and €750+, but not for increases of €1000+.

The results are also robust to using a fixed measure of firm size to define rates of separations, hiring and promotions. Table A6 shows that the results are similar but slightly less precise if we measure these rates relative to firms' size when the focal worker is aged 64.5 years old.

The results are also robust to alternative samples. While we maintain our restriction that firms have 5–200 workers when the focal worker is aged 64.5, we show how the results compare when we relax or impose other restrictions. In the first sample, we allow firms to grow or shrink beyond the 5–200 range.[32] In the second sample, we impose the additional restriction on our main sample (5–200 workers throughout the period focal workers are aged 63–66) that firms have at most one focal worker per pair. This reduces the sample size by 40% and ensures that none of the identification comes from within-firm comparisons. With both samples, the estimates are similar to the main estimates (Tables A8 and A9).

[32] We allow firms to shut down, and code outcomes as zeros in the case of a shut down. We exclude around 1% of observations where firms grow larger than 300 employees to minimize noise. We find no evidence that the SRA increments affect the probability of firms' shutting down (Table A7).

Finally, we show the estimated effects on our main outcomes up to quarter 6 (Table A12). One interpretation of our findings, such as the net decrease in hiring, is that we may be missing longer-term adjustments by firms. Analyzing the coefficients in quarters 4 to 6 allows us to assess these adjustments, although with the caveat that these estimates are less precise and estimated from a subset of cohorts. We find little evidence of any obvious dynamics in quarters 4–6, and the estimates do not suggest that the negative effects on hiring around the SRA are made up in subsequent quarters.

6.5 Decomposing the effects on coworkers' earnings

Increases in contractual earnings may result from increases in hours worked rather than wages. Indeed, sustained increases in hours worked are much more common in our sample than sustained increases in wages (see Table 1). Moreover, Table 4 shows little evidence that the wages growth of coworkers is delayed, while the estimated effects on hours growth largely mirrors the estimated effects on earnings. The estimated change in the mean growth in coworkers' hours is a decrease of 0.078 percentage points in quarter 0 ($p = 0.021$), followed by an increase of 0.064 percentage points in quarter 1 ($p = 0.072$). The decrease in quarter 0 is equivalent to to a decline in coworkers' hours growth of 0.12 hours per coworker per month, or 0.7 fewer hours over three months. This effect is small when compared to the size of the increase in focal workers' hours worked over the three months (67.0 x 3=201). Similarly, we find a delay in the rate of "hours promotions", defined as a sustained 20% increase in monthly hours worked; in quarter 0, hours promotions among coworkers fall by 0.157 promotions per 100 workers ($p < 0.001$), before increasing in quarter 1 by 0.072 promotions per 100 workers ($p = 0.044$).

Overall, these results suggest that the delays to coworkers' earning growth are predominantly explained by delays to hours growth rather than wages. Other analyses strengthen this conclusion: (i) the estimated effects on the combined hours growth of stable coworkers show similar dynamics to the estimated effects on the combined earnings growth of stable coworkers (Table A2)[33]; (ii) we find evidence of delayed "hours promotions", using alternative thresholds (10%–50% increases — see Table A10; monthly hours increases of 8+ hours, 16+ hours and

[33] We find a decrease in combined change in stable coworkers' hours of 5.8 percentage points in quarter 0. Across coworkers, this is equivalent to a decrease in hours growth of 3.8 hours per month, or 22.7 fewer hours worked across the three months. Overall, across coworkers, the decrease in hours growth in quarter 0 amounts to around 10% of focal workers' additional hours worked in quarter 0.

32+ hours — see Table A11) to define promotions; and (iii) we find little evidence of effects on coworkers' wages growth using any measure of wage growth or wage promotions.

6.6 Heterogeneity by firm characteristics

In this section, we examine heterogeneity in the effects based on the characteristics of firms. We focus on the effects on hiring and coworkers' earnings. Following Bianchi et al. (2021), who showed stronger effects on coworkers' earnings growth in shrinking firms, we start by examining heterogeneity with respect to the rate of firms growth. We divide firms into those that are shrinking (37%) and those that are either increasing or the same size (63%), based on changes in firm size over the 18 months following focal workers' 63^{rd} birthday. Table A13 shows the results for hiring, coworkers' earnings growth and promotions, and relative firm size. For all four outcomes, we observe different patterns in the estimates between shrinking and non-shrinking firms. Broadly speaking, non-shrinking firms appear to delay hiring and promotions in response to the SRA increments, while shrinking firms, appear to respond by decreasing hiring and promotions across multiple periods. Indeed, we observe little evidence of any net effects on these outcomes for non-shrinking firms, but there is evidence that these firms adjust when they hire and offer promotions based on the timing of focal workers' retirements. When firms are shrinking, though, they do not appear to use hiring or promotions to replace retiring focal workers — instead, these retirements allow these firms to reduce their labor costs. For shrinking firms, we observe consistent negative effects of the SRA increments on hiring and coworkers' earnings growth (and a statistically significant net decrease), which suggests that these firms compensate for the shock to their labor costs by decreasing other labor inputs over several periods. Supporting this contention, we find that shrinking firms are 0.8% smaller by quarter 3 when exposed to SRA increments, while the size of non-shrinking firms is unaffected.

We also examine heterogeneity based on firm size. We split firms into those that are very small, with just 5–14 workers on average over the period that focal workers are aged 63–66 years old, and larger firms, with 15–200 workers. This results in an uneven split, with more than three-times as many focal workers in "larger firms". However, since focal workers in smaller firms contribute more strongly to the identification of the estimates, the standard errors are similar for both samples. Very small firms are typically more credit-constrained and depend more on individual workers, which makes it particularly interesting to understand their responses. The

results are presented in Table A14. For hiring rates, we find broadly similar patterns for both very small and larger firms, but the dynamic effects on coworkers' earnings growth appears to result from effects in very small firms. In larger firms, we observe negative effects on coworkers' earnings growth on average but the coefficients are generally not significant, either individually or combined.

7 Supporting evidence from the 2006 reform

Besides analyzing the consequences of SRA increments resulting from the 2011/12 reforms, we also investigate the consequences of the earlier 2006 reform. This reform made early retirement through second-pillar schemes less attractive for all workers born in or after the year 1950. We note that this reform is more difficult to analyze, as its effects on retirement behavior are less concentrated at a particular age. This is because the reform was announced when the first cohort of treated workers (born in 1950) was 10 years away from their SRA, and at this point the vast majority of workers were not on the cusp of retiring. Therefore, rather than causing a sharp discontinuity in the timing of early retirements, the main effect of the reform was to limit the number of retired workers at various ages prior to their SRAs.

This dynamic effect is illustrated in Figure 5, which compares the age-specific employment rates of workers born in the last cohort subject to the old regime (birth year 1949) and the first cohort subject to the new regime (birth year 1950), conditional on employment at age 61 years and 0 months.[34] After matching the 1949 cohort's declining employment rates until around age 62, we observe a relatively gradual decline in the 1950 cohort's employment rate, reflecting higher rates of early retirement at each age among the 1949 cohort. This meant that a higher proportion of workers in the 1950 cohort remained employed until the SRA ($\sim$40%) than the 1949 cohort ($\sim$32%). Remarkably, beyond the age of 65 years and 6 months, the two employment rates are largely indistinguishable. This similarity suggests that the different dynamics at earlier ages are likely to reflect the different retirement incentives of each cohort, rather than underlying differences in preferences or characteristics.

[34] Supplementary analyses reveal very similar employment dynamics for the two cohorts prior to this age.

7.1 Threats to identification

One of the key issues for empirical analyses of this reform is that the groups of control and treated workers who remain employed until their SRAs are likely to be different from each other. For example, at the age 64.5 the treated group will contain a substantial fraction of workers who would have retired earlier, if not for the 2006 reform. This means that any comparisons of worker or firm outcomes are likely to be subject to substantive selection biases.

To ensure that our analyses are not biased by this selectivity, we have to select the focal workers well ahead of their retirements and analyse their responses over the subsequent years (covering the period of early retirements and also a reasonable number of years following their SRAs). In line with Figure 5, we select our focal workers to be the ones who are employed at age 61. We identify the firms at which these workers are employed, and we follow the outcomes of these firms until the focal workers reach the age of 68 (regardless of whether they remain employed by these firms over the period of observation).

One of the main advantages of the 2006 reform is that it affected workers born relatively early, which means that we can study the outcomes of interest over longer time spans (recall that our analyses of the 2011/12 reforms end at the age of 66). The disadvantage of this reform is that its effects are likely spread over the observation period (especially in the years that precede the SRAs), which makes it challenging to attain statistically significant estimates. The magnitude of the estimates is also negatively affected by worker attrition.[35] In addition, because the effects on early retirement are more spread out, the interpretation of treatment effects may be challenging (e.g., the coefficient estimates at a given age may be influenced by retirement behavior at many different ages).

These problems are compounded with data issues that prevent us from imposing a set of sample selection criteria that would be comparable to our principal estimation sample corresponding to the 2011/12 reforms. Critically, we cannot restrict our attention to workers with stable attachments to their firms, because the employment histories preceding age 61 are incomplete and subject to data quality issues. Further, we have found that normalizing the treatment and outcome variables by the size of firms at a pre-specified age is problematic over longer

[35] That is, a large share of focal workers will leave the labor force for reasons unrelated to the reform ("always takers"), which means that the reform effects are identified by a small subset of firms in the estimation sample, with most firms not being subject to the reform-induced career extensions.

periods of observation, since many firms are subject to large changes in size over seven years.

For these reasons, we have decided to deviate from our baseline analysis and restrict our attention to small firms (5–30 employees). This restriction enables us to evaluate the reform-induced dynamics in firms where the focal workers represent a substantive share of their total workforce, which reduces the need to scale the treatment and outcome variables by workforce size. The inevitable limitation of this approach is that the corresponding sample size is smaller than in our principal analysis, comprising 5,450 firms with workers born in 1949, and 5,809 firms with workers born in 1950.

7.2 Firm-level responses

To evaluate the causal effects of the 2006 reform on firms and co-workers, we use a simplified version of the principal model specification:

$$y_{it} = \text{age}_{it} + \text{treated}_i + \sum_{j=61,q1}^{67,q4} \beta_j(\text{treated}_i \times \mathbf{1}(\text{age}_{it} = j)) + \text{time}_t + \epsilon_{it} \tag{6}$$

The model does not use the pair notation since we have a single pair of treated and control groups. The age-specific treatment effects cover a longer time span, starting at the time when the focal worker's age is 61 years and 1 quarter, and ending when the focal worker's age is 67 years and 4 quarters (i.e., on their 68^{th} birthdays). The treated dummy absorbs any time-invariant differences between the treated and the control group, and it is identified by the treatment effect in the reference period (corresponding to focal worker's 61^{st} birthdays.).

