Payroll Tax Cuts Past Retirement Age: Retention, Organizational Position, and Firm Performance*

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Abstract

We study Sweden's 2007–08 payroll tax cut for workers aged 66 and above using linked administrative data. We compare firms that spent a high share of their 2006 payroll on older workers with firms that spent a medium share, which we call high- and medium-exposure firms. Over 2007–2012, the probability of employing an older worker rises by 10 percentage points more in high-exposure firms. The number of older workers they employ rises by about 9 percent relative to medium-exposure firms, although this estimate is less precise, while their sales and value added fall by about 14 and 15 percent, or somewhat less among firms of similar size. In firms with older managers, about two-thirds of the predicted 2007–08 tax saving comes from the manager's own wages, and excluding the manager leaves little evidence of an increase in the probability of employing other older workers. Repeating the comparison in years without a new tax cut, before and after the reform, shows similar output declines, particularly among firms with older managers, and some employment increases, which weakens a causal interpretation, especially for output. We also compare tax savings with the employment response, but this does not give a reliable estimate of the saving per additional year of older-worker employment. Overall, our analysis shows why evaluations of payroll tax cuts for older workers need to distinguish the people who run firms from their other employees.

Keywords: Payroll taxes, older workers, retirement, firm heterogeneity, retention, Sweden

JEL Codes: D22, H25, H32, J14, J23, J26

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Introduction

Extending working lives depends in part on employers' willingness to keep employing older workers. Retaining an existing worker differs from hiring a new one. The employer already knows what the worker contributes. Pay in a long employment relationship may reflect past service as well as current productivity, so the cost of employing an older worker may exceed what the worker currently contributes. A reduction in payroll costs can therefore keep a relationship going that the employer would otherwise have ended. In a small business, the eligible worker may also be the person who runs the firm, and that person's decision to keep working can then be closely tied to the future of the business. We study how payroll tax cuts for older workers relate to continued employment and to the performance of the firms that employ these workers.

Sweden's 2007–08 reform abolished the special payroll tax on wages paid to workers aged 66 and above. At unchanged gross wages, it reduced the cost of employing an eligible worker by about 13 to 20 percent, depending on birth cohort. The tax cut applied to ongoing jobs as well as new hires, so it reached employers deciding whether to keep an older worker as well as those deciding whether to hire one. We link employer tax declarations to firm accounts, demographic registers, and Statistics Sweden's register of firm managers, and follow employment and firm outcomes from 2003 through 2012.

Our empirical comparison follows the firm-level approach of Saez et al. (2019), who study Sweden's payroll tax cut for young workers.[1] Because Sweden also reduced payroll taxes for young workers in 2007, we restrict the sample to firms whose payroll to young workers was small relative to their payroll to older workers. Within this sample, we sort firms by their older-worker payroll share in 2006, the year before implementation. Firms in the top quartile form the high-exposure group, and firms in the upper-middle quartile form the medium-exposure group. Both groups received the tax cut. Here, payroll includes gross wages and employer contributions. At unchanged gross wages, firms with larger older-worker payroll shares tend to save more relative to their total payroll costs. Holding each firm's 2006 workers and wages fixed, the predicted saving in 2008 equals 5.4 percent of payroll costs in high-exposure firms and 2.3 percent in medium-exposure firms. We estimate how outcomes changed in high-exposure firms relative to medium-exposure firms. Our baseline sample contains 2,221 private limited liability firms outside corporate groups, most of which are small.

Our first finding is that older-worker employment rises more in high-exposure firms than in medium-exposure firms. The probability that a firm employs at least one older worker rises by 10 percentage points more in the high-exposure group. Over 2007–2012, the number of older workers employed rises by about 9 percent relative to the medium-exposure group, although this estimate is less precise. The yearly estimates for the number of older workers are large and statistically significant in 2008–09, but close to zero by 2012. After 2006, both the share of firms employing an older worker and the average number of older workers per firm fall in both groups. The share falls less in high-exposure firms (Section 2.4). The probability of retaining at least one older worker rises by 8.4 percentage points more in high-exposure firms, compared with 0.6 percentage points for hiring. The difference between these estimates is statistically significant, but retention was already rising in high-exposure firms relative to medium-exposure firms before the reform. Total employment of workers aged 19 and above falls by about 5 percent in high-exposure firms relative to medium-exposure firms, an estimate significant only at the 10 percent level. The increase in older-worker employment therefore does not appear to be part of a broader expansion of employment in these firms.

Our second finding is that, in the same comparison, sales fall by about 14 percent and value added by about 15 percent in high-exposure firms relative to medium-exposure firms. When firms of different sizes in 2006 are allowed to follow different paths over time, the declines are about 10 and 13 percent. Both declines remain statistically significant, and the estimated increases in older-worker employment

[1] Minimum-wage studies similarly use pre-reform wage information to compare firms facing different increases in labor costs (Harasztosi and Lindner, 2019; Draca et al., 2011).

are larger (Section 6.5). The contrast between the relative rise in older-worker employment and the relative fall in output leads us to examine who the eligible workers are within the business.

In many of the firms we study, the person who runs the business is old enough to be eligible. Among firms with a recorded manager age, the registered manager was aged 66 or above in 2006 in 57 percent of high-exposure firms and 28 percent of medium-exposure firms. We call these firms senior-led and the others younger-led. In a senior-led firm, the manager's salary can be part of the payroll share that measures exposure, and the manager's continued employment can count toward older-worker employment.[2] In senior-led firms, about two-thirds of the predicted tax saving over 2007–2008 comes from lower employer contributions on the registered manager's own wages. In both groups, the probability of employing at least one older worker rises more in high-exposure than in medium-exposure firms: by 13 percentage points among senior-led firms and by 7 percentage points among younger-led firms, where managers who reach 66 after 2006 also count toward older-worker employment. When we exclude the manager, the estimated increase among senior-led firms is much smaller, at about 1 percentage point, and imprecise. This provides little evidence of an increase in the probability of employing an older worker other than the manager.

In the baseline comparison, the output declines are larger among senior-led firms. Within that group, sales and value added each fall by about 19 percent in high-exposure firms relative to medium-exposure firms, and by about 12 and 14 percent when firms of similar size are compared. Within younger-led firms, the sales decline is about 5 percent and imprecise, whereas value added falls by about 9 percent, an estimate significant at the 5 percent level. When firms of similar size are compared, the estimated differences between the two manager groups are smaller and no longer statistically significant. There are two ways to read the senior-led output pattern. Ranking firms by their older-worker payroll share may pick out businesses that were already contracting, because the manager's salary becomes a larger part of payroll as other workers leave. Repeating the comparison with firms selected and grouped in 2004, before the reform, and in each year from 2010 to 2013 adds to this concern. None of these grouping years was followed by a new payroll tax cut in the next two years. Over equally long periods, these placebo comparisons show similar declines in senior-led firms' log sales and log value added. However, Poisson sales estimates, which include years with zero sales, do not consistently show the same declines. A second possibility is that lower payroll costs allow some high-exposure firms to remain active for longer at a smaller scale. Their additional years of low activity could then contribute to the estimated decline in output. Our comparisons leave both explanations open.

Two features of the setting limit how much of the employment difference we can attribute to the payroll tax cut. First, we select firms and form the exposure groups using payroll from a single year, 2006, which was also the year in which the reform was announced. Grouping firms by one year's payroll share may separate firms whose employment would have developed differently even without the reform. The placebo comparisons bear on this concern. Without any new tax cut, the probability of employing an older worker again rises more in high-exposure firms, by about 4 percentage points in the comparison around 2013, where the gap between the groups was also changing before that year. In one comparison, the number of older workers in younger-led firms rises by more than after the reform. Most placebo employment estimates are smaller than those for the reform, but they suggest that some of the employment difference may arise from how we group firms. Second, an earned income tax credit with more generous terms for older workers took effect in 2007, alongside the payroll tax cut. The credit lowers income taxes by an amount that depends on earnings. If earnings differ between the two groups, the credit could have encouraged continued employment more in one group than in the other. To assess these concerns, we examine how the groups are formed and whether similar differences arise without a new tax cut (Section 6). We also discuss whether firms anticipated the reform and how other policy changes could affect the results (Section 3.5).

[2]The register identifies a management role, not ownership. A manager counts as employed only when earnings from the firm meet the same threshold as for other workers.

An important policy question is how the cost of the tax cut compares with its benefits. A full cost-benefit analysis would also need to account for the reform's effects on firms' activity, other tax revenues and benefit payments. Estimating all these effects credibly is difficult in our setting. We therefore examine the more limited question of how much predicted tax saving corresponds to an additional year of older-worker employment.[3] To do this, we divide the difference in predicted tax savings between the two groups by their estimated employment difference over 2007–2008. We find that the saving difference is precisely estimated at about 30,800 SEK per firm. However, the employment difference of 0.05 employment-years per firm cannot be distinguished from zero and, in this calculation, was already changing before the reform. We therefore conclude that we cannot reliably estimate the tax saving per additional year of older-worker employment.

Our main contribution is to add new evidence to the small literature on how payroll tax cuts affect employment among older workers. We also make two more specific contributions. First, we study the employers who decide whether older workers stay and connect older-worker employment to firm performance. Second, we examine the role of older workers within the firm, distinguishing registered managers from other employees. The person who runs the firm is often among the eligible workers, which affects how both payroll-based exposure and older-worker employment should be interpreted.

Previous evaluations of payroll tax cuts and subsidies for older workers have found different effects on retention, hours, and wages. A Belgian subsidy improved retention among older men at high risk of early retirement (Albanese and Cockx, 2019), whereas Finland's subsidy for older low-wage workers produced no employment or wage response but a small increase in hours among those already employed (Huttunen et al., 2013). Singapore's payroll tax reduction at age 60 instead raised wages without a detectable employment response (Kim et al., 2022). All three policies targeted workers below the usual retirement age. The Swedish reform, by contrast, reduced payroll taxes for workers aged 66 and above. Laun (2017) evaluates the payroll tax cut together with the earned income tax credit at the individual (as opposed to the firm) level. Laun finds that the two policies made individuals more likely to keep working in the year after they turned 65, and that the increase was concentrated among people who remained with the employer they had four years earlier. We examine the outcomes of the firms employing these workers. Crucially, whether an employment relationship continues is also the employer's decision.[4]

Our analysis of firm performance builds on studies that compare firms with different exposure to a payroll tax cut and show what employers do with the saving. Firms more exposed to Sweden's youth payroll tax cut expanded employment, capital, sales, and profits (Saez et al., 2019).[5] Bíró et al. (2022) apply the same comparison to Hungary's 2013 payroll tax cuts for workers under 25 and for workers aged 55 and above, in a descriptive exercise in their appendix. Firms more exposed to the youth cut also grew somewhat faster in employment and sales, whereas firms with high and medium exposure to the older-worker cut followed similar paths in size, wages, and sales after the reform.

Previous studies have compared responses to payroll tax cuts across firms of different productivity (Bíró et al., 2022) and size (Lobel, 2026), but have so far left aside whether an eligible worker is also the person running the firm. A firm's activity can depend on that person: Becker and Hvide (2022) find that sales of Norwegian start-ups fall sharply after the founder's death.[6] Because payroll tax cuts for workers past retirement age reach the people who run small firms as well as their employees, firm-level

[3] An employment-year means that a worker meets the annual earnings threshold at a firm, not necessarily a full year of full-time work.

[4] For Sweden's youth payroll tax cut, Saez et al. (2019) attribute about 80 percent of the employment increase to longer employment relationships rather than shorter unemployment spells. They measure how long people stay in work, even if they change employers.

[5] Other studies of the Swedish youth reform find no detectable short-run effect on profitability (Kaunitz and Egebark, 2017), a small employment and wage response among young workers (Egebark and Kaunitz, 2018), and employment effects that persisted after the subsidy was withdrawn (Saez et al., 2021).

[6] More generally, managers differ in the policies they pursue (Bertrand and Schoar, 2003), and management practices are associated with firm performance (Bloom and Van Reenen, 2007).

evaluations of such cuts need to consider whether a high payroll share reflects the salary of the person running the firm.

Section 1 describes the institutions and data. Section 2 describes older-worker employment, the firms in the sample, and their observation over time. Section 3 sets out the exposure comparison, reports the employment results, and discusses the conditions for reading them as employers' responses. Section 4 examines firm performance, and Section 5 compares senior-led and younger-led firms. Section 6 examines how payroll shares change after firms are grouped, placebo comparisons with firms grouped in 2004, before the reform, and in 2010–2013, after the reform, and whether allowing yearly changes to differ by industry, location or firm size alters the results. Section 7 relates the predicted tax savings to the employment difference. Section 8 concludes.

Appendix A reports additional results and robustness checks for the 2007–08 reform. Appendix B describes the payroll tax increase in 2016 and its repeal in 2019. Appendix C shows descriptive age profiles of gross wages and wages including employer payroll tax in 2015–2019.

1 Institutional Setting and Data

1.1 Institutional Setting

Swedish employers pay payroll taxes on employees' gross wages, and part of these taxes finances social insurance benefits. The special payroll tax was introduced in the 1990–91 tax reform to tax income that did not generate such benefits, including earnings after retirement age, so that different forms of income and compensation would face similar taxation. The 2007–08 abolition of this tax on older workers' earnings departed from that principle. Its aim was to encourage employers to retain and hire older workers and to support people running their own businesses. The government reports the exemption as a tax expenditure (Riksrevisionen, 2020).

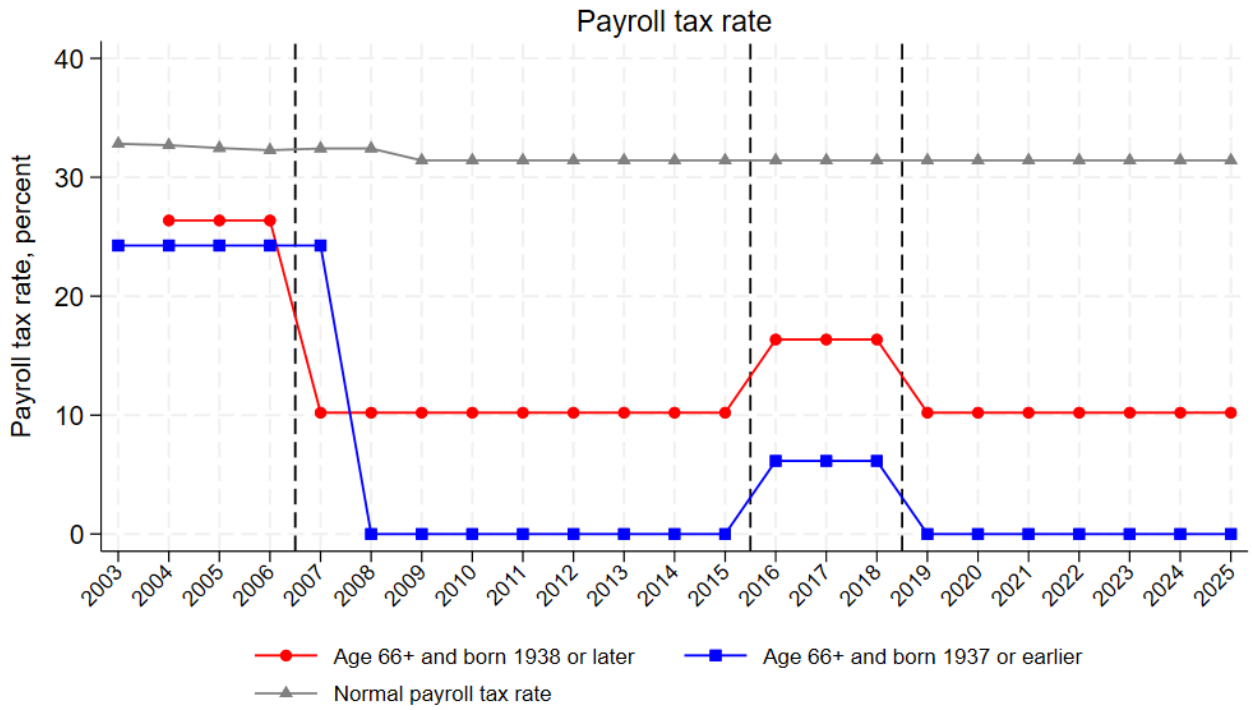
The abolition occurred in two steps because different payroll tax rules applied to workers born in 1937 or earlier and to those born in 1938 or later. For the later cohorts, the age-specific rules applied from the calendar year in which a worker turned 66. Before the reform, employers paid a 16.16 percent special payroll tax and a 10.21 percent old-age pension contribution on these workers' wages, for total employer contributions of 26.37 percent. The special tax was removed on January 1, 2007, leaving only the pension contribution. With gross wages fixed, the cost of employing an eligible worker fell from 126.37 to 110.21 per 100 kronor of wages, a reduction of 16.16/126.37, or 12.8 percent. Workers born in 1937 or earlier received the exemption one year later, on January 1, 2008. At unchanged gross wages, it reduced the cost of employing these workers by 19.5 percent.[7] Figure 1 shows the rates by age group, including the later changes discussed in Appendix B. Throughout the paper, employer contributions mean the charges levied on gross wages, including the special payroll tax, and payroll costs mean gross wages plus employer contributions.

Employers did not need to apply for the tax reduction, and eligibility depended only on birth cohort and calendar year, regardless of the date of the worker's birthday. We can therefore determine statutory eligibility in the annual data. Throughout the paper, ages refer to age reached during the calendar year unless stated otherwise.

The first step was proposed in the October 2006 budget bill, following the September election, and took effect in January 2007. Firms therefore had some scope to adjust employment before the end of 2006, the year we use to select the sample and measure exposure. The same policy program introduced

[7] These workers accrued no new pension rights, and employers paid only the special payroll tax of 24.26 percent. The cost reduction at fixed gross wages is therefore 24.26/124.26 (Prop. 2006/07:1; Prop. 2007/08:24; Riksrevisionen 2020). Older workers already faced lower employer contributions before the reform. For comparison, the ordinary employer contribution rate was 32.42 percent in 2007. See the Swedish Tax Agency's *Tax Statistical Yearbook of Sweden 2007* for historical contribution rates.

Figure 1: Payroll Tax Rates in Sweden by Age Group.



Notes: Data for 2006–2025 were collected from the Swedish Tax Agency website. Data for 2003–2005 come from the *Tax Statistical Yearbook of Sweden 2006*. The normal payroll tax rate refers to ages 27–65. The 1938 cohort turned 66 in 2004. The 2019 points show the rates after the July repeal, rather than annual averages. Through 2022, the reduced rate for workers born in 1938 or later applied from the year a worker turned 66. In 2023–2025, it applied from the year a worker turned 67.

an earned income tax credit (EITC, *jobbskatteavdraget*) with more generous treatment for older workers. A payroll tax cut for young workers took effect in July 2007 (Saez et al., 2019). Section 3.5 discusses anticipation and these concurrent policies.

The employment-protection and pension systems also shaped firms' decisions to retain older workers. Age 65 remained the conventional retirement age, although workers were not required to stop working then. During the period we study, employment protection weakened sharply at age 67 under Sweden's Employment Protection Act (*Lagen om anställningsskydd*, LAS). Employers could end a contract at the end of the month in which a worker turned 67 by giving at least one month's written notice. After turning 67, workers lost the priority that longer service normally gave them when an employer cut jobs, and they lost priority for rehiring by the same employer.[8] Employment did not end automatically at age 67, but Saez et al. (2025) document an increase in job separations at that age, more pronounced at larger employers and in the public sector. The tax cut lowered payroll costs both for eligible workers who had not yet turned 67 and for those who had. Workers could combine pension income with earnings, and receiving a pension did not affect eligibility for the payroll tax reduction.

For managers who also own shares in the business, tax rules can affect how they receive income as well as whether they continue working. In closely held companies, these managers may receive both salary and dividends, which receive different tax treatment under Sweden's 3:12 rules. Because our employment measure is based on wage payments, a shift from dividends to salary can raise measured employment and reduce accounting profits even if the manager's work is unchanged. Section 5 discusses this issue when examining employment among managers and other workers.

1.2 Data and Sample Construction

Our baseline analysis uses Swedish administrative data covering 2003–2012, with 2006 as the reference year. Earnings data come from annual employer tax declarations, which report each employee's gross cash earnings and exclude benefits in kind. We merge these records with firm accounts from Statistics Sweden's Structural Business Statistics (FEK), containing income statements and balance sheets, and with demographic registers that provide each worker's birth year. Following Saez et al. (2019), we count a worker as employed by a firm if annual earnings from that employer reach at least one price base amount, 39,700 SEK in 2006. Firm-level payroll and total employment cover workers aged 19 and above. When a firm appears in the accounts but has no matching wage records for workers aged 19 or above in that year, we record both total and older-worker employment as zero. Since hours are not observed, changes in measured employment can reflect workers crossing this earnings threshold as well as starting or leaving jobs.

The sample consists of Swedish-controlled private limited liability firms. We require at least three employees in 2006, because in firms with one or two employees a single worker can dominate both the older-worker payroll share and the measured employment response. We exclude non-profit organizations and, following Saez et al. (2019), firms belonging to corporate groups (*koncerner*), because employment decisions in these firms may be made at the group level and transfers of workers or costs within the group can blur an individual firm's exposure.[9] Financial firms are absent from the firm-level accounting data. We also require sales above 10,000 SEK in 2006, expressed in 2023 prices using the consumer price index (CPI).

[8] For the conventional retirement age, see Prop. 2006/07:1, pp. 15–16. The age-67 rule was introduced in 2001 (Prop. 2000/01:78) and remained in force throughout our study period. The right to end a contract without stating grounds applied only in the month of the 67th birthday. If the contract continued, a later dismissal required just cause, but statutory notice was limited to one month and the worker had no seniority or rehiring priority.

[9] Statistics Sweden also sometimes aggregates accounting information to the group level, which is why Saez et al. (2019) restrict their baseline sample to firms outside corporate groups. See Kaunitz and Egebark (2017) for a discussion of the related data issues.

We use Statistics Sweden's register of firm managers to compare results by the age of the registered operative manager. The register draws on information from the Swedish Company Registration Office, occupation records, and wage income to identify one operative manager for each firm. For the firms in our sample, manager identification is primarily based on CEO records. The register is available from 2004 onwards and identifies a management role, but it does not establish whether the manager owns the firm.

To limit exposure to the simultaneous youth payroll tax cut, we require the payroll share of workers aged 19–25 to be less than one quarter of the payroll share of workers aged 66 or above. Among firms meeting this and the other sample restrictions, we rank firms by their 2006 older-worker payroll share. The highest quartile forms the high-exposure group and the next quartile the medium-exposure group. Table 1 shows the restrictions and firm counts. Both groups consist of firms that paid older-worker wages before the reform.

We follow the firms in the 2006 sample from 2003 through 2012, although not every firm is observed in every year. Missing accounts alone do not tell us whether a firm has closed. Section 2.3 distinguishes observation in the accounts from reported activity; Section 5.3 discusses the implications for interpreting the output estimates.

2 Older Workers and the Firms That Employ Them

We describe how the sample is selected, how much of the older-worker employment and wages in the source data it covers, and the role of managers in these firms. We then follow employment and activity over time. These descriptions do not attribute differences between firms to the payroll tax cut.

2.1 Sample Selection and Coverage

After linking the tax register data to FEK, we obtain a source sample of 59,185 firms and organizations that paid wages to at least one worker aged 66 or above in 2006. Table 1 shows how the restrictions narrow this to the baseline sample of 2,221 firms. We measure coverage relative to the source sample rather than to all employers in Sweden.

Table 1: Sample Selection, 2006.

Restriction	Excluded	Remaining
Paid wages to a worker aged 66 or above	—	59,185
Limited liability firms	31,923	27,262
Swedish-controlled private firms outside corporate groups	11,035	16,227
At least three employees	5,413	10,814
Not a non-profit organization	7	10,807
CPI-adjusted sales above 10,000 SEK	52	10,755
Youth payroll below one quarter of older-worker payroll	6,312	4,443
Top two quartiles of the older-worker payroll share	2,222	2,221
High exposure		1,110
Medium exposure		1,111

Notes: Each restriction is applied to the firms remaining after the preceding row. Employment counts workers aged 19 or above with annual wages from the firm of at least one price base amount. Youth payroll covers ages 19–25 and older-worker payroll ages 66 and above; both include employer contributions. Quartiles are formed after the preceding restrictions. FEK covers Sweden's non-financial business sector.

The 2,221 firms make up 3.8 percent of firms in the source sample but account for 7.9 percent of its counted older-worker employment and 8.1 percent of the gross wages paid to workers aged 66 or above.[10]

2.2 Firm Characteristics and the Role of Managers

Table A.1 in Appendix A reports unweighted summary statistics for each exposure quartile.[11] The sample is concentrated in services, manufacturing, and trade. High-exposure firms are smaller than medium-exposure firms, with mean employment in 2006 of 4.35 and 6.45, respectively. Figure 6, discussed alongside the exposure definition in Section 3.2, shows the payroll-share distributions and their paths over time.

We identify the registered manager in 2,202 of the 2,221 firms; the remaining 19 firms enter only the full-sample analysis. We call a firm senior-led if its manager was aged 66 or above in 2006 and younger-led otherwise, and keep this classification fixed in later years. Senior-led firms make up 57 percent of the high-exposure firms and 28 percent of the medium-exposure firms with manager information. Table 2 reports the counts, and Figure 2 shows the distribution of manager ages. A registered manager need not own the firm. In about 23 percent of younger-led firms, the manager was aged 60–65 in 2006 and would reach age 66 by 2012, so older-worker employment in these firms can include the manager in later years.

Table 2 compares firm characteristics within these groups. Within each exposure group, senior-led and younger-led firms have similar mean employment and median sales. Among high-exposure firms, however, the mean older-worker payroll share is higher in senior-led firms, at 35 percent compared with 28 percent in younger-led firms.

[10] Both shares are ratios of unweighted totals: 2,917 of 36,946 older-worker employment relationships and about 508 of 6,308 million SEK in wages. Employment uses the same earnings threshold in both samples and counts a person who works for several firms at each firm. Wages include pay below the employment threshold and exclude employer contributions.

[11] A firm enters the source sample if it paid any positive wages to a worker aged 66 or above in 2006. The employment measure counts that worker only if earnings reached one price base amount. Mean measured older-worker employment can therefore be below one, as it is in the lower quartiles of Table A.1.

Table 2: Firm Characteristics by Manager Type and Exposure Group, 2006.

Characteristics	Mean	Median	Firms
Panel A: Younger-led firms, high exposure			
Total employment	4.41	4	478
Sales (thousand SEK)	7,418	4,402	478
Older-worker payroll share	0.28	0.25	478
Panel B: Younger-led firms, medium exposure			
Total employment	6.33	5	791
Sales (thousand SEK)	10,306	5,949	791
Older-worker payroll share	0.12	0.11	791
Panel C: Senior-led firms, high exposure			
Total employment	4.31	4	622
Sales (thousand SEK)	8,258	4,373	622
Older-worker payroll share	0.35	0.31	622
Panel D: Senior-led firms, medium exposure			
Total employment	6.72	5	311
Sales (thousand SEK)	10,633	6,392	311
Older-worker payroll share	0.13	0.13	311

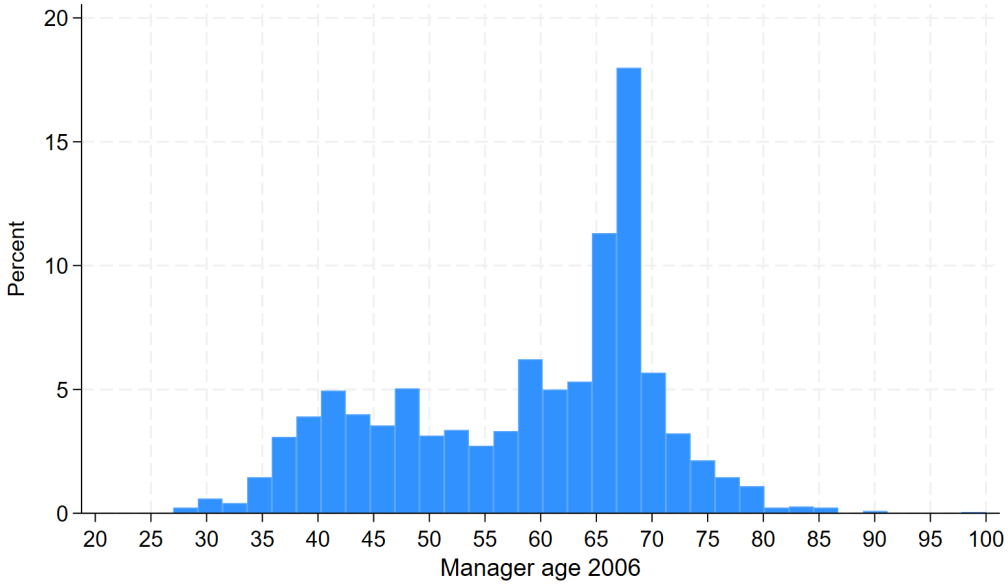
Notes: Each firm receives equal weight. Manager type is fixed by age in 2006: senior-led means aged 66 or above. Sales are CPI-adjusted and expressed in thousands of SEK. Employment counts workers aged 19 or above with annual wages from the firm of at least one price base amount. The older-worker payroll share is wages plus employer contributions for workers aged 66 or above divided by the corresponding total for workers aged 19 or above.

Managers account for about 65 percent of counted older-worker employment in senior-led firms in 2006 (854 of 1,317 worker–firm relationships). In 547 of the 609 senior-led firms with exactly one counted older worker, or 90 percent, that worker is the manager. Figure 3 shows the number of older workers per firm in each group.