The results are presented in a series of charts plotting the age-specific treatment effects, corresponding to the key outcomes of interest. First, we evaluate firm size (Figure A4a). While the coefficient do not attain statistical significance, the chart indicates that over the period characterized by early retirements (61–65), the treated firms become larger than the control firms. This is an interesting finding, since it contrasts the dynamics associated with the 2011/12 reforms, which indicated that the treated firms become relatively smaller ahead of the SRA (as control firms commence their hiring of replacement workers earlier). Here we see the opposite effect, which might relate to the fact that early retirements might be harder to anticipate than the retirements at the SRA. This is because early retirements are often linked to health shocks. Because of this, control firms may not commence replacement hiring ahead of the early retirement

events, leading to a temporary drop in their workforce size. Importantly, we see that this trend breaks at the time of the SRA, at which point the treated firms become smaller than the control firms. This reflects a larger share of treated workers retiring 'on time'. However, according to the treatment effects observed at ages 66 and 67, this decline appears to be only temporary.

These dynamics are reflected in firm-level hiring decisions (Figure A4b), which appear to be delayed in the treated firms. Between the ages 61 and 65, the hiring incidence is lower in the treated firms than in the control firms (although the individual coefficients are once again not statistically significant). At the SRA, the treated firms are subject to a short-lived spike of hiring, after which the treatment effects return to their former levels. Overall, the level of hiring appears to be lower in the treated firms, which suggests that the reform may have limited the employment opportunities for job seekers.

We also see that coworkers of treated workers earn less money than coworkers of control workers (Figure A5a), which is attributable to them working fewer hours (Figure A5b), although there is a possibility that their wage growth is delayed as well (Figure A5c). In terms of the overall labor costs (Figure A6a), all factors combined imply that treated firms are subject to higher labor costs over the period preceding the SRA, and lower labor costs over the period following the SRAs. However, based on the observed dynamics of the firm size, it can be expected that the overall labor cost levels in treated and control firms will eventually converge as well.

While these results are more suggestive and less precisely estimated than our principal analysis of the 2011/12 reforms, they do imply similar responses from older workers and their firms. First, both reforms have been effective in extending the careers of older workers. Second, these career extensions have been incorporated into firms' hiring decisions, making them delay their hiring and hire fewer workers overall. Third, as a result of the reforms, co-workers earn less money, which is largely attributable to them working fewer hours.

8 International relevance and comparisons

In this section, we place the Dutch pension reforms into a broader international context. In particular, we contrast the Dutch institutional and economic setting to Italy, as nearly all of the existing evidence on the effects of retirement reforms on firms and coworkers is based on the

2011 reform of Italian pension system. We also discuss the implications of our results for other countries.

Theoretical insights. In order to determine which economic and institutional factors may be relevant for firms' responses to pension reforms, it is helpful to start the discussion with some theoretical insights. At the extensive margin, the extent to which a delay in pension eligibility represents a shock to the firms' labor demand depends on the presence of wage rigidities and on their ability to adjust the stock of workers — both the stock of older workers that are directly affected by the policy and also their younger co-workers. Boeri, Garibaldi and Moen (2021) show that, if there are no wage rigidities, wages of workers in treated firms will be adjusted downwards, assuming the marginal product of workers falls as the stock of workers who remain attached to the firm increases.[36] However, in the presence of downward wage rigidities, firms may lay off older, more expensive workers so that the hiring of younger workers remains unaffected. Older workers' labor supply may also have rigidities, though, due to strict employment protection legislation. In this case, where both wages and labor supply are rigid, firms may hire fewer younger workers and possibly increase layoffs of younger workers, since they are less likely to be covered by employment protections (Boeri, Garibaldi and Moen, 2021).

At the intensive margin, Bianchi et al. (2021) show that in firms with limited opportunities for career advancement, a forced expansion of the stock of older workers will lead to career spillovers on their younger co-workers. Firms where the career capacity is likely to be most constrained are firms with relatively small shares of high-level jobs and slow-growing firms. These factors are likely to affect the magnitude of reform effects on career progressions and wage growth of younger coworkers.

Policy context. The so-called Fornero-Monti pension reform, on which all evidence from Italy is based, was legislated by a new government as part of a package of emergency measures in December 2011. These measures were taken under the pressure of markets and international organizations in the middle of a public debt crisis (Boeri, Garibaldi and Moen, 2021) and were clearly unexpected. Moreover, the pension reform was extremely sudden and impactful. It took effect less than one month after it was presented to the Parliament (Bianchi et al., 2021) and it

[36]The negative effects on workers' marginal products also depend on the degree of complementarity between older and younger workers. If the two are to some extent complements, a larger stock of senior workers may increase the marginal productivity of younger workers.

substantially increased pension eligibility ages. Indeed, the reform raised the full retirement age by up to 6 years for workers who were otherwise weeks from retirement. Moreover, the reform in Italy disproportionately affected some categories of workers depending for example on gender and years of contribution to social security (women in particular were affected much more than men).

In contrast, the Dutch 2011/12 reforms were not implemented suddenly (despite being unexpected when announced) nor during political turmoil; they affected all workers regardless of their labor market histories; and established an increase in the SRA of 2 years, to be reached gradually by 2024 (13 years after announcement) with incremental increases in the eligibility age of at most 4 months. In this sense, the Dutch case has the features of a "typical" pension reform in the context of OECD countries. Compared to the Italian case, its timing likely allowed for smoother adjustments on the employers' side.

Macroeconomic conditions. In terms of the macroeconomic context, while the Italian economy was shrinking during the implementation of the Fornero-Monti reform, the Dutch economy was growing during the implementation of the SRA reform. Over the implementation period, Italy had negative GDP growth (bottoming out in 2015) and high and increasing unemployment rates (increasing from 8.4% in 2011 to 12.7% in 2014). On the contrary, unemployment rates in the Netherlands halved over our observation period (2013–19), from 7.2% to 3.4%, and both GDP and GDP growth have been increasing. The unemployment rates in particular highlight two very different macroeconomic environments over the observation periods: even at its lowest point in 2011, the unemployment rate in Italy of 8.4% was higher than peak in the Netherlands in 2014 (7.4%).

Retirement incentives. In both countries, raising the pension eligibility age had a strong effect on older workers' employment since the vast majority of workers retire exactly when they reach eligibility. This behavior is due to different reasons, though. In Italy, it is in principle possible to continue working after reaching pension eligibility.[37] However, under the defined-benefit system applied to the cohorts affected by the reform, it was convenient to retire as soon as possible upon reaching pension eligibility. Consequently, almost 90% of workers reaching the pension eligibility threshold retired within a year (Boeri, Garibaldi and Moen, 2021; Bianchi

[37]The Fornero-Monti reform also increased the age when the employer can fire an employer without cause to 70 years and 7 months, which is later than the SRA.

et al., 2021). In the Netherlands, mandatory retirement is a common practice, and applies to virtually all private-sector employees,[38] which explains why a large share of individuals who remain employed just before the SRA retire at the SRA. We note that mandatory retirement is not uncommon in OECD countries. The United Kingdom, Denmark and Poland are the only European countries in the OECD that have abolished mandatory retirement. Outside of Europe, mandatory retirement has been abolished by Australia, Canada, New Zealand and, with some exceptions, the United States (OECD, 2017). In such countries, we may expect more gradual transitions from work to retirement, which can lead to a dilution of reform effects over a longer period of observation (akin to our analysis of the 2006 reform in the Netherlands).

Employment protection. As often argued by employers, mandatory retirement can be used as a convenient mechanism for parting with less productive workers, especially in countries where employment protection rules are rigid (OECD, 2017). In both Italy and the Netherlands, terminating the employment relation is very costly, especially for senior workers who are more likely to be covered by employment protection legislation (EPL). The Netherlands has the strongest employment protection measures in the OECD, in terms of the strictness of regulation on individual and collective dismissals, and Italy is close behind. Moreover, most studies indicate that more regulated labor markets are associated with higher downward wage rigidities, which may hamper the ability of firms to adjust to a labor supply shock. In Italy, virtually all workers are covered by collective agreements, in the sense that base wages fixed in collective agreements are functional equivalents of sectoral minimum wages for all workers, to which all firms have to comply. In the Netherlands, around four out of five workers are covered.[39] These coverage rates are much higher than the OECD average of 32%, indicating that downward wage adjustments are relatively difficult for firms in both countries.[40] In countries with more lax employment protection policies, we may expect that pension reforms will lead to more downward pressure on workers' wages, and potentially also increase the likelihood of job loss.

Part time work. Even though wages cannot be adjusted downwards, a labor supply shock could still affect co-workers by reducing opportunities for earnings growth. Earnings growth through an increase in hours worked is only possible, though, if many workers are not already

[38] In 2008, the government of the Netherlands stopped enforcing mandatory retirement on its national-level civil servants, in an attempt to promote longer working lives and set an example for other industries (Mulders, 2018).

[39] OECD statistics: "Employment protection" at `https://stats.oecd.org/` [Accessed February 23, 2022].

[40] Indeed, our data suggests that sustained wage decreases are extremely rare in the Netherlands.

working full time. This is certainly the case in the Netherlands, where part-time employment comprised 50.2% of total employment among 15–64-year-olds in 2019, more than double the European average of 21.4%. Like most countries, there is a large gender divide, with around three-quarters of female employment made up by part-time work compared to one-quarter for men.[41] These high rates of part-time work have followed several changes in collective agreements and labor law since the early 1990s that made part-time work more attractive (Picchio, Suetens and van Ours, 2018). In Italy, the share of part-time employment is similar to the European average (18.7%). Overall, these comparisons suggest that there is more room for working hours to increase in the Netherlands than Italy. We thus posit that the effects on contractual earnings growth in Italy estimated by Bianchi et al. (2021) may be more likely to reflect wage increases, although the authors do not directly estimate the effects on wages and hours worked.[42]

Upward wage rigidities. On the other side of the spectrum, upwards wage rigidities appear to be stronger in Italy than the Netherlands. In particular, survey evidence from a large group of EU countries found that less than 30% of Italian firms adjust wages annually or at higher frequency compared to 60% in the Netherlands, although both percentages have fallen since the Great Recession (Branten, Lamo and Room, 2018). Moreover, among the surveyed countries, Italy has the second-highest levels of wage rigidities (behind Greece). This is not surprising, given that the frequency of wage changes is positively related to firm size (Branten, Lamo and Room, 2018), and Italian firms are half the European average (Giovannetti, Ricchiuti and Velucchi, 2011). Because large firms are more productive, more profitable and offer higher wages (Rubin et al., 2012), and because Italian firms specialize in traditional low-tech sectors characterized, in general, by lower productivity, it is also not surprising that the productivity of Italian firms is below the European average (OECD, 2021). In comparison, the Netherlands is 25% more productive than the European and OECD average. Moreover, because smaller firms have a lower probability of survival, Italian firms have a high turnover rate. Four years after birth, less than 50% of Italian firms remain, compared to around 60% in the Netherlands and 50% for the European Union.

Occupational characteristics. Finally, there are notable differences in occupations be-

[41] Eurostat, Labour Force Survey, at http://ec.europa.eu/eurostat/web/lfs/data/database [accessed February 23, 2022]

[42] Bianchi et al. (2021) show a negative effect on categorical promotions, which are likely to be associated with wage increases.

tween Italians and Dutch workers: Italy ranks among the OECD countries with the lowest share of managerial and professional positions. In 2012, its share of managerial positions was about half that of the Netherlands (3.7% versus 6.9%) and its share of professional positions about 60% (13.5% versus 22.8%).[43] This hints towards a larger span and fewer promotion opportunities in Italy.