Managers also receive a large share of the wages paid by senior-led firms. In high-exposure senior-led firms, the manager receives an average of 26 percent of the firm's gross wages (median 24 percent), and other older workers receive 10 percent (median 2 percent). In medium-exposure senior-led firms, the corresponding averages are 10 and 3 percent (medians 11 and zero percent).[12] Managers' own wages are therefore an important component of the older-worker payroll used to measure exposure. These 2006 figures do not show whose employment changed after the reform.

[12]These are unweighted means and medians of firm-level shares, rounded to whole percentages. Both wage categories include pay below the employment threshold, and the denominator is the firm's total gross wages to workers aged 19 or above. In 56 senior-led firms, the manager received no wages from the firm, and the manager's share is zero.

Figure 2: Distribution of Manager Age in 2006.



Notes: Age in 2006 of the registered operative manager in the 2,202 firms with manager information; 19 firms are excluded because manager information is missing. Each firm receives equal weight. Bins are about two years wide. Senior-led firms have a manager aged 66 or above in 2006; this classification is fixed throughout the analysis. The senior-led groups contain 622 high-exposure and 311 medium-exposure firms, and the younger-led groups 478 and 791, respectively.

Figure 3: Distribution of Older Workers in 2006, by Manager Type and Exposure Group.



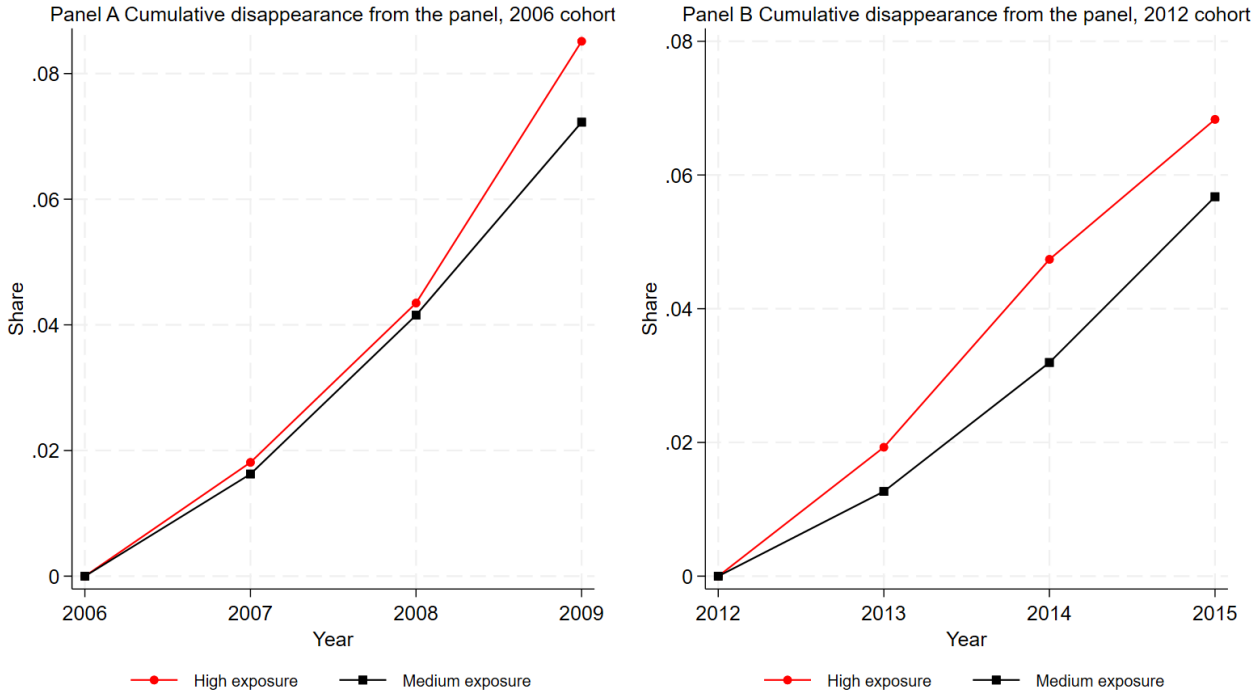
Notes: Senior-led firms have a manager aged 66 or above in 2006. Older workers are aged 66 or above and receive annual wages from the firm of at least one price base amount; managers meeting these conditions are included. Each panel shows percentages within its manager-type and exposure group, conditional on having at least one counted older worker. The figure excludes 3 senior-led and 22 younger-led firms with no counted older worker. Percentages are weighted by the logarithm of CPI-adjusted 2006 sales.

2.3 Observation and Activity over Time

We follow whether the selected firms continue to appear in the accounts data. A firm that stops appearing has not necessarily closed; absence can also reflect a merger, a reorganization or a change in legal form. A firm that appears in the accounts always has a recorded sales figure, which may be zero. Zero sales are therefore distinct from disappearance and are treated as an outcome in Section 4. Disappearance also determines which firms contribute to the output comparisons in later years.

Figure 4 compares cumulative disappearance from the panel for firms selected in 2006 and for a second set selected by the same rules in 2012. Section 6.3 uses the 2012 cohort for the comparison around 2013. Table A.2 in Appendix A reports the starting counts, the shares by year and the differences between exposure groups. In both cohorts, between 6 and 9 percent of firms have disappeared after three years. Section 5.3 presents the comparison by manager age in Figure 14 and relates it to the sales findings.

Figure 4: Cumulative Disappearance from the Panel, 2006 and 2012 Cohorts.

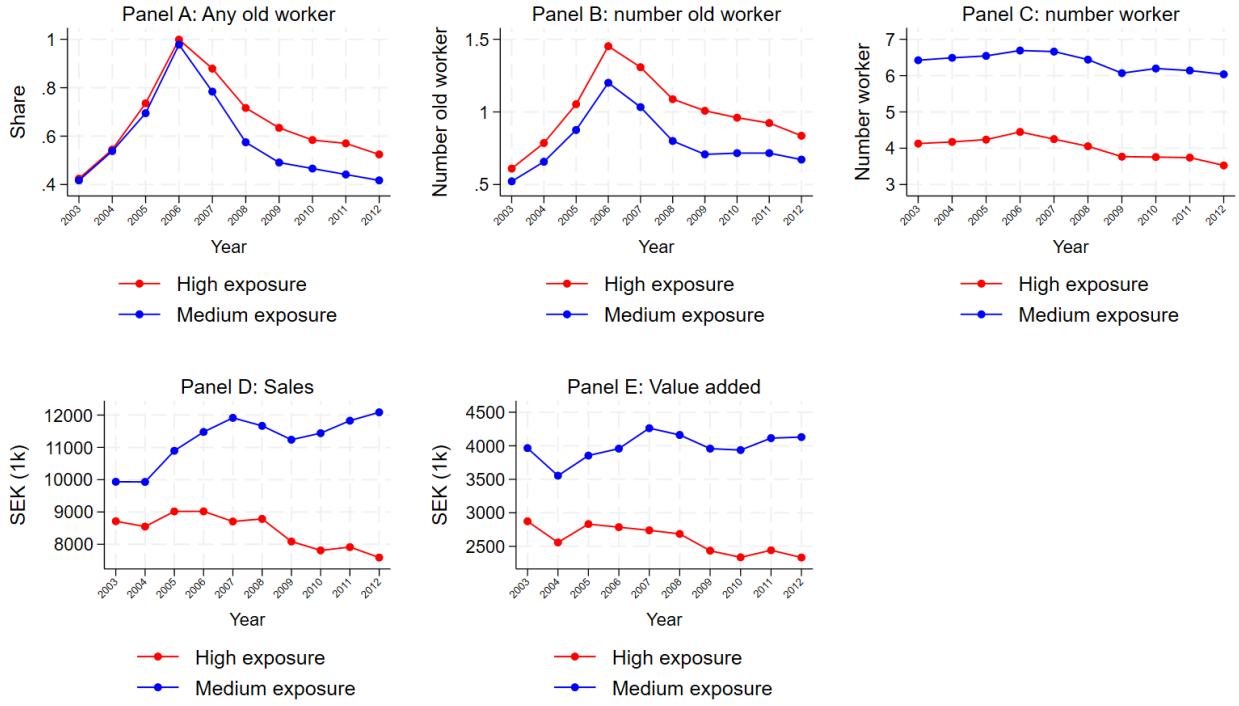


Notes: Panel A follows firms selected in 2006 over 2006–2009 and checks for later appearances through 2012. Panel B follows firms selected in 2012 over 2012–2015 and checks through 2018. A firm counts as having disappeared from the year after its last appearance within the relevant period. The 2006 cohort contains 2,211 firms and the 2012 cohort 3,630 firms, after excluding 10 and 7 firms that disappear and return within the observation period. Each firm receives equal weight. Table A.2 reports the starting counts and the differences between exposure groups with their standard errors. Disappearance from the panel does not establish that a firm closed.

2.4 Raw Outcome Paths

Figure 5 shows the annual means of the main outcomes for each exposure group, with groups fixed at their 2006 assignment and the same weights as the regressions. The firms contributing to each mean can change from year to year as firms enter and leave the accounts data. The sales and value-added panels average levels, including zero and negative values, so they differ from the log specifications, which use positive values only.

Figure 5: Annual Outcome Paths by Exposure Group, 2003–2012.



Notes: Weighted means by year for high- and medium-exposure firms, with groups fixed at their 2006 assignment and weights equal to the logarithm of CPI-adjusted 2006 sales, as in the regressions. Panel A is the share of firms employing at least one counted older worker, Panel B the mean number of older workers, Panel C mean total employment, and Panels D and E mean sales and value added in thousands of SEK at 2023 prices, including zero and negative values. Firm-years with accounts but no wage records count as zero employment. The firms contributing to each mean change over time as firms enter and leave the accounts data; the figure does not report their number.

Both employment measures peak in 2006, the year used to select firms and form the groups, and decline thereafter in both groups. The share of firms with a counted older worker is close to one in 2006, when every sample firm paid wages to an older worker, and falls in both groups afterwards, less in high-exposure firms than in medium-exposure firms. The average number of older workers also declines in both groups. Total employment is lower in high-exposure firms throughout and drifts down after 2006 in both groups. Sales fall in high-exposure firms after 2006 while they rise in medium-exposure firms, and value added shows a similar divergence. The figure illustrates the selection concern discussed in Section 6.1: the estimated relative increase in older-worker employment accompanies declining employment levels in both groups.

Table A.10 reports annual firm counts and the share employing a counted older worker in the main sample and the comparison around 2013, using the firms and years included in the corresponding employment-probability regressions. The shares differ by less than one percentage point with and without the regression weights.

3 Employment Effects within Firms

3.1 Employers' Decisions to Retain and Hire

An employer deciding whether to retain an older worker compares the cost of continued employment with the worker's contribution to the business. In a long employment relationship, pay may reflect past service as well as the worker's current contribution (Lazear, 1979). If gross wages remain unchanged, a payroll tax cut lowers the employer's cost. It can then make retention worthwhile when the worker's

contribution covers the new payroll cost but not the old one. This decision is particularly relevant at age 67, when the employer can end the relationship without stating grounds, and afterwards, when notice and priority protections are weaker. Lower costs can also make hiring an older worker worthwhile. Which response is larger is an empirical question, so we examine both retention and hiring.

Unchanged gross wages provide a reasonable short-run benchmark for examining employers' decisions to retain and hire older workers. In Sweden, Saez et al. (2019) find no detectable wage increase for workers eligible for the youth payroll tax cut relative to slightly older workers.[13] The descriptive profiles in Appendix C show a visible fall in wages including payroll tax at age 66, without an offsetting increase in gross wages. These later age profiles illustrate the difference between gross wages and payroll costs. They do not establish the wage response to the 2007–08 reform.

3.2 Exposure and Empirical Design

Let $P_{i,t}$ denote firm i's payroll costs for workers aged 19 and above in year t, including gross wages and employer contributions, and let $P_{i,66+,t}$ denote the corresponding costs for workers aged 66 and above. We define exposure as the older-worker share of the firm's 2006 payroll costs:

$$\text{Index}_{i,2006} = \frac{P_{i,66+,2006}}{P_{i,2006}}. \tag{1}$$

A larger index means that older workers account for a greater share of the firm's payroll costs. The index measures exposure to the reform, but it does not give the firm's actual statutory saving.[14]

We use the upper-middle quartile as the comparison group because its firms are closer in size to high-exposure firms than are firms in the lower two quartiles, while still having a substantially smaller older-worker payroll share than high-exposure firms. Firms in the lower two quartiles are larger and employ fewer older workers (Table A.1).[15]

Figure 6 shows the distribution of exposure in 2006 and the average share over time for each quartile. High-exposure firms have shares above approximately 17 percent, and medium-exposure firms have shares between approximately 8 and 17 percent. The groups remain ordered by their average share throughout the period. The high-exposure group's average rises from about 13 percent in 2003 to 32 percent in 2006, then declines. Ranking firms on their 2006 share can place firms in the high-exposure group because their share was temporarily high that year. A return toward their usual payroll composition, or mean reversion, could therefore account for part of the subsequent decline, as in Saez et al. (2019). Workers and managers reaching age 66 in the years before the reform may also contribute to the rise. After the reform, lower contribution rates on older-worker wages also change these payroll-cost shares, even with unchanged wages and workers. Section 6 examines the implications for the employment and output estimates.

We compare changes in the two groups using difference-in-differences (DiD) with firm and year fixed effects. For outcomes estimated with a linear model, the baseline specification is

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot D_i \times \text{Post}_t + \varepsilon_{it}. \tag{2}$$

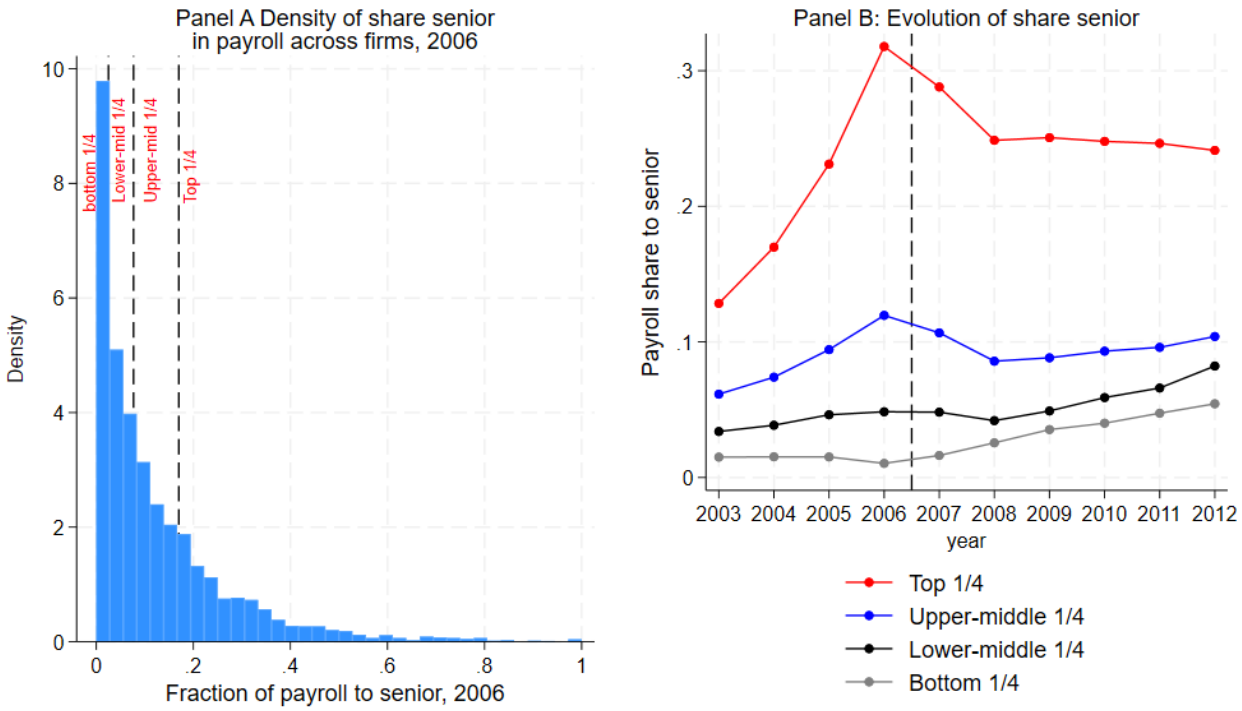
Here, Y_{it} is the outcome for firm i in year t, α_i and γ_t are firm and year fixed effects, and D_i equals one for high-exposure firms and zero for medium-exposure firms. The indicator $\text{Post}_t = \mathbf{1}\{t \geq 2007\}$

[13] Their firm-level analysis does, however, find wage gains among workers in more exposed firms.

[14] Both the size and timing of the tax reduction depend on birth cohort. The index combines workers covered by the exemption from the special payroll tax in 2007 with those covered only from 2008. It also excludes workers below age 66 in 2006 who became eligible later.

[15] Saez et al. (2019) use the middle two quartiles as their comparison group. We considered alternative exposure groups and chose the upper-middle quartile partly because its pre-reform outcome paths were closer to those of the top quartile.

Figure 6: Older-Worker Payroll Shares: Distribution and Evolution by Quartile.



Notes: Panel A shows the distribution across firms of the older-worker payroll share in 2006. Panel B shows the average share over time for each quartile, weighted by the logarithm of CPI-adjusted 2006 sales. Payroll includes gross wages and employer contributions for workers aged 19 and above. The contribution rates vary by year, age and birth cohort, so changes in the share also reflect changes in tax rates. Firms are assigned to quartiles using their 2006 shares.

marks the post-reform period. Firm effects account for differences in outcome levels that are constant over time, while year effects account for changes common to both groups.

The coefficient β measures the change in the outcome in high-exposure firms minus the change in medium-exposure firms. Because medium-exposure firms also received the tax cut, β shows how the response of firms with a larger older-worker payroll share differed from that of firms with a smaller share. It does not measure the effect of the national reform compared with a situation without the tax cut.[16] Lower costs can lead a firm to expand or to employ older workers in place of other inputs, and our comparison does not separate these two responses.

A causal interpretation requires that, without the reform, high- and medium-exposure firms would have followed parallel paths in the outcome being studied.[17] As in other difference-in-differences studies, spillovers between firms can affect the comparison. Workers or customers may move from one group to the other, so changes in the groups' relative outcomes need not imply a change in total employment or production.

The estimation period is 2003–2012, and the pooled post-reform comparison covers 2007–2012. We also interact the high-exposure indicator with each year, omitting 2006. These event-study coefficients show how the difference between the groups changes relative to 2006, both before and after the reform.

[16] Employers in both groups face the same statutory tax cut for workers in a given birth cohort; the groups differ in how much of their payroll is affected. Our estimate therefore does not show how employment responds to a one-percent reduction in the cost of employing an older worker. Dividing the percentage employment estimate by the difference in predicted savings as a share of total payroll costs would show the employment difference relative to firms' savings. Interpreting this ratio causally also requires assumptions about differences in firms' responses to the saving, beyond parallel trends (Callaway et al., 2024).

[17] All firms faced the same statutory schedule, so adoption was not staggered across firms in the sense studied by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). Firms nevertheless differed in effective exposure because the two birth-cohort groups became exempt in different years.

Group assignment remains fixed at its 2006 value. The separate 2007 coefficient is useful because the tax exemption was only partly implemented in that year.

Our two older-worker employment outcomes are whether a firm employs any worker aged 66 or above and the number of such workers, using the earnings threshold defined in Section 1. Many firm-years have no older workers. We estimate count outcomes using Poisson quasi-maximum likelihood, which accommodates these zeros (Chen and Roth, 2024), with conditional mean

$$E[Y_{it} \mid \alpha_i, \gamma_t, D_i] = \exp(\alpha_i + \gamma_t + \beta \cdot D_i \times \text{Post}_t)\,. \tag{3}$$

The implied proportional difference is $e^{\beta} - 1$, or $100(e^{\beta} - 1)$ percent. For the probability of employing any older worker, we use a linear probability model, so 100β is the difference in percentage points. The Poisson fixed-effects model excludes firms whose count is zero in every year, whereas the linear probability model retains them. We also estimate total employment using Poisson. Section 4 describes how we measure sales, value added, assets, and profits and estimate the effects on these outcomes.

Following Harasztosi and Lindner (2019), we weight the firm-level employment and performance regressions by the logarithm of pre-reform sales. Specifically, for the 2007–08 reform, we construct a fixed weight for each firm using CPI-adjusted 2006 sales, measured in thousands of SEK. These weights give greater influence to firms accounting for more economic activity. Taking logarithms moderates the differences in weights between large and small firms compared with weighting directly by sales.[18] We cluster standard errors by firm.

3.3 Older-Worker Employment

Our first result is that older-worker employment rises more in high-exposure firms than in medium-exposure firms after the reform. Table 3 reports the pooled post-2006 estimates. The Poisson coefficient for the number of older workers is 0.083 (standard error 0.047), implying an 8.7 percent increase relative to the employment path in medium-exposure firms; the estimate is significant at the 10 percent level. For a firm employing one older worker before the reform, an 8.7 percent increase would amount to about 0.09 additional older workers. The probability of employing any older worker rises by 10 percentage points more in high-exposure firms, compared with a 2003–2006 mean of 68 percent in that group.

Table 3: Baseline Employment Results, 2007–08 Reform.

Outcome	Coefficient (DiD)	Std. Error	Pre-reform Average	Obs.
Any older worker (LPM)	0.100***	(0.021)	0.68	20,754
Number of older workers (Poisson)	0.083*	(0.047)	0.98	20,667
Total employment (Poisson)	-0.056*	(0.033)	4.25	20,754

Notes: Each coefficient compares the change between 2003–2006 and 2007–2012 in high-exposure firms with the change in medium-exposure firms. Standard errors clustered at the firm level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. LPM refers to linear probability model. The Poisson fixed-effects model for the number of older workers excludes firms whose count is zero in every year. Firm-years without wage records count as zero employment. Pre-reform average refers to the average value for high-exposure firms during 2003–2006, weighted like the regressions. All regressions include firm and year fixed effects and are weighted by the logarithm of CPI-adjusted 2006 sales, measured in thousands of SEK.

The payroll share measures exposure, not the saving in kronor. To show how much the tax cut was worth to the firms in each group, we calculate the saving each firm would have made in 2007 and

[18] Estimates without sales weights are similar for employment and firm performance, both overall and within manager groups. The older-worker count estimate rises from 8.7 to 9.7 percent and becomes significant at the 5 percent level. The senior-led log-sales and log-value-added estimates are almost unchanged.

2008 had its 2006 workers and wages remained unchanged. We apply each year's exemption to the 2006 wages of the cohorts eligible in that year, as described in the notes to Table 4; workers born in 1941 and 1942 are included from the year they turn 66, although they were younger in 2006. Table 4 expresses the predicted saving as a percentage of each firm's 2006 payroll cost for workers aged 19 and above, including employer contributions. In 2008, the predicted saving equals 5.4 percent of payroll costs in high-exposure firms and 2.3 percent in medium-exposure firms, a difference of 3.1 percentage points. In 2007, the corresponding figures are 2.6 and 1.0 percent. High-exposure firms have lower payroll costs. Their predicted saving in 2008 is about 64,400 SEK per firm, compared with 43,300 SEK in medium-exposure firms. This is about one and a half times as much, compared with more than twice as much when savings are measured as a share of payroll costs. These are predicted savings on unchanged workers and wages, not the amounts that firms actually saved.

Table 4: Predicted Tax Savings on 2006 Workers and Wages.

	High exposure	Medium exposure	Difference
Percent of 2006 payroll cost			
2007	2.60	1.00	1.59*** (0.08)
2008	5.44	2.34	3.10*** (0.09)
SEK per firm			
2007	29,402	20,203	9,199*** (1,384)
2008	64,396	43,261	21,135*** (2,469)
2006 payroll cost, SEK per firm	1,255,184	1,936,286	
Firms	1,090	1,083	

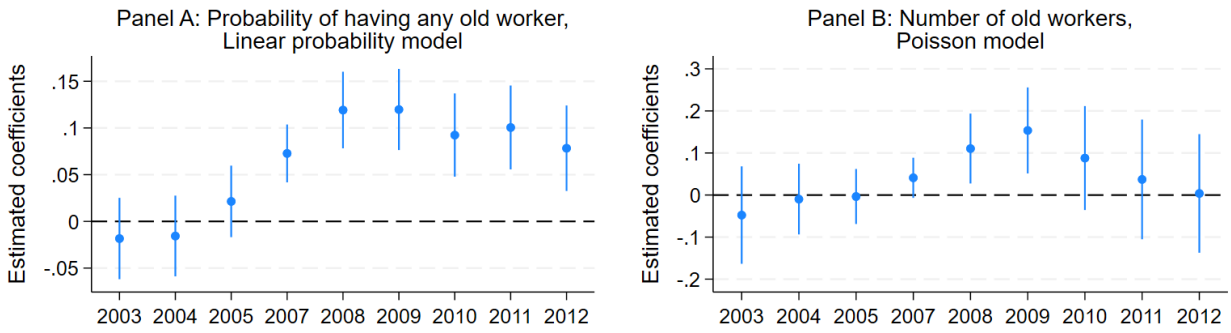
Notes: Predicted savings hold each firm's 2006 workers and wages fixed. The 2007 saving is 16.16 percent of 2006 wages paid to workers born in 1938–1941; the 2008 saving adds 24.26 percent of wages paid to workers born in 1937 or earlier and 16.16 percent of wages paid to workers born in 1942. Payroll cost covers workers aged 19 and above and includes employer contributions at 2006 rates. The firms are those contributing post-reform observations to the regression for the number of older workers in Table 3. Means are weighted by the logarithm of CPI-adjusted 2006 sales, with one observation per firm. Differences come from a regression on a high-exposure indicator with the same weights; heteroskedasticity-robust standard errors in parentheses. Kronor are nominal 2006 amounts. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 7 shows how these employment differences develop over time, using 2006 as the reference year. Both estimates are small before the reform and positive afterward, but they then follow different paths. The increase in the probability of employing at least one older worker remains statistically significant through 2012, whereas the increase in the number of older workers is significant only in 2008 and 2009 and becomes smaller and imprecise thereafter. The persistence of the probability difference alongside a fading count difference could arise if firms continue to employ an older worker while other older workers retire. Such changes in the workforce can also alter firms' exposure to the tax cut. Because we keep the groups fixed throughout the analysis, their 2006 payroll shares may become less informative about their subsequent exposure.

3.4 Retention, Hiring, and Total Employment

The linked employer–employee records allow us to follow each worker at each firm from 2002 to 2012. In each outcome year, we count workers aged 66 or above whose earnings from the firm reach at least one price base amount. We classify these workers by their record at the same firm in the preceding calendar year. A retained worker meets the earnings threshold in both years. A worker crossing the threshold has a wage record at the firm in the preceding year but earns less than that year's threshold. A new hire has no wage record at that firm in the preceding year; this includes workers returning after a

Figure 7: Dynamic Effects of the 2007–08 Reform on Employment of Older Workers.



Notes: Coefficients compare high- with medium-exposure firms, with groups fixed at their 2006 assignment; specifications follow Table 3. Panel A shows estimates for the probability of employing any worker aged 66+, using a linear probability model estimated by ordinary least squares (OLS). Panel B shows Poisson QMLE estimates for the number of older workers. Both panels report 95% confidence intervals. The reference year is 2006. Joint tests of the 2003–2005 coefficients do not reject: $p = 0.21$ (Panel A) and 0.83 (Panel B). Standard errors are clustered at the firm level.

gap. Workers with several employers are followed separately at each firm. The age requirement applies in the current year, so turning 66 at an existing employer does not by itself make someone a new hire.

We use the same firm-years as the baseline employment analysis. When a firm appears in the accounts but has no matching wage records for workers aged 19 or above, we set all three counts to zero. At each firm, every counted older worker belongs to exactly one of these three groups. Most are retained workers. The probability regressions use the baseline sample of 20,754 firm-years.

Table 5 shows that the probability of retaining at least one older worker rises by 8.4 percentage points more in high-exposure firms than in medium-exposure firms. The corresponding estimate for hiring is 0.6 percentage points, with a standard error of 0.6 points. Their difference is 7.9 percentage points (standard error 2.3), significant at the 1 percent level.[19] The estimated increase in the probability of employing at least one older worker who crosses the earnings threshold is 0.3 percentage points (standard error 0.5). The three count estimates are imprecise and none is statistically significant at the 10 percent level.

These estimates do not establish how much of the increase in older-worker employment comes from retention, threshold crossings or hiring. A firm can retain workers and hire others in the same year, so the probabilities do not add up to the probability of employing any older worker. Nor can the Poisson count coefficients be added up: they describe proportional changes from different starting levels and are estimated on different samples. A retained older worker may also be the firm's registered manager. Section 5 reports the employment results by manager age and, for senior-led firms, with the manager excluded.

Figure 8 shows that the difference between high- and medium-exposure firms in the probability of retaining at least one older worker was already increasing before 2006 (Panel A). The post-reform point estimates are positive relative to 2006, peak around 2009 and are lower by 2012. The joint test of the 2003–2005 coefficients gives $p = 0.067$, rejecting at the 10 percent level but not at 5 percent. Some of the later relative increase may therefore continue an earlier pattern. Grouping firms by their 2006 older-worker payroll shares may contribute to these patterns, but the figure does not establish how much or in which direction. The estimates in Table 5 compare 2007–2012 with 2003–2006; the figure compares each year with 2006.