Overall assessment. The two countries seem to have an institutional framework characterized by similarly strict labor market regulations, which concentrates the reform effects at the points of age discontinuities and limits the scope for firms' adjustment in terms of workers' wages and job terminations. At the same time, the two countries were subject to very different economic conditions. Italy had a contracting economy that may have hampered firms' ability to adjust. In contrast, the Dutch economy was growing, which may have facilitated workforce adjustments. Moreover, the higher productivity and smaller span of Dutch firms may have helped them smooth the effects of a labor supply shock. This, together with the more gradual nature of the Dutch reform, might explain why, in contrast to Bertoni and Brunello (2020), we do not observe higher firm turnover caused by the reform. Moreover, Bianchi et al. (2021) estimate a positive effect on layoffs, while we don't find any effect on coworker separations.

In terms of coworkers' earnings, Bianchi et al. (2021) find that a one standard-deviation increase in retirement delays (equivalent to 0.07 years per worker), leads to a reduction in coworkers' earnings growth of €69 per worker. In comparison, we find a decrease in coworkers' earnings growth of €52 per worker.[44] An important caveat, though, is that Bianchi et al.'s (2021) estimate is an annual estimate for wage growth across four post-reform years. In contrast, we estimate a temporary decrease in coworkers' earnings in quarter 0, which is significantly offset once treated focal workers retire in quarter 1.

Bianchi et al. (2021) also find that a one standard-deviation increase in retirement delays decreased the number of new hires by 0.069, but this estimate is not statistically significant. Our estimate of the total effect across 18 months implies a decrease in hires of 1.54 per 100 workers, which is around 0.77 per firm (assuming an average firm size of 49.8 workers).[45]

[43] OECD statistics: "World indicators of skills for employment" at `https://stats.oecd.org/` [Accessed February 23, 2022].

[44] In our setting, we are considering the effects of an SRA increment of (typically) 3 months for 10% of the firm's workforce. Thus, the retirement delay is 0.025 years per worker. Therefore, to compare our estimates to Bianchi et al. (2021), we need to multiply our estimates by 0.07/0.025=2.8.

[45] We find the decrease in hires of 1.54 by multiplying our estimate of -0.55 by 2.8, see Footnote 44.

Overall, we find negative effects of the reform on co-workers' earnings and on hiring, which is consistent with the fact that the Dutch economy was growing during the reform and that it is characterized by higher promotion opportunities and productivity than the Italian one. We also find no significant effect on hourly wages and a negative effect on earnings that appears to be fully explained by working hours. This is consistent with strong employment protection legislation and widespread collective agreements, which prevent downward wage adjustments.

9 Conclusion

We study the causal effects of longer careers on firms and coworkers. We exploit variation across firms in the Statutory Retirement Age (SRA) of its older workers, induced by a stepwise retirement reform in the Netherlands that raised the SRA across adjacent birth cohorts. Using linked employer-employee data, we show that the reform led to significant delays in the retirement decisions of directly affected workers. Firms employing these workers responded by delaying and decreasing the number of workers hired. For the coworkers of those that were affected by the reform, we do not find any effect on their separation rates from firms, but the reform lowered their earnings growth. The slower earnings growth appears to be explained by constraints on hours worked rather than by a decline in wage growth. We examine heterogeneity in the effects and find that the SRA increments lead to more sustained negative effects on hiring and promotions among firms that are contracting, while firms that are growing mainly respond by *delaying* hiring and promotions. Our findings are supported by a descriptive analysis of an earlier Dutch reform, which significantly decreased the uptake of early retirement and had qualitatively similar effects on firms and coworkers. Overall,

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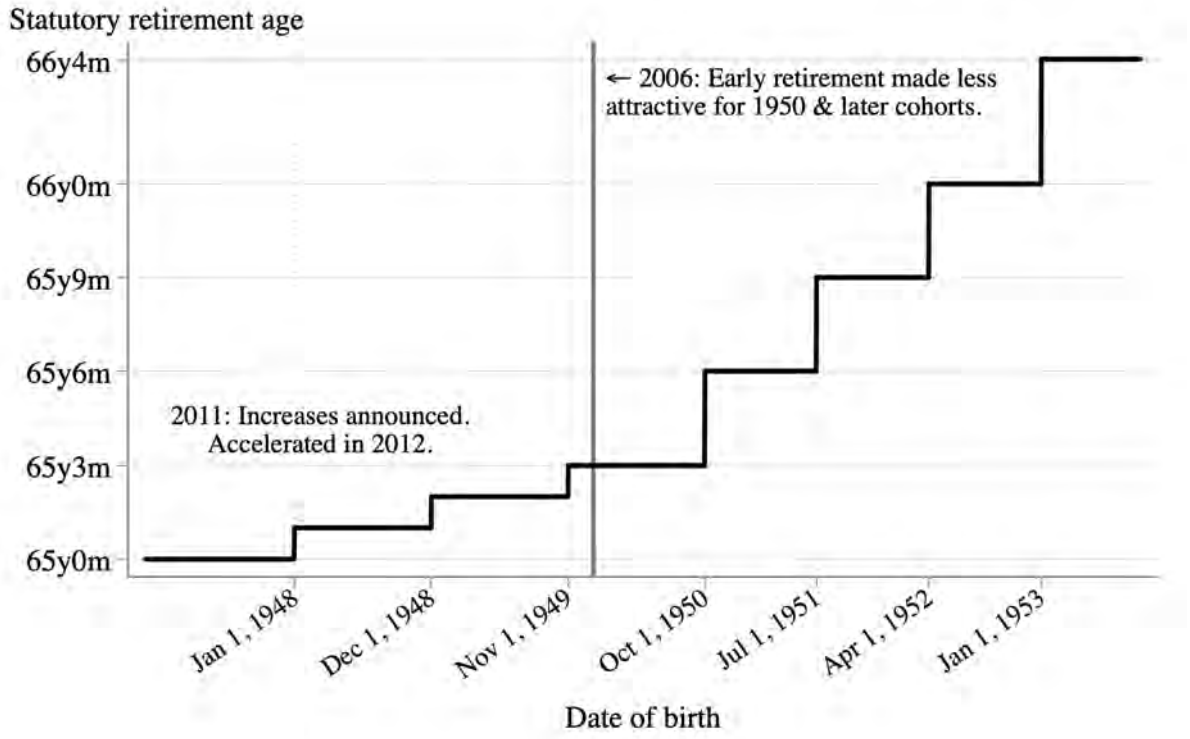
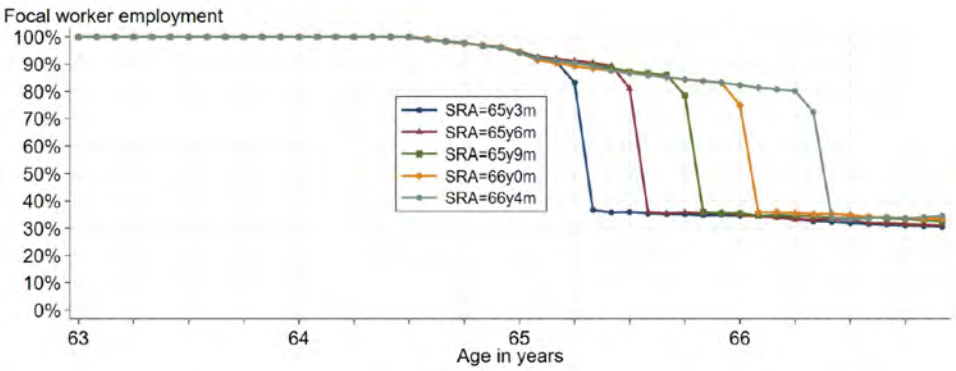
Figure 1: Retirement rules by cohort: Statutory retirement age and early retirement generosity



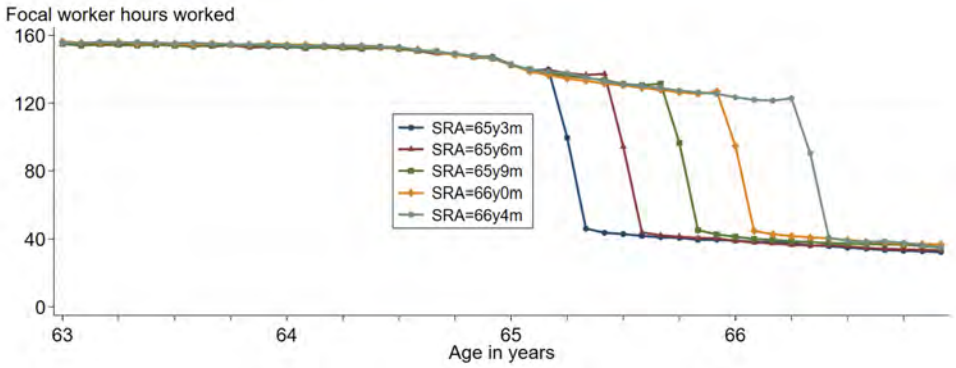
Notes: This figure shows key differences in the retirement rules of different birth cohorts in the Netherlands. We focus on increases in the Statutory Retirement Age (SRA) from 65 years, 3 months to 66 years, 4 months. These step-wise increases in the SRA of 3 or 4 months occurred as a result of reforms in 2011 and 2012. We focus on older workers born between January 1950 and September 1953, as earlier birth cohorts had access to a much more generous early-retirement scheme. Accordingly, relatively few individuals in these cohorts remained employed up until the SRA.