[19] Among firms reporting sales, value added and total assets in every year from 2003 to 2012, the corresponding difference is 8.6 percentage points (standard error 2.5; $p < 0.01$). This comparison includes only firms that continue to appear in the data after the reform.

Table 5: Retention, Threshold Crossings, and Hiring around the 2007–08 Reform.

	Probability				Number of workers		
	(1) Retention	(2) Threshold crossing	(3) Hiring	(4) Retention minus hiring	(5) Retention	(6) Threshold crossing	(7) Hiring
Coefficient	0.0842***	0.0033	0.0057	0.0785***	0.067	0.023	0.147
	(0.0214)	(0.0055)	(0.0059)	(0.0229)	(0.049)	(0.161)	(0.154)
Observations	20,754	20,754	20,754	20,754	20,117	5,705	6,119
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates compare changes in high- and medium-exposure firms between 2003–2006 and 2007–2012. All workers are aged 66 or above in the current year and earn at least one price base amount from the firm that year. Retention requires earnings at the same firm to meet the preceding year's threshold too. Threshold crossing means the worker had a wage record there but earned below the threshold in the preceding year. Hiring means no wage record at that firm in the preceding year, including a return after a gap. All worker–firm relationships are kept. Firm-years with accounts but no matching wage records enter with zero counts. Columns (1)–(3) are linear probability models for having at least one worker in each category. Column (4) regresses the retention indicator minus the hiring indicator on the same variables, using the same sample. Columns (5)–(7) are Poisson fixed-effects models, estimated on separate samples for each outcome and excluding firms whose count is zero in every sample year. All models include firm and year effects and are weighted by the logarithm of CPI-adjusted 2006 sales, measured in thousands of SEK. Firm-clustered standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

For threshold crossings, the probability estimates are positive both before and after 2006 (Panel C), but the estimated change between the pre- and post-reform periods is small. The hiring estimates show no clear change at the reform (Panels E and F). Panel G reports the yearly differences between the retention and hiring probability estimates.

Total Employment. Total employment is the number of workers aged 19 and above earning at least one price base amount from the firm during the year. The baseline coefficient for total employment is −0.056, a decline of about 5 percent that is significant only at the 10 percent level. The annual estimates are negative from 2007 onward and generally become more negative over time (Figure 9). The pre-reform coefficients are negative and imprecise, and a joint test does not reject that they are zero ($p = 0.28$). Note, however, that these estimates do not establish that older workers displaced younger workers. Because firms were grouped by the age composition of their 2006 payroll, later changes in employment at different ages may partly reflect that grouping, for example a return toward a firm's usual workforce (Section 6.1), as well as substitution in response to the reform. Comparing high- and medium-exposure firms cannot separate these explanations (Saez et al., 2019).

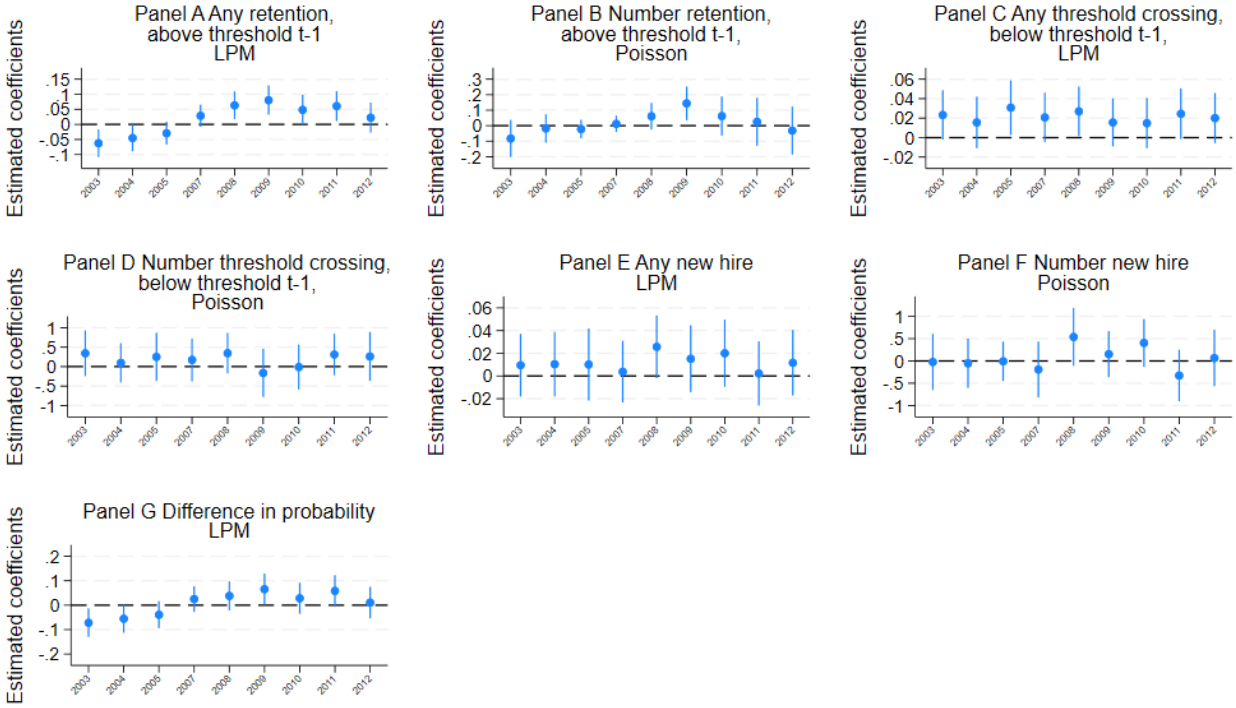
3.5 Interpreting the Older-Worker Employment Results

Section 6 examines how the choice of comparison groups and differences in their prior paths affect the interpretation of both employment and output. Here we consider the restriction to firms observed throughout the period, possible anticipation, and concurrent tax changes.

Restricting the sample to firms observed in every year from 2003 to 2012 gives similar employment results (Figure A.2). The reform may itself affect which firms remain observed and therefore meet this restriction. Section 4.3 reports the corresponding checks for firm performance.

Anticipation. The reform was announced in October 2006, leaving only a few months for employers to respond before the end of the year used to measure exposure. Any such response could affect firms'

Figure 8: Yearly Retention, Threshold-Crossing, and Hiring Estimates.



Notes: Coefficients compare high- with medium-exposure firms, with groups fixed at their 2006 assignment. Definitions and specifications follow Table 5. Panels A, C and E report linear probability estimates for retention, threshold crossing and hiring, respectively. Panels B, D and F report the corresponding Poisson count estimates. Panel G regresses the retention indicator minus the hiring indicator on the same yearly terms as Panels A and E. Estimates are relative to 2006; bars show 95% confidence intervals for the other years. Joint tests of the 2003–2005 coefficients give $p = 0.067$ (A), 0.385 (B), 0.168 (C), 0.684 (D), 0.899 (E), 0.998 (F) and 0.121 (G). Standard errors are clustered at the firm level.

2006 payroll and therefore which firms entered the sample or how they were classified. Joint tests do not reject that the pre-reform coefficients for older-worker employment are zero (Figure 7), which is reassuring but does not rule out anticipation.

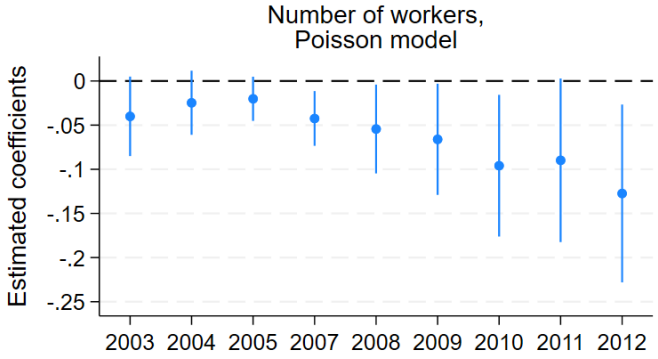
Concurrent Tax Changes. The payroll tax cut coincided with the introduction of the EITC in 2007.[20] At a given gross wage, the credit raised older workers' take-home pay and could encourage them to remain employed. It was available to older workers in both groups of firms, but the size of the credit depended on earnings. If earnings differed between the groups, the credit could have encouraged continued employment more in one group than in the other. Interpreting the employment comparison as employers' response to lower labor costs therefore requires that changes in labor supply affected the two exposure groups similarly.[21]

The youth payroll tax cut of 2007 is a further concurrent change. For this reason, we restrict the sample to firms whose payroll to young workers was small relative to their payroll to older workers (Section 1). These firms may nevertheless have had some exposure to the youth payroll tax cut.

[20]The New Start Jobs program (*Nystartsjobb*), introduced in January 2007, subsidized hiring from specified groups outside employment. A different response to that program across exposure groups would also affect the comparison, even though eligibility was not determined by the firm's older-worker payroll share.

[21]Equal individual responses to the EITC need not imply equal effects on the firm's probability of employing any older worker. If a firm has n older workers whose retention decisions are independent and each remains employed with probability p, the probability of retaining at least one is $1-(1-p)^n$, whereas the expected number retained is np. A common change in p can therefore change the first probability by different amounts across firms. When p is already high, firms with several older workers have less scope for that probability to rise than firms with one.

Figure 9: Dynamic Effects of the 2007–08 Reform on Total Employment.



Notes: Coefficients compare high- with medium-exposure firms, with groups fixed at their 2006 assignment; specifications follow Table 3. Poisson QMLE estimates for the number of workers aged 19 and above, with 95% confidence intervals. The reference year is 2006. A joint test of the 2003–2005 coefficients does not reject ($p = 0.28$). Standard errors are clustered at the firm level.

Restricting the sample to firms with no payroll to young workers leaves the older-worker employment estimates positive (Figure A.1).

4 Firm Performance

The employment estimates raise a further question. Do firms that employ more older workers also expand their business? At unchanged wages, a payroll tax cut can make expansion worthwhile, but it can also change the mix of inputs used to produce a given level of output. More older-worker employment therefore need not be accompanied by proportionately higher sales or employment at other ages. Capital, too, can rise when it complements older labor and fall when it substitutes for it. We examine sales, value added, assets, and profits to see how the employment pattern relates to the firms' wider activity.

4.1 Sales, Value Added, and Assets

Our measures of firm performance come from the FEK accounts. Sales measure revenue from goods and services, value added is output less intermediate inputs, and total assets include physical and financial assets. Monetary amounts from the accounts are adjusted to 2023 prices using the consumer price index. These outcomes describe firms' recorded business activity, rather than physical production or aggregate output.

We apply the comparison described in Section 3.2 using ordinary least squares, with each outcome expressed in logarithms. These regressions include only firm-years with positive values. About 2 percent of firm-years lack positive sales and about 4 percent lack positive value added. We report the coefficients in log points.

Our first result on firm performance is that sales and value added decline in high-exposure firms relative to medium-exposure firms after the reform. The pooled log coefficients in Table 6 are −0.15 for sales, −0.16 for value added, and −0.08 for total assets, corresponding to differences of approximately 14, 15, and 8 percent. Figure 10 shows that the sales and value-added differences emerge in 2007 and persist through 2012, whereas assets decline more gradually. The output differences thus begin before the 2008–09 recession. Joint tests of the pre-reform coefficients do not reject zero in any of the six panels at the 10 percent level.

Table 6: Baseline Firm Performance Results, 2007–08 Reform.

Outcome	Coefficient (DiD)	Std. Error	Pre-reform Average	Obs.
Sales (ln, positive values)	-0.15***	(0.04)	8.50	20,358
Sales (Poisson, zeros retained)	-0.17**	(0.07)	8,832	20,749
Zero sales (LPM)	0.008	(0.005)	0.005	20,754
Value added (ln, positive values)	-0.16***	(0.03)	7.58	19,954
Value added / 2006 sales	-0.065***	(0.011)	0.46	20,754
Total assets (ln)	-0.08***	(0.03)	8.06	20,750

Notes: Each coefficient compares the change between 2003–2006 and 2007–2012 in high-exposure firms with the change in medium-exposure firms. Standard errors clustered at the firm level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sales, value added, and total assets are CPI-adjusted and in thousands of SEK. The log outcomes use firm-years with positive values. The Poisson model for sales retains zero sales but not negative values. 'Zero sales' is an indicator for zero recorded sales, estimated with a linear probability model. 'Value added / 2006 sales' divides value added by the firm's 2006 sales, keeps zero and negative values, and is winsorized at the 1st and 99th percentiles within each year; its coefficient is in units of 2006 sales. Pre-reform average refers to the average value for high-exposure firms during 2003–2006, weighted like the regressions; for the Poisson sales row it is in thousands of SEK. All regressions include firm and year fixed effects and are weighted by the logarithm of CPI-adjusted 2006 sales, measured in thousands of SEK.

We complement the log results with estimates that retain zero sales or nonpositive value added (Table 6). The Poisson estimate for sales, retaining zeros, is −0.17 (standard error 0.07). Following Chen and Roth (2024), we also examine separately whether firms record zero sales, using a linear probability model. The estimated increase is 0.8 percentage points, compared with a pre-reform share of 0.5 percent, but the estimate is imprecise (standard error 0.5 percentage points). The ratio of value added to the firm's 2006 sales, winsorized within year, retains zero and negative value added. Its estimated decline of 0.065 (standard error 0.011) equals 6.5 percent of 2006 sales, or about 14 percent of the ratio's pre-reform mean of 0.46.

4.2 Profits

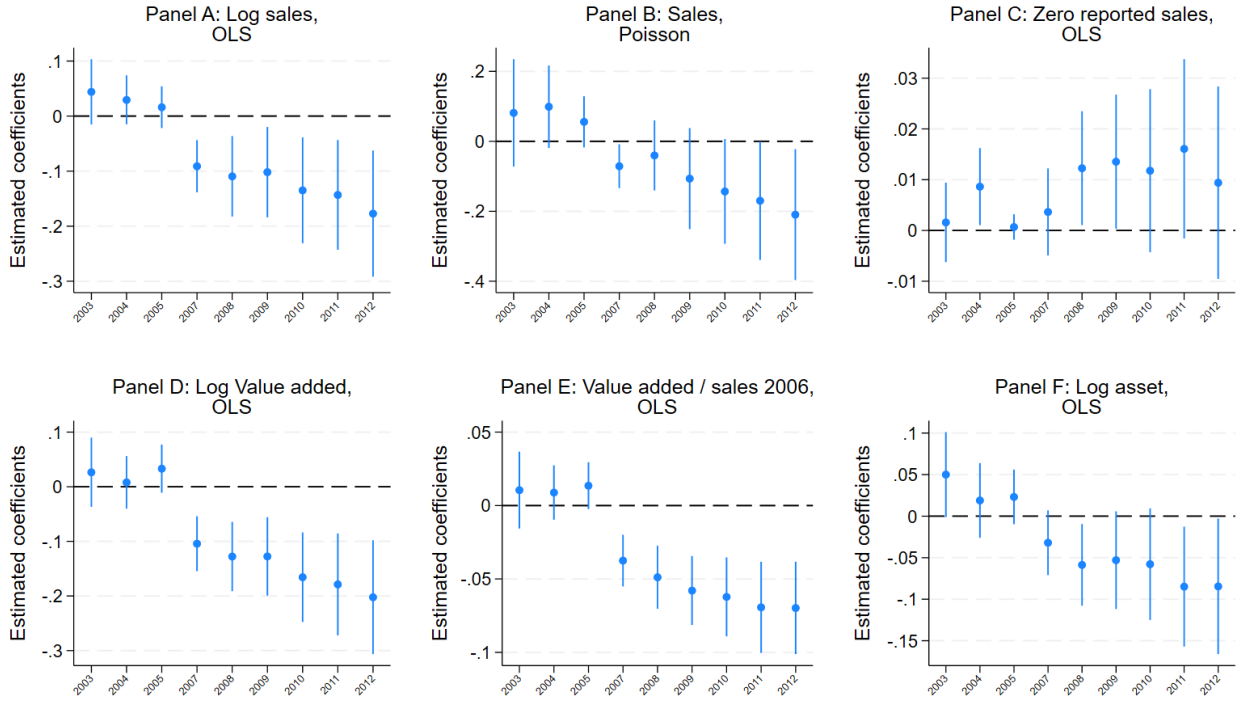
We next examine whether the declines in sales and value added are accompanied by lower profits, or whether savings in payroll costs offset the fall in output. If wages, other input prices, and the revenue function remain unchanged, the payroll tax cut raises profits. A firm could keep its previous inputs and save the tax on the older-worker wages it already paid. A profit-maximizing firm can do at least as well by adjusting its inputs. The argument does not give a general prediction for sales, since a change in the mix of inputs can also change sales. Wage and price adjustments and changes in competition can affect both profits and sales.

Profits can be negative, so Figure 11 reports estimates in levels (Panel A) and as a share of value added (Panel B). The two measures can move differently. Profits in levels would remain unchanged if lower labor costs offset the fall in value added, whereas the profit share would remain unchanged if profits fell in the same proportion as value added. The annual estimates are imprecise: in nearly every year, the confidence intervals include both increases and declines. They therefore provide little basis for distinguishing these possibilities or for determining how firms, workers, and customers shared the tax saving.

4.3 Alternative Samples

We first examine firms that report sales, value added and total assets in every year from 2003 to 2012: 863 high-exposure and 889 medium-exposure firms. Log sales and log value added decline after the

Figure 10: Dynamic Effects of the 2007–08 Reform on Firm Performance.



Notes: Coefficients compare high- with medium-exposure firms, with groups fixed at their 2006 assignment. Panels A, D, and F show OLS estimates for log sales, log value added, and log total assets on positive observations. Panel B shows Poisson estimates for sales, retaining zeros but excluding negative values. Panel C shows linear probability estimates for recording zero sales. Panel E shows OLS estimates for value added divided by 2006 sales, winsorized at the 1st and 99th percentiles within year. All models include firm and year fixed effects and are weighted by the logarithm of firms' 2006 sales. The reference year is 2006, and intervals are 95% confidence intervals based on standard errors clustered by firm. Joint pre-reform test p-values for Panels A–F are 0.510, 0.382, 0.105, 0.420, 0.427, and 0.175, respectively; Panel B uses a Wald test and the others use F-tests.

reform. The Poisson sales estimates are negative after the reform but imprecise (Figure A.2). Their pre-reform point estimates decline from 0.13 in 2003 to 0.07 in 2005, although a joint test does not reject that the pre-reform coefficients are zero ($p = 0.29$). Restricting the sample in this way does not eliminate concerns about selection, because the reform may itself affect which firms remain observed.

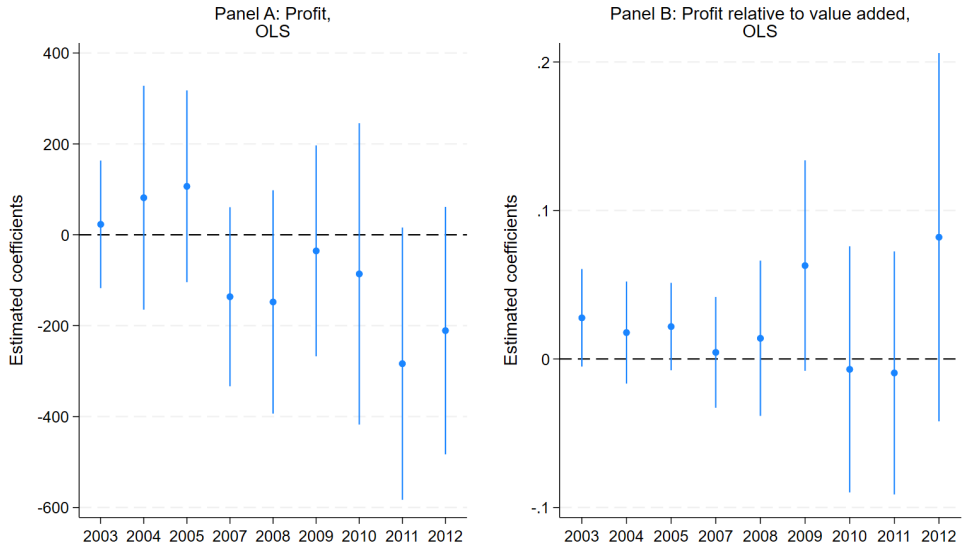
This comparison does not resolve whether high exposure identifies firms that would have contracted even without the tax cut, or whether the reform affected which firms remain in the data. Section 6 brings together the group-definition and differential-trend checks; Section 5.3 discusses the implications for senior-led firms.

5 The Organizational Position of Older Workers

An eligible older worker may also be the person who runs the firm. That worker's salary is then part of the payroll share that measures exposure, and the decision to keep working can be closely tied to the firm's activity. We therefore divide firms according to the age of their registered operative manager in 2006. We call firms with a manager aged 66 or above "senior-led" and those with a younger manager "younger-led." Among firms with a recorded manager age, 622 of the 1,100 high-exposure firms and 311 of the 1,102 medium-exposure firms are senior-led. We compare high- and medium-exposure firms separately within each group, keeping the manager classification fixed throughout the analysis.[22]

[22]These comparisons include only firms for which we can identify a single manager and determine that person's age. The remaining firms stay in the full-sample analysis.

Figure 11: Effects of the 2007–08 Reform on Profits.



Notes: OLS fixed-effects estimates with 95% confidence intervals. The estimates use the same weights as the baseline regressions in Table 3. Panel A shows profits in levels, in thousands of SEK at 2023 prices; Panel B shows profits as a share of value added. The reference year is 2006. Standard errors are clustered at the firm level. The variable "profit relative to value added" measures profit divided by value added (firm-years with zero value added are excluded, and negative values are retained), and then is winsorized at the 1st and 99th percentiles within each year. Panels A and B have 20,754 and 20,725 observations. Panel B has fewer observations because it excludes firm-years with zero value added. The p-values from the joint tests for Panels A and B are 0.787 and 0.370, respectively.

5.1 Employment of Managers and Other Workers

We first ask whether the employment gains extend beyond the manager. Managers count as employed when their earnings meet the same threshold as other workers. As Section 2.2 shows, 547 of the 609 senior-led firms with exactly one counted older worker in 2006 had the manager as that worker. We therefore compare the employment results with and without the manager.[23]

Among senior-led firms, the probability of employing at least one older worker rises by 13.0 percentage points more in high-exposure firms than in medium-exposure firms (Table 7, Panel A). This measure includes the manager's own employment. The corresponding increase among younger-led firms is 7.4 percentage points. Both probability estimates are statistically significant at the 1 percent level. The estimated number of older workers also rises more in high-exposure firms within both manager groups, but neither count estimate is statistically significant at the 10 percent level.

For senior-led firms, the estimates also suggest that older-worker employment had already risen more in high-exposure firms than in medium-exposure firms between 2003 and 2006 (Figure 12). This earlier increase appears in both employment measures. Joint tests of whether the pre-reform coefficients are zero yield p-values of 0.083 for the probability of employing an older worker and 0.069 for the number of older workers. Both tests reject at the 10 percent level but not at the 5 percent level. The evidence of an earlier increase is therefore suggestive, and the increase after the reform may partly continue it.

For each senior-led firm, we remove the person registered as its manager in 2006 from the employment count in every year. The estimated increase in the probability that a firm employs an older worker is then 1 percentage point, with a standard error of 3 percentage points, compared with 13.0 percentage points when the manager is counted. The estimate excluding the manager is imprecise (Table A.4).

[23]The manager register identifies a management role, not ownership. If a manager owns shares in a closely held company, changes between salary and dividends can affect an earnings-based employment measure without a new employment relationship.

Table 7: Effects of the 2007–08 Reform by Firm Management Type.

Outcome	Senior-led	Younger-led	Triple difference
Panel A: Employment outcomes			
Any older worker	0.130*** (0.034) N = 8,637	0.074*** (0.028) N = 11,970	–
Number of older workers	0.103 (0.073) N = 8,627	0.081 (0.064) N = 11,898	–
Total employment	-0.055 (0.039) N = 8,637	-0.028 (0.044) N = 11,970	–
Panel B: Firm outcomes			
Sales (ln)	-0.213*** (0.055) N = 8,428	-0.051 (0.047) N = 11,799	-0.16** (0.07) N = 20,227
Sales (Poisson)	-0.269*** (0.086) N = 8,636	-0.018 (0.082) N = 11,967	-0.25** (0.12) N = 20,603
Zero sales	0.014 (0.010) N = 8,637	-0.007 (0.006) N = 11,970	–
Value added (ln)	-0.215*** (0.046) N = 8,242	-0.096** (0.042) N = 11,589	-0.12* (0.06) N = 19,831
Value added / 2006 sales	-0.069*** (0.018) N = 8,637	-0.042*** (0.015) N = 11,970	-0.03 (0.02) N = 20,607
Total assets (ln)	-0.059 (0.043) N = 8,637	-0.092** (0.036) N = 11,967	–

Notes: Standard errors clustered by firm are in parentheses; N is the number of firm-years. Senior-led firms have a registered manager aged 66 or above in 2006 and younger-led firms a manager below 66; the classification remains fixed, and firms with missing manager age are excluded. Outcomes and estimators are defined as in Tables 3 and 6; all models include firm and year fixed effects and are weighted by the logarithm of firms' 2006 sales. The triple difference is the coefficient on High Exposure × Senior-Led × Post in the pooled specification. A dash means that this coefficient is not reported. Table A.3 reports the remaining interactions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Employment gains also appear in younger-led firms, where the older workers on the 2006 payroll were employees other than the manager. The probability estimates turn positive in 2007, but the increase is smaller and imprecise in 2012 (Figure 13). Because the classification remains fixed in 2006, these estimates also pick up managers who reach 66 after the reform (Section 2.2). Their continued employment can contribute to the older-worker employment measure, so these estimates do not isolate gains among other employees.

For senior-led firms, the estimate excluding the manager provides little evidence of an increase in the probability of employing an older worker other than the manager. These comparisons do not establish how much of the overall increase in older-worker employment comes from other employees.

5.2 Differences in Firm Performance

We next examine whether the declines in firm performance differ between the two manager groups. Among senior-led firms, sales and value added each fall by about 19 percent in high-exposure firms relative to medium-exposure firms (Table 7, Panel B). Among younger-led firms, the estimated sales decline is about 5 percent and imprecise, whereas value added falls by about 9 percent, significant at the 5 percent level.

We test whether these declines differ between manager groups by comparing their high-versus-medium exposure estimates in a triple-difference specification. The triple-difference estimate is -0.16 log points for sales, significant at the 5 percent level, and -0.12 log points for value added, significant at the 10 percent level (Table 7, Panel B, last column). These estimates support a larger sales decline

in high-exposure relative to medium-exposure firms among senior-led firms than among younger-led firms. The evidence that the value-added decline differs between the two manager groups is weaker. Table A.3 reports the remaining interactions. When the yearly effects are allowed to differ by firm size within each manager group, the triple-difference estimates are −0.09 (standard error 0.07) for sales and −0.06 (standard error 0.06) for value added, neither significant at the 10 percent level (Section 6.5). The evidence that the declines differ between manager groups is therefore weaker once firm size is taken into account.

The larger sales decline among senior-led firms also appears in the Poisson estimates, which retain firm-years with zero sales. These estimates show little change among younger-led firms, and the difference between groups is significant at the 5 percent level.[24] Winsorized value added relative to 2006 sales falls significantly in both groups, but the difference between groups is imprecise. The estimated decline in assets is about 6 percent among senior-led firms, but this estimate is imprecise. Among younger-led firms, the estimated decline is about 9 percent and statistically significant at the 5 percent level. The declines in value added and assets among younger-led firms show that weaker firm performance is not confined to senior-led firms.

In senior-led firms, the log-sales and log-value-added estimates are close to zero before the reform and negative from 2007 through 2012 (Figure 12, Panels D and G). The pooled log-sales estimate for senior-led firms therefore reflects a relative decline sustained across the post-reform period. The Poisson sales estimates for senior-led firms (Panel E) decline over the pre-reform years, from a positive value in 2003 toward zero in 2005, although a joint test does not reject that the pre-reform coefficients are zero ($p = 0.572$). These estimates do not establish an earlier downward trend. Among younger-led firms, the log-sales estimate becomes more negative in 2012, while the log-value-added estimates decline gradually over the post-reform period (Figure 13, Panels D and G).