Figure 2: Labor supply patterns of focal workers by age and cohort

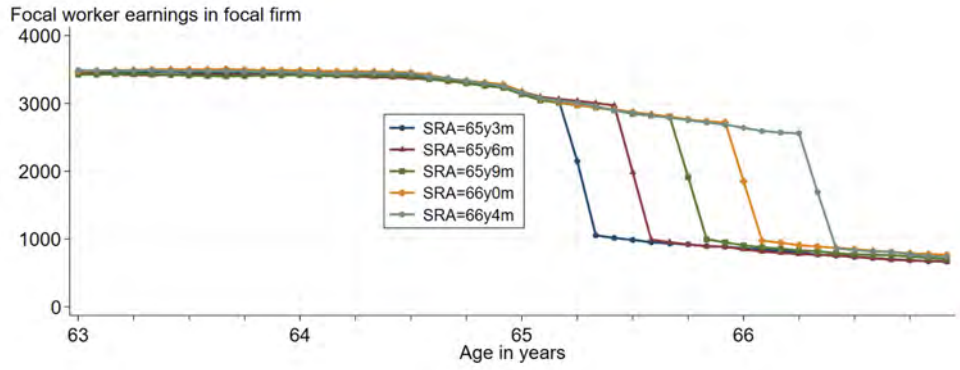
(a) Employment



(b) Monthly contractual work hours



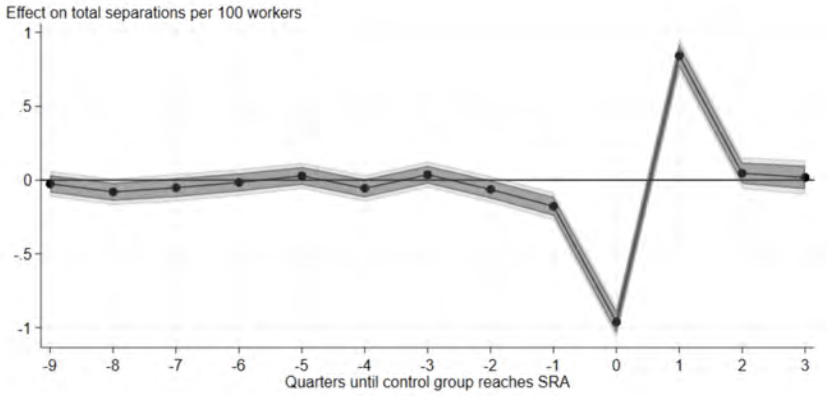
(c) Monthly contractual earnings



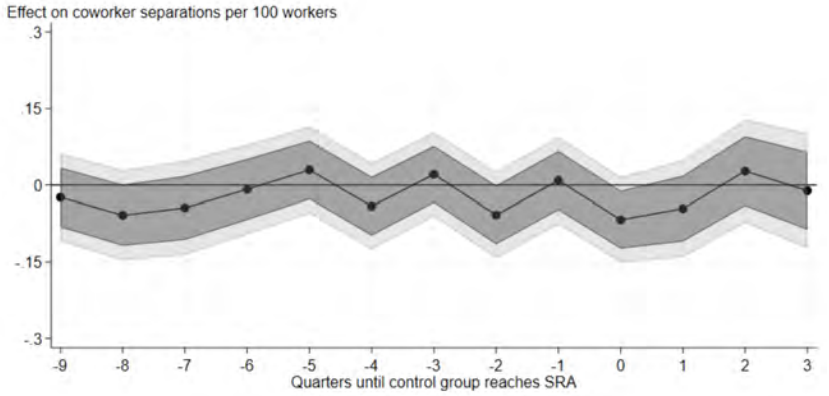
Notes: These figures shows differences in the age-specific labor supply trends of older Dutch workers by birth cohort. We divide individuals born between January 1950 and September 1953 into five nine-month birth cohorts, matching the variation in the Statutory Retirement Age (SRA) from 65 years, 3 months to 66 years, 4 months. We show the labor supply trends of focal workers in each cohort, who are workers with a strong attachment to a particular firm and the labor market in general at ages 63 to 64.5 years. We observe similar labor supply trends across cohorts up until age 65 years and 3 months, when differences in eligibility take effect. Labor supply trends then diverge considerably, with large declines among workers reaching the SRA. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure 3: Effects of raising the statutory retirement age on affected firms' job flows

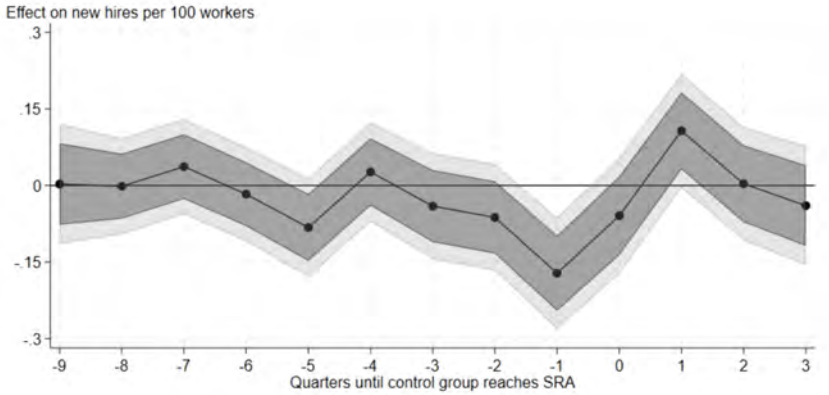
(a) Total separations from firm



(b) Coworker separations from firm



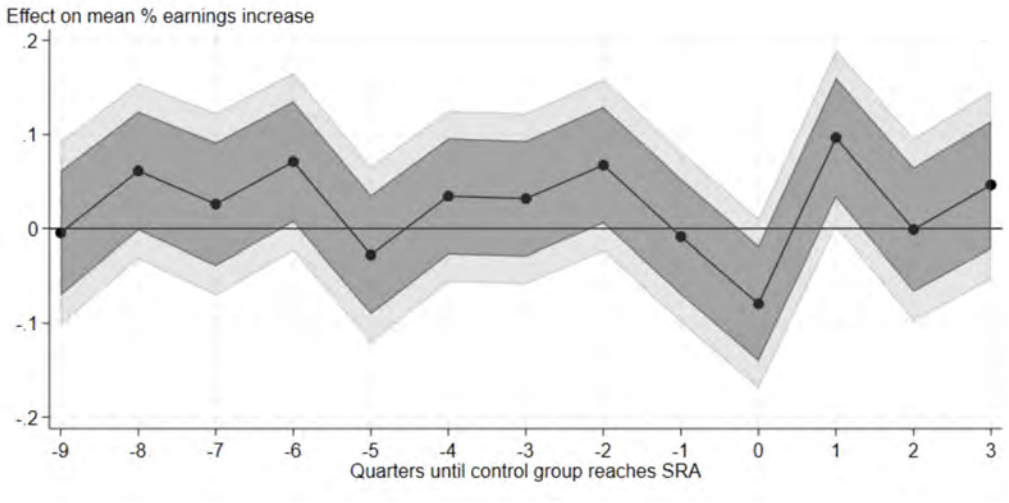
(c) New hires to firm



Notes: These figures show the estimated effects of an incremental increase in the Statutory Retirement Age (SRA) of 3–4 months on job flows at affected older workers' firms. The estimates come from equation (4), which implements our cohort-pairs differences-in-differences identification strategy. The event-time estimates on (the firms of) treated workers are relative to workers in the previous nine-month SRA cohort (control group). We present conventional 95% confidence intervals in dark gray, and more conservative confidence intervals in light gray that account for the 13 hypothesis tests in each regression via a Bonferroni correction. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure 4: Effects of raising the statutory retirement age on coworkers' earnings growth

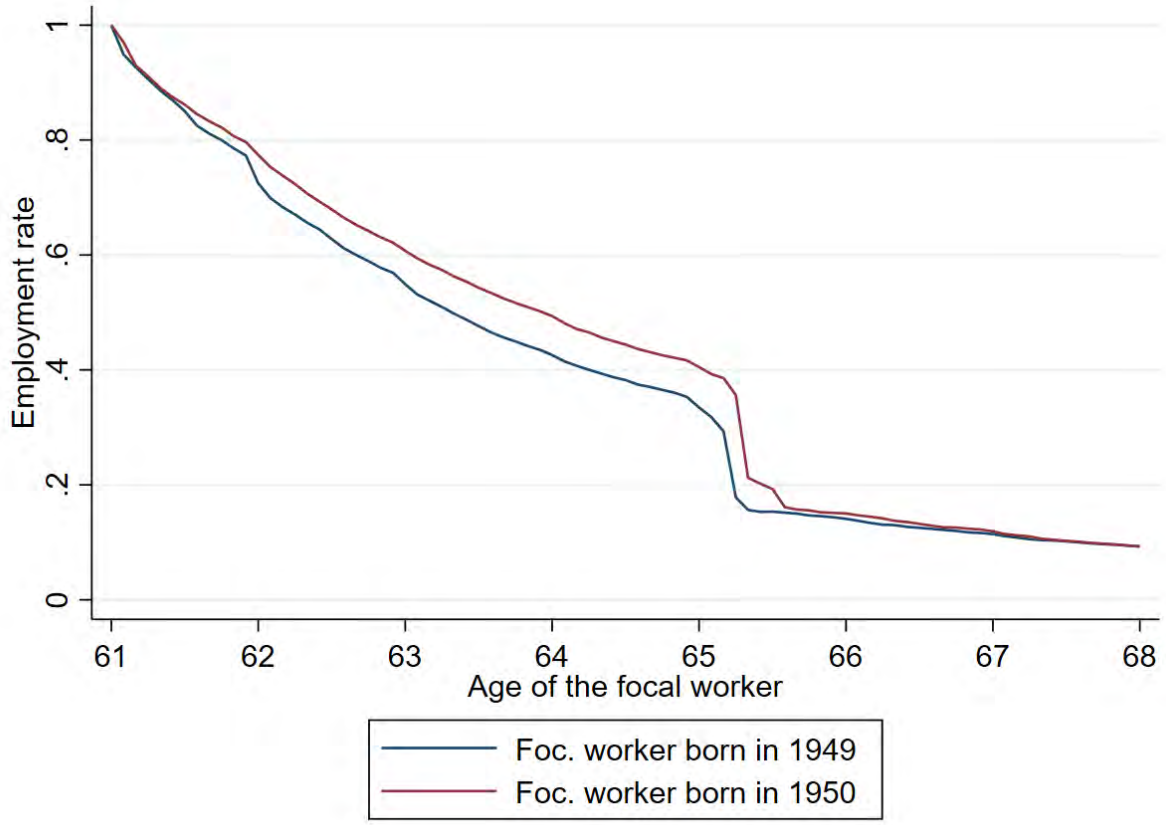
(a) Mean percentage point change in coworkers' earnings



(b) Rate of coworker promotions (sustained 20% increase in monthly earnings)

Notes: These figures show the estimated effects of an incremental increase in the Statutory Retirement Age (SRA) of 3–4 months on the earnings growth and promotion rates of coworkers. The estimates come from equation (4), which implements our cohort-pairs differences-in-differences identification strategy. The event-time estimates on (the set of coworkers of) treated workers are relative to workers in the previous nine-month SRA cohort (control group). We present conventional 95% confidence intervals in dark gray, and more conservative confidence intervals in light gray that account for the 13 hypothesis tests in each regression via a Bonferroni correction. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure 5: Labor supply patterns of focal workers from birth cohorts 1949 and 1950, by age



Notes: This figure shows age-specific labor supply trends of older Dutch workers born in years 1949 and 1950 who were observed to be employed at the age of 61. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table 1: Summary statistics at age 64.5, main sample

	Mean (1)	Std. Dev. (2)
Focal worker characteristics		
Age in years	64.5	0
Employed	100%	0
Monthly contractual work hours	153	32
Monthly contractual earnings	€3,418	€1,780
Contractual hourly wage	€22.3	€11.7
Share male	79.3%	
Firm/coworker characteristics		
Number of workers	49.8	42.8
Young workers (age 20–34)	12.5	13.2
Middle-age workers (age 35–49)	18.3	17.8
Older workers (age 50+)	19.0	17.9
Focal workers	2.4	2.1
Total monthly contractual wage costs	€152,874	€154,202
No. of separations per month per 100 workers	1.19	2.56
No. of new hires per month per 100 workers	1.13	2.74
Average p.p. coworker earnings increase	0.93	4.11
Average p.p. coworker hours increase	0.92	5.70
Average p.p. coworker wage increase	0.47	4.20
No. of coworkers with 20% earnings increases per 100 workers	1.00	2.99
No. of coworkers with 20% hours increases per 100 workers	0.85	2.82
No. of coworkers with 20% wage increases per 100 workers	0.35	1.95
Percent change in combined earnings of coworkers	0.10	3.50
Percent change in combined hours of coworkers	0.05	5.33
Mean earnings of coworkers in t-1	€2,952	€929
Mean hours of coworkers in t-1	148	24
Combined earnings of stable coworkers in t-1	€134,975	€140,873
Combined hours of stable coworkers in t-1	6,516	6,203
Individuals	28,779	
Firms	17,211	