5.3 Manager Age, Payroll Composition, and Firm Prospects

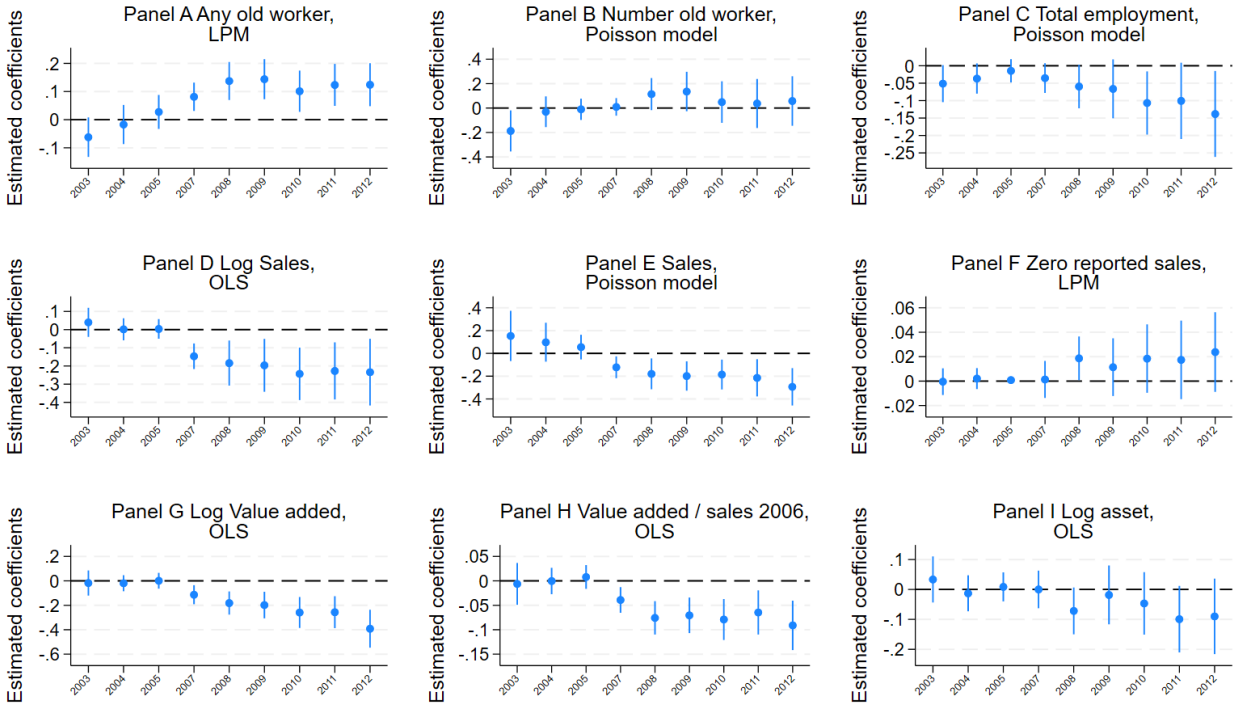
We next consider why, in the baseline comparison, the output decline in high-exposure relative to medium-exposure firms is larger among senior-led firms. Answering this question requires considering both manager age and firms' initial payroll composition. A high older-worker payroll share may identify businesses that were already contracting. The tax cut may also have allowed some firms to keep operating at a smaller scale. These explanations, which concern senior-led firms, can operate together. The declines in value added and assets among younger-led firms remain unexplained by our analysis.

We first ask whether senior-led firms fall behind younger-led firms in output within the medium-exposure group. This checks whether output diverges by manager age even where older workers account for a smaller share of payroll. Within that group, the estimated differences between senior-led and younger-led firms in their post-reform changes are close to zero for both log sales and log value added, with standard errors of 0.05 and 0.04, respectively (Table A.3, row Senior × Post). This comparison does not determine how senior-led firms with a high older-worker payroll share would have evolved without the tax cut.

Consider a firm whose only older worker is its manager. If the firm reduces its payroll for younger workers while keeping the manager's salary unchanged, the older-worker share of payroll rises. Using this share to classify firms in 2006 could therefore put a business that was already shrinking into the high-exposure group. Its sales could continue to fall after the reform even without the tax cut. The older-worker share would then reflect the firm's earlier decline as well as its potential tax saving.

[24] The senior-led sales decline remains statistically significant in the additional specifications. On the same firm-years as the log-sales estimate, a Poisson model on positive sales gives −0.26 (standard error 0.09); capping sales at the 1st and 99th percentiles within year and retaining zeros gives −0.16 (0.05). The corresponding younger-led estimates are −0.03 (0.08) and 0.00 (0.04).

Figure 12: Dynamic Effects of the 2007–08 Reform in Senior-Led Firms.



Notes: The sample is restricted to firms whose manager was aged 66 or above in 2006; this classification remains fixed. Coefficients compare high- with medium-exposure firms within this group, with exposure groups fixed at their 2006 assignment. Older-worker employment includes managers aged 66 or above who meet the employment threshold. The top row shows, from left to right, the probability of employing any older worker (linear probability model), the number of older workers (Poisson), and total employment (Poisson). The middle row shows log sales on positive values (OLS), sales in levels (Poisson, retaining zeros), and the probability of recording zero sales (linear probability model). The bottom row shows OLS estimates for log value added on positive values, value added divided by 2006 sales (winsorized at the 1st and 99th percentiles within year), and log total assets. All specifications include firm and year effects and are weighted by the logarithm of firms' 2006 sales. The figure shows 95% confidence intervals and uses 2006 as the reference year. Standard errors are clustered at the firm level. Joint pre-reform test p-values (Panels A–I): 0.083, 0.069, 0.271, 0.595, 0.572, 0.739, 0.905, 0.855, 0.361.

Temporary changes in an older owner-manager's salary provide another way for business conditions in 2006 to affect both the payroll share and subsequent sales, as discussed in Section 6.1.[25]

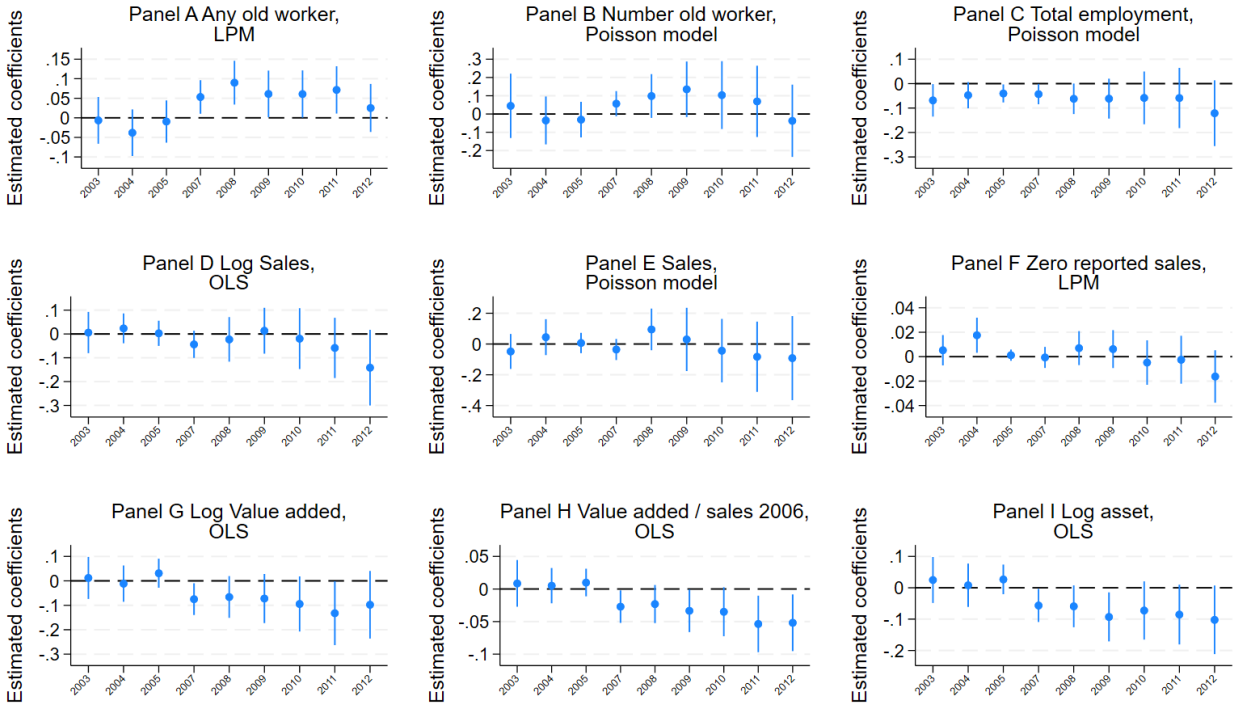
The comparison around 2013 in Section 6.3 finds another relative decline in senior-led log sales without a new payroll tax cut, although the corresponding Poisson estimate is positive and imprecise, and the comparisons with other grouping years in Section 6.4, including 2004, before the reform, find similar declines. These results show that a relative log-sales decline can recur without a new cut; they do not establish how much underlying contraction contributes to the 2007–08 findings. Section 6.3 also examines the implications for older-worker employment across all firms.

The tax cut could also affect which firms remain in our data. Lower payroll costs may allow a business to keep operating while its sales decline.[26] If this happens more often among high-exposure firms than among medium-exposure firms, the comparison would include additional years of declining sales for those firms. This could contribute to the estimated sales decline even if the reform did not reduce sales in firms that would have kept operating either way. Our main sample includes firms in the years for which we observe their accounts. Requiring firms to appear in every year would not remove this concern, because the reform may itself affect which firms remain in the data. Nor can we use missing

[25] Comparing firms whose managers are the same age would only partly address this concern. One firm may be expanding while another is shrinking. Firms still led by the oldest managers may also differ from firms whose managers stepped down earlier.

[26] If the manager also owns the firm, the EITC could increase the return to continuing the business.

Figure 13: Dynamic Effects of the 2007–08 Reform in Younger-Led Firms.

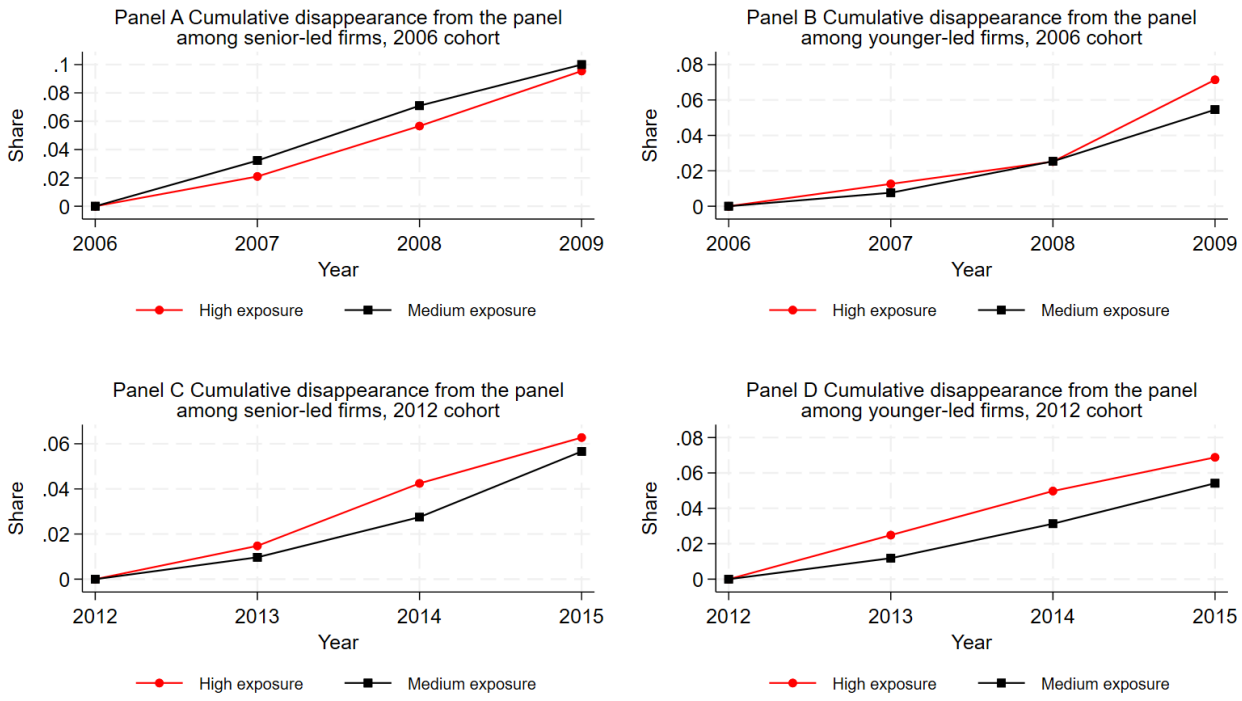


Notes: The sample is restricted to firms whose manager was below age 66 in 2006; this classification remains fixed. Coefficients compare high- with medium-exposure firms within this group, with exposure groups fixed at their 2006 assignment. Older-worker employment includes managers aged 66 or above who meet the employment threshold, including managers who reach 66 after 2006. The top row shows, from left to right, the probability of employing any older worker (linear probability model), the number of older workers (Poisson), and total employment (Poisson). The middle row shows log sales on positive values (OLS), sales in levels (Poisson, retaining zeros), and the probability of recording zero sales (linear probability model). The bottom row shows OLS estimates for log value added on positive values, value added divided by 2006 sales (winsorized at the 1st and 99th percentiles within year), and log total assets. All specifications include firm and year effects and are weighted by the logarithm of firms' 2006 sales. The figure shows 95% confidence intervals and uses 2006 as the reference year. Standard errors are clustered at the firm level. Joint pre-reform test p-values (Panels A–I): 0.452, 0.508, 0.104, 0.830, 0.625, 0.0904, 0.522, 0.816, 0.643.

accounts to identify closures: a firm may disappear from the data because of a merger, reorganization, reporting gap, or change in legal form, and even persistent absence does not establish closure. We therefore cannot use these absences alone to establish that the tax cut kept firms in business.

Figure 14 follows disappearance separately for senior-led and younger-led firms in the 2006 and 2012 cohorts. The panels compare high- and medium-exposure firms within each manager group, using the same follow-up windows as Figure 4.

Figure 14: Cumulative Disappearance from the Panel by Manager Age.



Notes: Panels A and B follow the 2006 cohort over 2006–2009, checking subsequent appearances through 2012. Panels C and D follow the 2012 cohort over 2012–2015, checking through 2018. Panels A and C show senior-led firms, whose registered manager is aged 66 or above in the selection year; Panels B and D show younger-led firms, whose manager is below 66. These classifications remain fixed within each cohort. Disappearance begins in the year after a firm's last appearance in the relevant panel; firms that disappear and return within the observation period are excluded. Each firm receives equal weight. Table A.2 reports the starting counts and the differences between exposure groups with their standard errors. Different disappearance paths across the cohorts do not establish that the tax cut delayed disappearance, and missing accounts do not establish closure.

We find no consistent evidence that high-exposure firms are less likely than medium-exposure firms to disappear from the accounts data. By 2009, 8.5 percent of the high-exposure firms selected in 2006 and 7.2 percent of the medium-exposure firms had disappeared; among senior-led firms, the shares were 9.5 and 10.0 percent (Table A.2). For these senior-led firms, the difference is −0.5 percentage points with a standard error of 2.1, so a 95 percent confidence interval runs from about −4.5 to 3.6 percentage points. Among firms selected in 2012, when no tax cut followed, high-exposure firms disappeared slightly more often than medium-exposure firms. A firm missing from the accounts has not necessarily closed.

These comparisons and the estimates for senior-led firms therefore leave open whether the tax cut allowed some of these firms to keep operating at a smaller scale, whether their output would have fallen without the reform, or both.

6 Group Definitions and Placebo Comparisons

The preceding estimates compare firms selected and grouped using their payroll in 2006. A high older-worker payroll share can reflect how a firm's employment and business were already developing, as well as its potential saving from the tax cut. This section examines whether the employment and output differences could also have arisen without the tax cut. The requirement is the same for both: without the reform, the groups would have followed similar paths in the outcome being studied, under the model used to estimate it.