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

Table 2: Effects of SRA increments on older workers' labor supply

	Employed (1)	Monthly hours (2)	Monthly earnings (3)
Quarters above SRA			
-9 to -3	Omitted reference period		
-2	-0.003	-0.7	-24
	(0.001)	(0.2)	(6)
-1	-0.002	-1.3	-19
	(0.002)	(0.3)	(7)
0	0.345	67.0	1,461
	(0.003)	(0.5)	(13)
1	0.176	26.7	500
	(0.002)	(0.4)	(9)
2	0.006	1.5	28
	(0.003)	(0.3)	(10)
3	0.005	0.9	17
	(0.003)	(0.3)	(10)
Total effect	1.580	282.4	5,890
	(0.030)	(3.9)	(114)
Fixed effects			
Pair-by-age	X	X	X
Month-year			
Focal worker			
R-squared	0.451	0.503	0.270
Observations	2,165,070	2,165,070	2,165,070

Notes: This table presents estimates from equation (3) of the effects of SRA increments of 3–4 months on the labor supply of older workers. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table 3: Effects of SRA increments on firms' job flows, workforce size and labor costs

	Separations and hires per 100 workers			Log of firm size/labor costs	
	Separations (total) (1)	Separations (coworkers) (2)	Hires (total) (3)	Firm size (4)	Labor costs (5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	-0.042	-0.042	-0.056	-0.004	-0.001
	(0.033)	(0.031)	(0.039)	(0.001)	(0.001)
-1	-0.155	0.027	-0.165	-0.007	-0.004
	(0.033)	(0.029)	(0.038)	(0.001)	(0.001)
0	-0.937	-0.047	-0.053	0.016	0.033
	(0.038)	(0.029)	(0.040)	(0.002)	(0.002)
1	0.869	-0.023	0.115	0.010	0.012
	(0.040)	(0.033)	(0.039)	(0.002)	(0.002)
2	0.069	0.053	0.012	-0.003	0.001
	(0.037)	(0.035)	(0.039)	(0.002)	(0.002)
3	0.040	0.014	-0.034	-0.005	-0.002
	(0.041)	(0.039)	(0.040)	(0.002)	(0.002)
Total effect	-0.470	-0.050	-0.550	1.9%	11.9%
	(0.240)	(0.220)	(0.250)	(2.500)	(2.700)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.117	0.122	0.095	0.988	0.990
Observations	2,165,070	2,165,070	2,165,013	2,165,070	2,165,070

Notes: This table presents estimates from equation (5) of the effects of SRA increments of 3–4 months on the job flows, firm size and labor costs of affected older workers' firms. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table 4: Effects of SRA increments on coworkers' earnings, hours and wage growth

	Mean growth			Promotions per 100 workers		
	Earnings (1)	Hours (2)	Wages (3)	Earnings (4)	Hours (5)	Wages (6)
Quarters above SRA						
-9 to -3	Omitted reference period					
-2	0.038	-0.002	0.064	0.066	0.061	0.012
	(0.034)	(0.038)	(0.022)	(0.034)	(0.032)	(0.020)
-1	-0.035	0.039	-0.047	-0.049	-0.030	-0.014
	(0.031)	(0.034)	(0.019)	(0.034)	(0.030)	(0.017)
0	-0.105	-0.078	0.003	-0.186	-0.157	-0.035
	(0.031)	(0.034)	(0.018)	(0.037)	(0.035)	(0.019)
1	0.070	0.064	0.046	0.057	0.072	-0.024
	(0.032)	(0.036)	(0.020)	(0.038)	(0.035)	(0.022)
2	-0.032	-0.019	0.014	0.044	0.054	-0.010
	(0.034)	(0.038)	(0.022)	(0.036)	(0.034)	(0.022)
3	0.012	0.040	-0.015	-0.027	-0.041	-0.011
	(0.035)	(0.039)	(0.022)	(0.036)	(0.033)	(0.023)
Total effect	-0.160	0.130	0.190	-0.280	-0.120	-0.250
	(0.210)	(0.230)	(0.140)	(0.270)	(0.220)	(0.140)
Fixed effects						
Pair-by-age	X	X	X	X	X	X
Month-year	X	X	X	X	X	X
Focal worker	X	X	X	X	X	X
R-squared	0.104	0.242	0.336	0.249	0.185	0.091
Observations	2,164,783	2,164,787	2,164,783	2,165,070	2,165,070	2,165,070

Notes: This table presents estimates from equation (5) of the effects of SRA increments of 3–4 months on the earnings, hours and wage growth of the coworkers of affected older workers. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

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

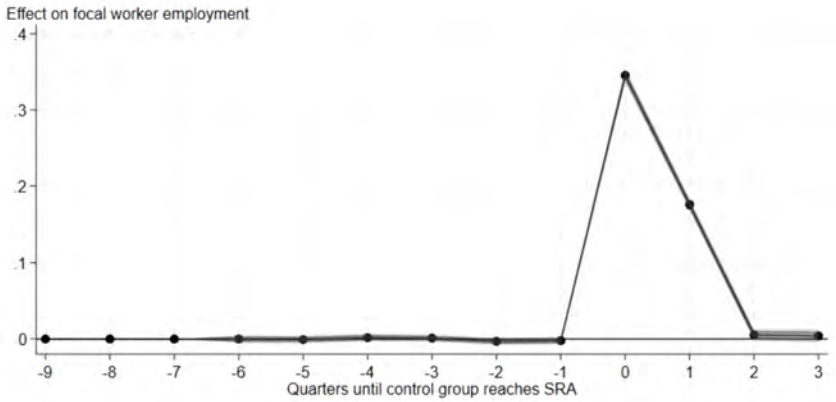
Irene Ferrari Jan Kabátek Todd Morris

List of Figures

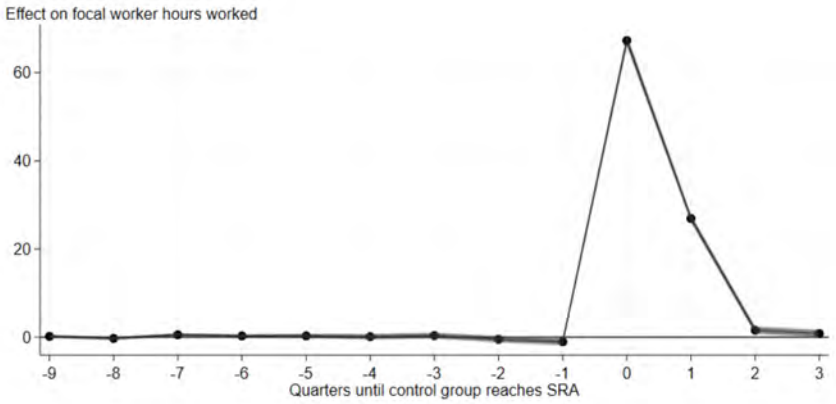
List of Tables

Figure A1: Effects of raising the statutory retirement age on focal workers' labor supply

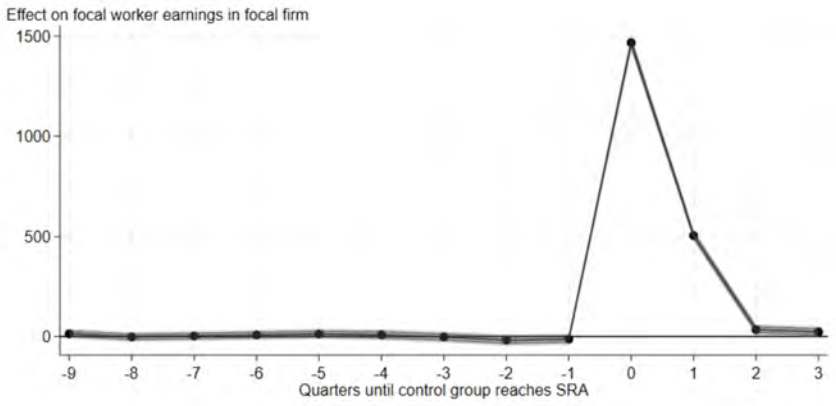
(a) Employment



(b) Monthly hours worked



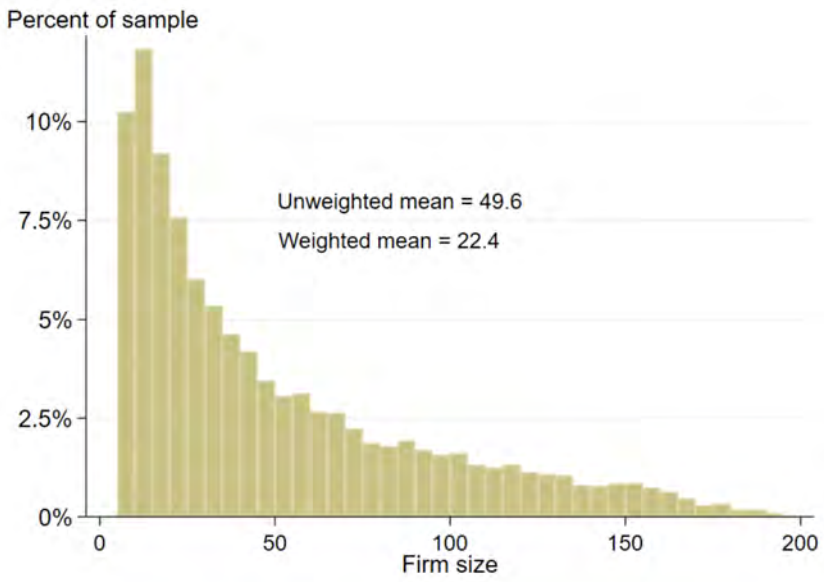
(c) Monthly earnings at focal firm



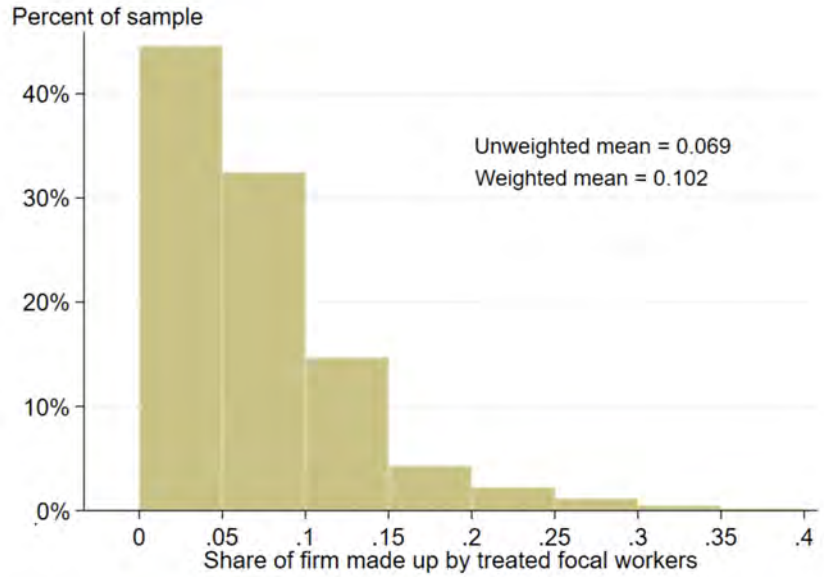
Notes: These figures show the estimated effects of an incremental increase in the Statutory Retirement Age (SRA) of 3–4 months on the labor supply of affected older workers. The estimates come from equation (2), which implements our cohort-pairs differences-in-differences identification strategy. The event-time estimates on treated workers are relative to workers in the previous nine-month SRA cohort (control group). We present conventional 95% confidence intervals in dark gray, and more conservative confidence intervals in light gray that account for the 13 hypothesis tests in each regression via a Bonferroni correction. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A2: Distribution of firm size and the firms' share of treated focal workers

(a) Firm size



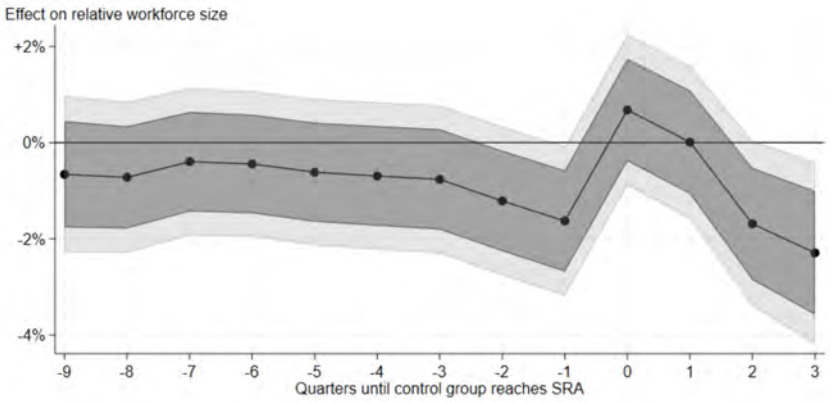
(b) Share of firm made up by treated focal workers



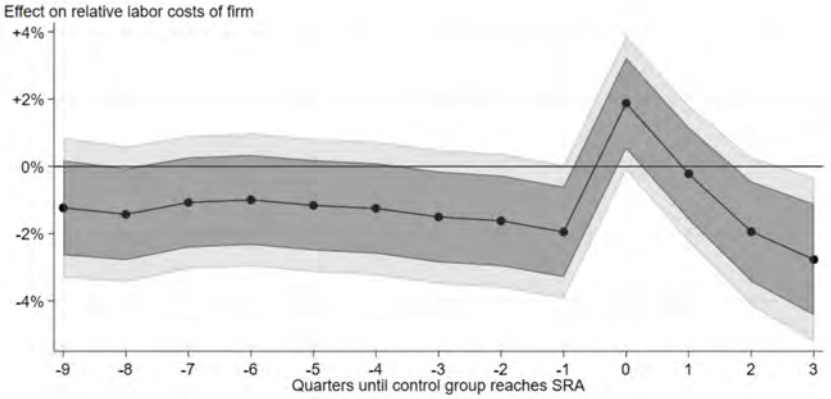
Notes: These figures show the distribution of firm size and firms' share of treated focal workers in the sample. The unit of observation is the focal worker, as in the regressions. We define treated focal workers as workers in cohorts 2–5, who are exposed to at least one SRA increment of 3–4 months. "Weighted mean" denotes the mean of the variable when focal workers' observations are weighted by the inverse of firm size. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A3: Effects of raising the statutory retirement age on firms' size and labor costs

(a) Firm size



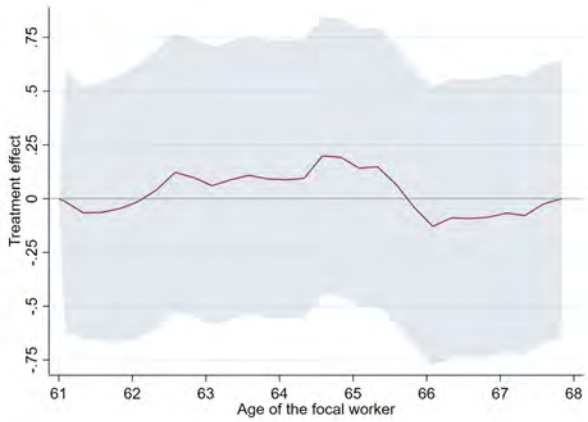
(b) Labor costs



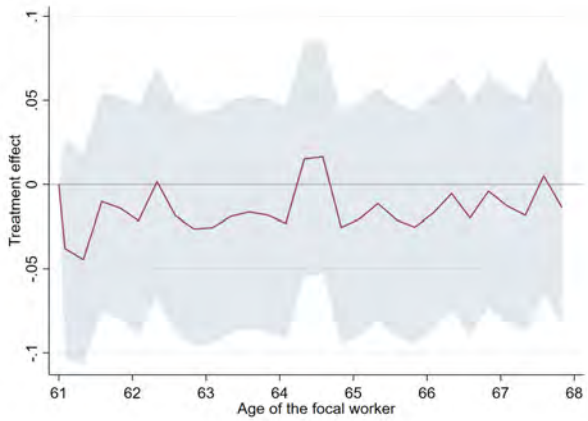
Notes: These figures show the estimated effects of an incremental increase in the Statutory Retirement Age (SRA) of 3–4 months on the size and total labor costs of affected older workers' firms. The estimates come from equation (4), which implements our cohort-pairs differences-in-differences identification strategy. The event-time estimates on treated workers are relative to workers in the previous nine-month SRA cohort (control group). We present conventional 95% confidence intervals in dark gray, and more conservative confidence intervals in light gray that account for the 13 hypothesis tests in each regression via a Bonferroni correction. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A4: Effects of the 2006 reform on firm outcomes

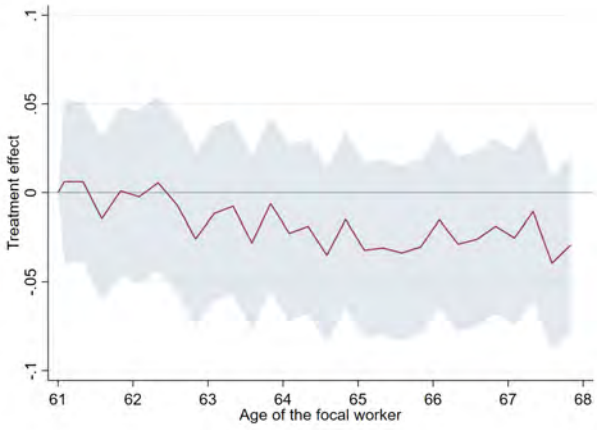
(a) Firm size



(b) New hires to firm



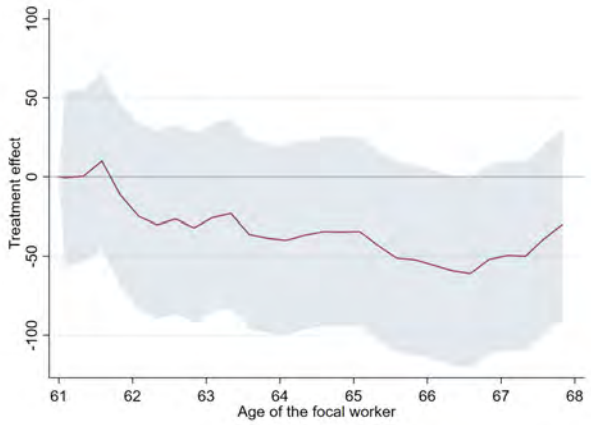
(c) Coworker separations from firm



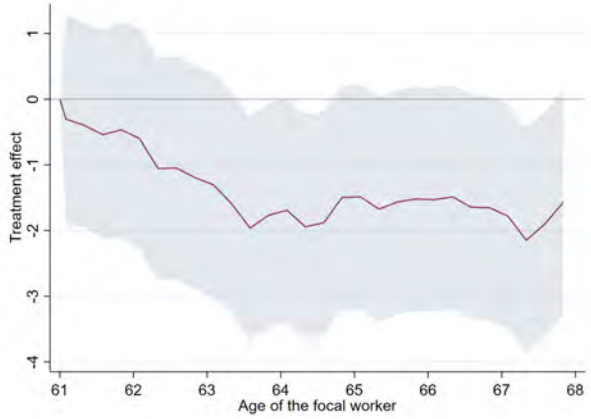
Notes: These figures show the estimated effects of the 2006 reform that made early retirement less generous for workers born in or after 1950. The estimates come from equation (6), which implements a differences-in-differences identification strategy. Given the lower power of the corresponding models, we only present conventional 95% confidence intervals. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A5: Effects of the 2006 reform on coworker outcomes, cont'd

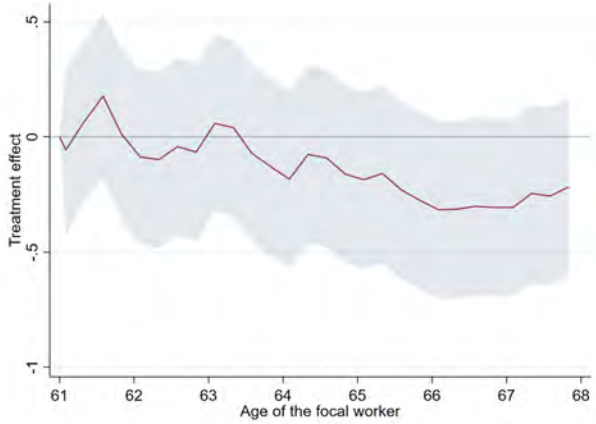
(a) Monthly earnings



(b) Hours of work



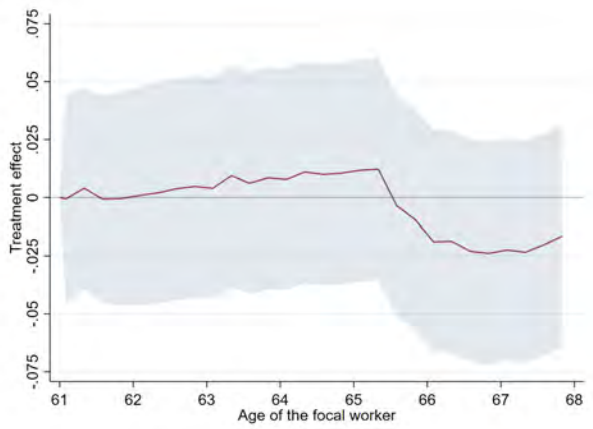
(c) Hourly wages



Notes: These figures show the estimated effects of the 2006 reform that made early retirement less generous for workers born in or after 1950. The estimates come from equation (6), which implements a differences-in-differences identification strategy. Given the lower power of the corresponding models, we only present conventional 95% confidence intervals. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Figure A6: Effects of the 2006 reform on coworker outcomes, cont'd

(a) Total labor costs (logarithmic scaling)



Notes: These figures show the estimated effects of the 2006 reform that made early retirement less generous for workers born in or after 1950. The estimates come from equation (6), which implements a differences-in-differences identification strategy. Given the lower power of the corresponding models, we only present conventional 95% confidence intervals. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A1: Balance across treated and control cohorts in pre-period, ages 63 to 64.5, main sample

	Difference: Treatment *minus* control	Control mean
Focal worker labor supply		
Employed	0	1
	(0)	
Hours worked	0.2	154.1
	(0.1)	
Earnings at focal firm	7	3.44
	(8)	
Firm size, labor costs and job flows		
Number of workers	0.6	49.0
	(0.2)	
Total labor costs per month	3,089	€148,788
	(782)	
Monthly hires per 100 workers	0.068	1.012
	(0.005)	
Monthly separations per 100 workers	0.045	1.061
	(0.005)	
Monthly growth in coworkers' earnings, hours and wages		
Average earnings growth (p.p.)	-0.020	0.944
	(0.006)	
Average hours growth (p.p.)	-0.068	1.000
	(0.007)	
Average wage growth (p.p.)	-0.025	0.513
	(0.003)	
Coworker promotions: Sustained 20% increases per 100 workers		
Earnings	-0.010	1.032
	(0.008)	
Hours	-0.000	0.875
	(0.007)	
Wages	-0.009	0.336
	(0.003)	
Percent change in combined coworker earnings/hours		
Earnings	-0.009	0.089
	(0.002)	
Hours	-0.033	0.074
	(0.003)	

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 simple regression with a single treatment dummy and pair fixed effects. Standard errors in parentheses are clustered by firm. The estimates show that, while there are some statistically significant differences between treated and control workers, these differences are small compared to the means of each variable and unlikely to lead to different *trends* for treated and control groups. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A2: Effects of SRA increments on combined earnings and hours of coworkers

	Percentage point change in combined	
	Earnings	Hours
	(1)	(2)
Quarters above SRA		
-9 to -3	Omitted reference period	
-2	0.069	0.017
	(0.030)	(0.033)
-1	-0.052	-0.002
	(0.027)	(0.029)
0	-0.077	-0.058
	(0.027)	(0.028)
1	0.069	0.025
	(0.029)	(0.031)
2	0.010	0.028
	(0.030)	(0.032)
3	-0.024	-0.016
	(0.029)	(0.033)
Total effect	-0.020	-0.020
	(0.160)	(0.180)
Fixed effects		
Pair-by-age	X	X
Month-year	X	X
Focal worker	X	X
R-squared	0.073	0.289
Observations	2,164,783	2,164,787

Notes: This table presents estimates from equation (5) of the effects of SRA increments of 3–4 months on the growth in the combined earnings and hours worked of stable coworkers. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A3: Effects of SRA increments on coworker promotions: Sensitivity to earnings threshold

	Sustained earnings increase of at least				
	10%	20%	30%	40%	50%
	(1)	(2)	(3)	(4)	(5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	0.086	0.066	0.038	0.029	0.034
	(0.047)	(0.034)	(0.028)	(0.024)	(0.021)
-1	-0.118	-0.049	-0.040	-0.026	-0.006
	(0.044)	(0.034)	(0.028)	(0.024)	(0.022)
0	-0.237	-0.186	-0.129	-0.086	-0.063
	(0.049)	(0.037)	(0.028)	(0.024)	(0.022)
1	0.048	0.057	0.045	0.042	0.030
	(0.050)	(0.038)	(0.029)	(0.025)	(0.022)
2	0.052	0.044	-0.011	-0.016	-0.011
	(0.050)	(0.036)	(0.029)	(0.024)	(0.022)
3	-0.081	-0.027	-0.024	-0.011	-0.023
	(0.049)	(0.036)	(0.030)	(0.025)	(0.021)
Total effect	-0.750	-0.280	-0.360	-0.200	-0.120
	(0.340)	(0.270)	(0.230)	(0.200)	(0.190)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.249	0.249	0.271	0.299	0.329
Observations	2,165,070	2,165,070	2,165,070	2,165,070	2,165,070

Notes: This table shows the robustness of the estimates on earnings-based promotions among coworkers to different thresholds for the size of the earnings increase. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A4: Effects of SRA increments on coworker demotions, based on sustained earnings decreases of given percentage

	Demotions (1)
Quarters above SRA	
-9 to -3	Omitted reference period
-2	-0.018
	(0.031)
-1	0.022
	(0.029)
0	-0.053
	(0.030)
1	-0.038
	(0.032)
2	0.049
	(0.032)
3	-0.038
	(0.033)
Total effect	-0.230
	(0.210)
Fixed effects	
Pair-by-age	X
Month-year	X
Focal worker	X
R-squared	0.201
Observations	2,165,070

Notes: This table shows the estimated effects on earnings-based demotions among coworkers. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A5: Effects of SRA increments on coworker promotions, based on absolute earnings increases

	Sustained earnings increase of at least			
	250 euro	500 euro	750 euro	1000 euro
	(1)	(2)	(3)	(4)
Quarters above SRA				
-9 to -3	Omitted reference period			
-2	0.069	0.035	0.019	0.004
	(0.040)	(0.026)	(0.019)	(0.014)
-1	-0.119	-0.044	-0.027	-0.013
	(0.036)	(0.023)	(0.016)	(0.013)
0	-0.132	-0.073	-0.037	-0.007
	(0.042)	(0.026)	(0.017)	(0.012)
1	0.040	0.015	-0.009	-0.018
	(0.043)	(0.026)	(0.018)	(0.013)
2	0.052	0.022	-0.002	-0.008
	(0.043)	(0.026)	(0.018)	(0.014)
3	-0.101	-0.011	0.011	0.002
	(0.041)	(0.025)	(0.018)	(0.013)
Total effect	-0.570	-0.170	-0.140	-0.120
	(0.280)	(0.180)	(0.120)	(0.090)
Fixed effects				
Pair-by-age	X	X	X	X
Month-year	X	X	X	X
Focal worker	X	X	X	X
R-squared	0.024	0.009	0.006	0.004
Observations	2,165,070	2,165,070	2,165,070	2,165,070

Notes: This table shows the estimated effects on earnings-based promotions among coworkers using different measures of absolute monthly earnings increase to define a promotion. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A6: Sensitivity of estimates on job flows and promotions to using fixed measure of firm size in denominator

	Job flows			Promotions per 100 workers		
	Separations (total) (1)	Separations (coworkers) (2)	Hires (total) (3)	Earnings (4)	Hours (5)	Wages (6)
Quarters above SRA						
-9 to -3	Omitted reference period					
-2	-0.053	-0.051	-0.057	0.061	0.064	0.010
	(0.036)	(0.034)	(0.039)	(0.036)	(0.034)	(0.021)
-1	-0.172	0.024	-0.186	-0.065	-0.044	-0.018
	(0.037)	(0.033)	(0.039)	(0.036)	(0.032)	(0.018)
0	-0.982	-0.039	-0.039	-0.171	-0.144	-0.027
	(0.042)	(0.032)	(0.041)	(0.039)	(0.037)	(0.019)
1	0.928	-0.016	0.140	0.067	0.086	-0.025
	(0.044)	(0.037)	(0.039)	(0.040)	(0.037)	(0.023)
2	0.072	0.056	0.005	0.046	0.051	-0.020
	(0.041)	(0.039)	(0.040)	(0.039)	(0.036)	(0.024)
3	0.013	-0.008	-0.038	-0.031	-0.055	-0.007
	(0.044)	(0.042)	(0.041)	(0.039)	(0.036)	(0.025)
Total effect	-0.580	-0.100	-0.520	-0.280	-0.130	-0.260
	(0.270)	(0.250)	(0.260)	(0.280)	(0.240)	(0.150)
Fixed effects						
Pair-by-age	X	X	X	X	X	X
Month-year	X	X	X	X	X	X
Focal worker	X	X	X	X	X	X
R-squared	0.115	0.120	0.115	0.251.000	0.186	0.90
Observations	2,165,070	2,165,070	2,165,070	2,165,070	2,165,070	2,165,070

Notes: This table shows the robustness of the estimated effects on rates of separations, hires and promotions with a fixed measure of firm size for each focal worker in the denominator of these rates. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A7: Effects on firm survival

	Firm operating (1)
Quarters above SRA	
-9 to -3	Omitted reference period
-2	-0.002
	(0.001)
-1	-0.002
	(0.002)
0	-0.001
	(0.002)
1	-0.001
	(0.002)
2	-0.001
	(0.002)
3	-0.003
	(0.002)
Total effect	-0.030
	(0.030)
Fixed effects	
Pair-by-age	X
Month-year	X
Focal worker	X
R-squared	0.673
Observations	1,576,504

Notes: This table shows the estimated effects on firm survival from equation (5). We restrict the sample to observations where the focal worker is older than 64.5 years, since all firms must survive to this age in order to be in the sample. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A8: Robustness to alternative samples: Allowing firms to grow/shrink beyond 5–200 workers

	Firm job flows			Coworkers' earnings	
	Separations (total)	Separations (coworker)	Hires (total)	Mean p.p. change	Promotions
	(1)	(2)	(3)	(4)	(5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	-0.010	0.007	-0.035	0.024	0.048
	(0.059)	(0.052)	(0.034)	(0.027)	(0.031)
-1	-0.134	0.064	-0.147	-0.008	-0.042
	(0.059)	(0.047)	(0.034)	(0.025)	(0.030)
0	-1.038	-0.162	-0.037	-0.105	-0.181
	(0.065)	(0.052)	(0.036)	(0.025)	(0.031)
1	0.931	-0.032	0.099	0.064	0.077
	(0.064)	(0.055)	(0.036)	(0.026)	(0.035)
2	0.061	0.087	0.014	-0.003	0.048
	(0.063)	(0.056)	(0.038)	(0.028)	(0.034)
3	0.244	0.152	0.009	0.026	-0.039
	(0.067)	(0.059)	(0.039)	(0.029)	(0.033)
Total effect	0.160	0.350	-0.290	-0.010	-0.270
	(0.410)	(0.350)	(0.240)	(0.170)	(0.220)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.075	0.073	0.064	0.100	0.224
Observations	2,568,755	2,568,755	2,572,876	2,564,261	2,568,755

Notes: This table shows the robustness of the main estimates from equation (5) to allowing firms to grow or shrink beyond 5–200 workers over the 48 months that the focal worker is aged 63–66 years old. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A9: Robustness to alternative samples: Sample restricted to firms with at most one worker per pair

	Firm job flows			Coworkers' earnings	
	Separations (total)	Separations (coworker)	Hires (total)	Mean p.p. change	Promotions
	(1)	(2)	(3)	(4)	(5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	-0.062	-0.062	-0.041	0.045	0.068
	(0.036)	(0.034)	(0.043)	(0.036)	(0.038)
-1	-0.153	0.016	-0.190	-0.031	-0.041
	(0.036)	(0.032)	(0.043)	(0.033)	(0.037)
0	-0.891	-0.050	-0.082	-0.111	-0.196
	(0.041)	(0.032)	(0.044)	(0.034)	(0.041)
1	0.839	-0.037	0.124	0.054	0.066
	(0.044)	(0.037)	(0.043)	(0.035)	(0.043)
2	0.065	0.035	0.009	-0.035	0.037
	(0.040)	(0.038)	(0.043)	(0.037)	(0.040)
3	0.033	-0.017	-0.041	0.029	-0.031
	(0.043)	(0.042)	(0.045)	(0.037)	(0.040)
Total effect	-0.500	-0.340	-0.660	-0.150	-0.290
	(0.270)	(0.250)	(0.300)	(0.230)	(0.320)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.112	0.116	0.087	0.095	0.246
Observations	1,288,548	1,288,548	1,288,500	1,288,283	1,288,548

Notes: This table shows the robustness of the main estimates from equation (5) to restricting the sample of focal workers to those that are the only focal worker within each cohort-pair. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A10: Sensitivity of estimated effects of SRA increments on coworkers rate of "hours promotions" to different thresholds

	Sustained hours increase of at least				
	10%	20%	30%	40%	50%
	(1)	(2)	(3)	(4)	(5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	0.070	0.061	0.032	-0.001	0.005
	(0.045)	(0.032)	(0.026)	(0.022)	(0.019)
-1	-0.053	-0.030	-0.031	-0.011	-0.013
	(0.041)	(0.030)	(0.025)	(0.021)	(0.018)
0	-0.177	-0.157	-0.087	-0.059	-0.053
	(0.046)	(0.035)	(0.026)	(0.022)	(0.020)
1	0.064	0.072	0.047	0.036	0.023
	(0.047)	(0.035)	(0.026)	(0.022)	(0.020)
2	0.056	0.054	0.017	0.011	0.023
	(0.048)	(0.034)	(0.027)	(0.022)	(0.020)
3	-0.052	-0.041	-0.027	-0.035	-0.040
	(0.048)	(0.033)	(0.027)	(0.023)	(0.020)
Total effect	-0.280	-0.120	-0.150	-0.180	-0.160
	(0.310)	(0.220)	(0.180)	(0.150)	(0.130)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.230	0.185	0.159	0.143	0.129
Observations	2,165,070	2,165,070	2,165,070	2,165,070	2,165,070

Notes: This table shows the robustness of the estimated effects on "hours promotions" — sustained increases in monthly hours worked — from equation (5) to different thresholds. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A11: Estimated effects of SRA increments on coworkers rate of "hours promotions" using absolute thresholds

	Sustained monthly hours increase of at least			
	8 hours	16 hours	32 hours	64 hours
	(1)	(2)	(3)	(4)
Quarters above SRA				
-9 to -3	Omitted reference period			
-2	0.063	0.014	0.010	-0.023
	(0.056)	(0.037)	(0.022)	(0.011)
-1	-0.029	-0.039	-0.031	-0.001
	(0.052)	(0.034)	(0.021)	(0.011)
0	-0.209	-0.121	-0.057	-0.016
	(0.056)	(0.037)	(0.022)	(0.011)
1	0.045	0.011	0.016	-0.004
	(0.053)	(0.038)	(0.023)	(0.011)
2	0.051	0.065	0.035	0.008
	(0.057)	(0.039)	(0.023)	(0.011)
3	-0.008	-0.074	-0.040	-0.024
	(0.057)	(0.036)	(0.022)	(0.012)
Total effect	-0.260	-0.430	-0.200	-0.180
	(0.420)	(0.260)	(0.150)	(0.080)
Fixed effects				
Pair-by-age	X	X	X	X
Month-year	X	X	X	X
Focal worker	X	X	X	X
R-squared	0.124	0.005	0.003	0.001
Observations	2,165,070	2,165,070	2,165,070	2,165,070

Notes: This table shows the robustness of the estimated effects on "hours promotions" from equation (5) using absolute increases in hours worked to define such promotions. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A12: Estimates for main outcomes up to quarter +6

	Firm job flows			Coworkers' earnings	
	Separations (total) (1)	Separations (coworker) (2)	Hires (total) (3)	Mean p.p. change (4)	Promotions (5)
Quarters above SRA					
-9 to -3	Omitted reference period				
-2	-0.041	-0.040	-0.053	0.038	0.069
	(0.033)	(0.031)	(0.039)	(0.034)	(0.034)
-1	-0.155	0.027	-0.162	-0.038	-0.049
	(0.033)	(0.029)	(0.038)	(0.031)	(0.034)
0	-0.938	-0.049	-0.050	-0.111	-0.188
	(0.038)	(0.029)	(0.040)	(0.031)	(0.037)
1	0.866	-0.026	0.116	0.063	0.050
	(0.040)	(0.033)	(0.039)	(0.033)	(0.039)
2	0.070	0.048	0.011	-0.038	0.033
	(0.037)	(0.035)	(0.039)	(0.034)	(0.036)
3	0.042	0.011	-0.032	0.005	-0.041
	(0.041)	(0.039)	(0.040)	(0.035)	(0.036)
4	0.012	0.042	0.037	-0.062	-0.035
	(0.045)	(0.041)	(0.046)	(0.036)	(0.041)
5	0.063	0.043	0.036	-0.009	0.032
	(0.057)	(0.054)	(0.054)	(0.045)	(0.051)
6	-0.064	-0.038	-0.151	0.031	-0.036
	(0.089)	(0.081)	(0.077)	(0.065)	(0.076)
Total effect (qtrs -2 to +3)	-0.470	-0.090	-0.510	-0.240	-0.380
	(0.240)	(0.230)	(0.260)	(0.210)	(0.270)
Fixed effects					
Pair-by-age	X	X	X	X	X
Month-year	X	X	X	X	X
Focal worker	X	X	X	X	X
R-squared	0.014	0.007	0.011	0.036	0.005
Observations	2,165,070	2,165,070	2,165,013	2,164,783	2,165,070

Notes: This table shows the estimates for the main outcomes from equation (5) up to quarter 6. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A13: Heterogeneity by growth in firm size

	Hire rates		Mean earnings growth		Coworker promotions		Log (firm size)	
	Shrinking	Other	Shrinking	Other	Shrinking	Other	Shrinking	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quarters above SRA								
-9 to -3	Omitted reference period							
-2	-0.151	-0.004	0.006	0.059	0.088	0.057	-0.006	-0.001
	(0.069)	(0.047)	(0.060)	(0.041)	(0.065)	(0.040)	(0.002)	(0.001)
-1	-0.130	-0.183	-0.071	-0.015	-0.120	-0.011	-0.008	-0.005
	(0.066)	(0.046)	(0.055)	(0.037)	(0.065)	(0.038)	(0.003)	(0.002)
0	0.022	-0.095	-0.067	-0.125	-0.105	-0.229	0.016	0.018
	(0.067)	(0.050)	(0.054)	(0.038)	(0.063)	(0.045)	(0.003)	(0.002)
1	0.072	0.135	-0.022	0.120	-0.036	0.108	0.011	0.012
	(0.063)	(0.049)	(0.054)	(0.040)	(0.062)	(0.049)	(0.003)	(0.002)
2	-0.024	0.029	-0.102	0.007	-0.005	0.072	-0.008	0.002
	(0.065)	(0.048)	(0.059)	(0.042)	(0.067)	(0.043)	(0.003)	(0.002)
3	-0.097	-0.004	-0.040	0.041	-0.027	-0.026	-0.008	-0.001
	(0.065)	(0.052)	(0.061)	(0.042)	(0.067)	(0.042)	(0.004)	(0.003)
Total effect	-0.920	-0.370	-0.880	0.260	-0.620	-0.090	-0.010	0.080
	(0.410)	(0.320)	(0.360)	(0.250)	(0.520)	(0.300)	(0.040)	(0.030)
Fixed effects								
Pair-by-age	X	X	X	X	X	X	X	X
Month-year	X	X	X	X	X	X	X	X
Focal worker	X	X	X	X	X	X	X	X
R-squared	0.104	0.090	0.104	0.104	0.276	0.232	0.988	0.989
Observations	808,812	1,356,201	808,769	1,356,014	808,831	1,356,239	808,831	1,356,239

Notes: This table shows heterogeneity in the estimates for the main outcomes from equation (5) based on the growth rate of firms, in terms of workforce size, over the 18 months following the focal worker's 63rd birthday. We divide firms into those that shrunk over this period and those that grew or remained the same size. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.

Table A14: Heterogeneity by firm size

	Hire rates		Mean earnings growth		Coworker promotions		Log (firm size)	
	Very small (1)	Larger (2)	Very small (3)	Larger (4)	Very small (5)	Larger (6)	Very small (7)	Larger (8)
Quarters above SRA								
-9 to -3	Omitted reference period							
-2	-0.048	-0.003	0.028	-0.037	0.052	0.064	-0.001	-0.003
	(0.047)	(0.067)	(0.039)	(0.079)	(0.042)	(0.065)	(0.002)	(0.003)
-1	-0.169	-0.130	-0.044	-0.027	-0.042	-0.082	-0.004	-0.006
	(0.047)	(0.064)	(0.035)	(0.074)	(0.043)	(0.057)	(0.002)	(0.003)
0	-0.032	-0.124	-0.120	-0.011	-0.224	-0.037	0.018	0.025
	(0.050)	(0.065)	(0.037)	(0.072)	(0.047)	(0.060)	(0.003)	(0.004)
1	0.101	0.152	0.041	0.082	0.103	-0.059	0.013	0.017
	(0.050)	(0.065)	(0.039)	(0.073)	(0.053)	(0.059)	(0.003)	(0.004)
2	0.043	-0.063	-0.040	-0.151	0.050	-0.002	0.001	-0.001
	(0.051)	(0.064)	(0.042)	(0.080)	(0.050)	(0.064)	(0.004)	(0.005)
3	-0.061	0.033	0.027	-0.145	-0.007	-0.082	-0.001	-0.000
	(0.055)	(0.069)	(0.042)	(0.084)	(0.053)	(0.064)	(0.004)	(0.006)
Total effect	-0.500	-0.400	-0.320	-0.870	-0.200	-0.590	0.080	0.090
	(0.420)	(0.490)	(0.320)	(0.530)	(0.470)	(0.500)	(0.050)	(0.060)
Fixed effects								
Pair-by-age	X	X	X	X	X	X	X	X
Month-year	X	X	X	X	X	X	X	X
Focal worker	X	X	X	X	X	X	X	X
R-squared	0.060	0.125	0.067	0.116	0.225	0.270	0.807	0.982
Observations	453,213	1,668,369	453,048	1,668,314	453,239	1,668,400	453,239	1,668,400

Notes: This table shows heterogeneity in the estimates for the main outcomes from equation (5) based on firm size. We divide firms into very small firms (5–14 workers) and larger firms (15–200 workers) based on the mean number of workers at the firm during the 48 months that the focal worker is aged 63–66 years old. Standard errors in parentheses are clustered by firm. The sample is constructed using monthly administrative data on the universe of employment spells from Statistics Netherlands.