6.1 Selection Based on a Single Year

Possible mean reversion in payroll shares, discussed in Section 3.2, can affect the employment and output estimates. Saez et al. (2019, p. 1746) discuss this concern for firms grouped by their young-worker payroll share. A firm with unusually many older workers in 2006 might later return to its normal workforce, which would reduce the estimated increase in older-worker employment. A firm whose payroll for workers below age 66 was temporarily low because it was operating below its usual scale might later recover and raise its sales, which would reduce the estimated decline in output. Bias in the output estimate could also run in the other direction: an older owner-manager in a closely held firm might draw a larger salary during an unusually good year. A return to normal business conditions would then lower both sales and the older-worker payroll share, potentially producing an output decline without a reform effect. The direction of bias in the output estimates from using 2006 payroll is therefore not known in general.

Salary choices under Sweden's 3:12 rules could also affect the payroll used to form the groups. Cash wages paid in 2006, including owners' salaries, entered the wage-based dividend allowance for 2007; eligibility depended on the owner or a close relative meeting a salary requirement in 2006.[27] Salary responses could therefore change a firm's older-worker payroll share without a corresponding change in work. The rules do not imply that salaries necessarily rose, and our manager register does not establish ownership or whether this occurred in the sample.

6.2 How Quickly Do Payroll Shares Change?

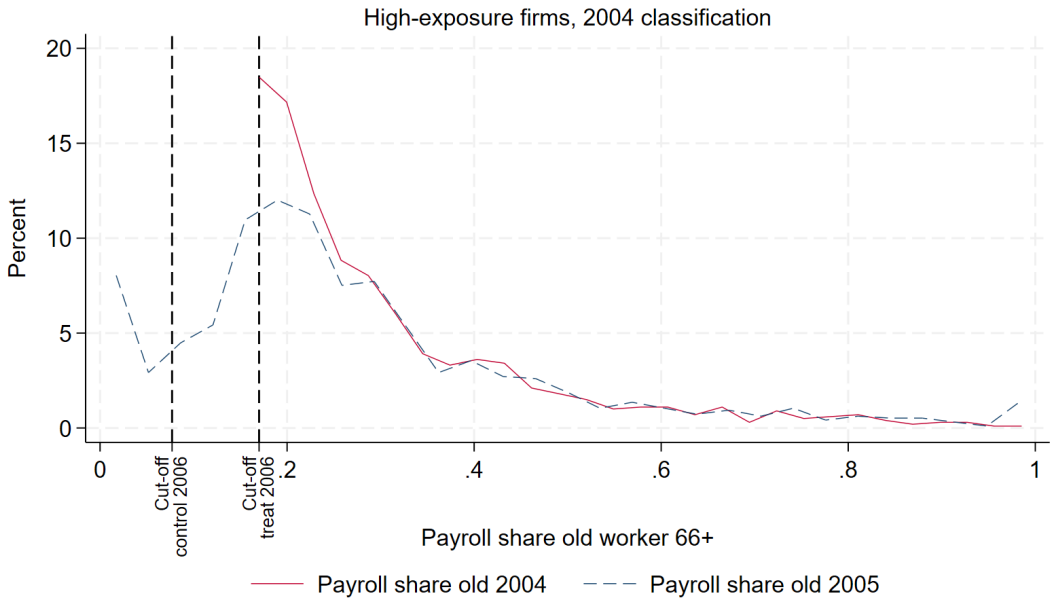
Using payroll from before 2006 avoids forming groups from wages paid after the reform announcement, but those earlier wages may describe jobs that ended before the tax cut. This concern is particularly relevant for workers already aged 66 or above, who may retire before the reform takes effect. In a firm with only one or two older workers, a single departure can substantially change the older-worker payroll share. Averaging several earlier years smooths temporary changes but still includes wages from these earlier jobs.

Figure 15 follows firms classified as high exposure in 2004 or 2005 and compares their older-worker payroll shares with those one year later. In both panels, a larger proportion of firms has a low share in the following year. The change is therefore not unique to 2005–2006: earlier payroll shares can become less representative even within a year. The vertical lines show the main analysis's 2006 cutoffs for reference.

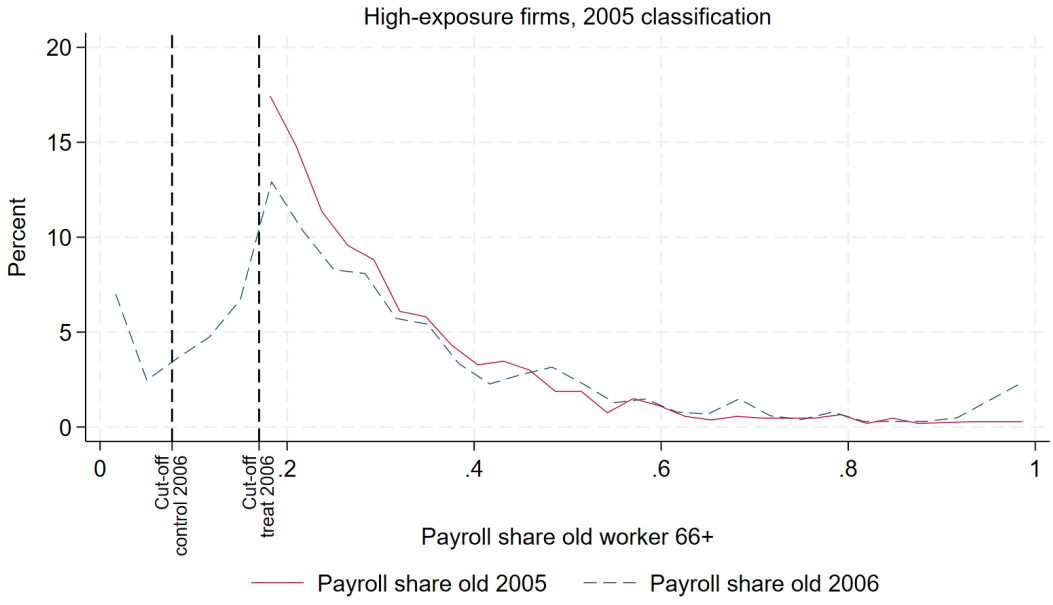
These changes can reflect older workers leaving, changes in their earnings, or changes in other workers' payroll; workers reaching age 66 also change the share. The figure does not distinguish these explanations or establish retirement as the cause. Selecting firms with a high share can also pick out a temporary peak. That concern applies equally to the 2006 groups and motivates the placebo comparisons below. The figure illustrates a limitation of earlier measures of exposure; it does not establish that the 2006 comparison identifies the reform's effect. As discussed above, salary choices under the 3:12 changes could affect payroll in 2005 as well as in 2006.

[27] The 2006 reform included active owners' salaries in the wage base and changed the salary requirement; further changes effective in 2007 increased the allowance. See Prop. 2005/06:40, Sections 5.2–5.3 and Chapter 57, Sections 17 and 19 of the proposed statutory text, and Prop. 2006/07:1, Section 5.5.1.2.

Figure 15: Older-Worker Payroll Shares One Year after Grouping Firms.



(a) Firms grouped in 2004, followed into 2005.



(b) Firms grouped in 2005, followed into 2006.

Notes: Each panel follows firms classified as high exposure in its grouping year. The solid red line shows the percentage of firms in each payroll-share interval in that year; the dashed blue line shows the corresponding percentages in the following year. Lines connect the interval values without smoothing. The share is payroll costs for workers aged 66 or above in the year measured divided by the firm's payroll costs for workers aged 19 and above. Payroll costs include gross wages and employer contributions. Both panels show the main analysis's 2006 medium- and high-exposure cutoffs for reference. Falling below one of these cutoffs does not establish membership of a group in a separately selected sample.

6.3 The Comparison Around 2013

We repeat the comparison using firms selected and classified in 2012, treating 2013 as an artificial reform date. No new payroll tax cut occurred in 2013. Table A.5 compares 2013–2014 with 2009–2012. Firms are assigned to the highest and next-highest quartiles of their 2012 older-worker payroll share,

and manager type is also fixed in 2012. This rebuilds the groups in a later year; it is not a change in the reference year for the original 2006 groups.

Across all high- and medium-exposure firms, the probability of employing an older worker rises by 3.9 percentage points more in high-exposure firms (standard error 1.4 percentage points; Table A.5, Panel B). For this outcome, a joint test rejects that the coefficients for 2009–2011 are zero, with 2012 as the reference year ($p = 0.002$). The estimate for the number of older workers is imprecise (0.027, standard error 0.031). The probability difference therefore appears without a new tax cut, but this does not establish that exposure ranking caused it or identify the direction or size of bias in the 2007–08 probability estimate. Panel B of Table A.10 shows that the employment-share gap narrowed from 6.7 percentage points in 2009 to 0.6 points in 2012, before widening to 6.0 points in 2013 and 10.2 points in 2014. The widening after 2012 therefore follows a narrowing before the year used to form the groups.

Among senior-led firms, the log-sales estimate implies a decline of about 14 percent in high-exposure firms relative to medium-exposure firms. The Poisson estimate for the same firm-years with positive sales is, however, positive and imprecise (0.087, standard error 0.080). For the 2007–08 reform, senior-led sales decline under both methods on the same firm-years with positive sales: the log-sales estimate is -0.21 and the Poisson estimate using only positive sales is -0.26 (standard error 0.09), close to the Poisson estimate that retains zero sales (Table 7, Panel B). The two comparisons also cover post-periods of different length. Senior-led value added also falls in the 2013 comparison, but joint tests reject that its 2009–2011 coefficients are zero, at the 10 percent level for log value added ($p = 0.073$) and at the 1 percent level for value added divided by 2012 sales ($p = 0.007$). The younger-led estimates for 2013 are smaller and imprecise. This comparison does not establish why output fell after the 2007–08 reform.

A pre-period test that rejects the hypothesis of zero coefficients provides evidence that the groups' paths differ before the artificial reform date under that specification. It raises a concern about comparability, but does not establish that ranking caused the difference or provide a bias estimate to subtract from the original result. A test that does not reject cannot establish that the groups would have followed parallel paths without the reform. A small or imprecise placebo estimate does not validate the original design.

The relevant parallel-trends restriction differs across models. After allowing for firm and year effects, a linear model in levels requires the groups' average outcomes to have changed by the same amount without the reform. A regression on logs requires the same of their average log outcomes. Poisson requires their average outcomes to have changed in the same proportion. Their pre-period tests can therefore disagree. Within the 2013 comparison, the log-sales estimates and the Poisson estimates using only positive sales concern different summaries of the outcome on the same observations, so excluding zeros does not resolve their disagreement.

6.4 Placebo Comparisons with Other Grouping Years

We repeat the comparison by manager type for firms selected and grouped in 2004, before the reform, and in each year from 2010 to 2013. No new payroll tax cut for older workers took effect in the two years following any of these grouping years. As in the main analysis, firms are selected, assigned to exposure groups and classified by manager type in the grouping year, and these classifications remain fixed. For each grouping year, we compare outcomes in the next two years with those in the four years ending in that year. We also re-estimate the 2007–08 reform over 2003–2008, using the 2006 groups, so that all comparisons use the four years up to and including the grouping year and the two years after it. Tables A.6 and A.7 report the estimates. The 2004 groups are the earliest that can be split by manager type, because the manager register begins in 2004; their window ends in 2006, the year in which the 2007 cut was announced. The comparison with 2012 groups is the comparison around 2013 above, estimated separately by manager type. The right to remain employed until age 67 took effect in September 2001, with earlier retirement ages in existing collective agreements allowed to continue

through 2002, within the pre-period for the 2004 groups, and employer contributions for young workers changed in 2015, within the period covered by the 2013 groups.

Among senior-led firms, log sales and log value added fall in every comparison. Log sales decline by about 8 to 14 percent in the five placebo comparisons, compared with about 16 percent for the reform, and log value added declines by about 8 to 13 percent, compared with about 12 percent for the reform. All these estimates are statistically significant at the 5 percent level or better, except the value-added decline with 2004 groups, which is significant at the 10 percent level. With 2004 groups, log sales decline by about 13 percent, so a decline of this size also appears before the reform. Among younger-led firms, log value added falls by about 8 percent both after the reform and in the comparisons with 2010 and 2011 groups. With the 2004, 2012 and 2013 groups, the estimated declines are about 2 to 4 percent and are imprecise. Poisson estimates for sales, which retain firm-years with zero sales, do not consistently show the same declines. Among senior-led firms, they indicate a decline of about 20 percent after the reform. With 2010 to 2013 groups, the placebo estimates range from a decline of about 7 percent to an increase of about 19 percent, and none is significant at the 5 percent level. With 2004 groups, the Poisson estimate is a decline of about 17 percent, significant at the 5 percent level, but a joint test rejects that its pre-period coefficients are zero ($p = 0.011$), so under that specification the groups' sales were already changing differently before 2004.

Older-worker employment shows a less consistent pattern. In the reform window, the probability of employing at least one older worker rises by 8 percentage points more in high-exposure than in medium-exposure firms among younger-led firms, and by 12 percentage points more among senior-led firms. Most placebo estimates are smaller, but two are statistically significant at the 5 percent level: 5 percentage points among younger-led firms with 2013 groups and 6 percentage points among senior-led firms with 2012 groups. With 2004 groups, the estimate is 5 percentage points among younger-led firms, significant at the 10 percent level, and 1 percentage point among senior-led firms, so this comparison does not reproduce the senior-led increase. For the number of older workers, the largest estimate comes from a placebo comparison. With 2013 groups, the count rises by about 15 percent among younger-led firms, compared with about 8.5 percent after the reform, while it falls by about 8 percent among senior-led firms. Joint tests reject that the pre-period coefficients are zero at the 5 percent level for at least one outcome in seven of the ten placebo comparisons, most often with 2013 groups; with 2004 groups, the only rejection is for senior-led Poisson sales. For the reform window, no pre-period test rejects at the 5 percent level, although the two senior-led employment tests reject at the 10 percent level.

Output declines of similar size in comparisons without a tax cut, including one that precedes the reform, weaken the case for attributing the decline after 2006 to the reform. They do not show what produces these declines. The employment increases after the reform are larger than most, but not all, placebo estimates. These comparisons therefore also limit how confidently we can attribute the employment increases to the reform. As with the comparison around 2013, they do not tell us how much, if anything, to subtract from the original estimates.

6.5 Allowing for Different Trends Across Firms

For the 2007–08 comparison, replacing the common year effects with industry-by-year or municipality-by-year fixed effects leaves the older-worker employment estimates positive and statistically significant. The estimated declines in log sales and log value added remain close to the baseline estimates and statistically significant with either set of fixed effects (Table A.8). Fixing each firm's municipality at its 2006 value, rather than using its municipality in each year, gives nearly the same estimates. These controls allow for annual shocks shared by firms in the same industry or municipality. The results give little support to an explanation based on those shared shocks.

High-exposure firms were smaller before the reform. If the recession hurt small firms more than large firms within the same industry or municipality, these controls would not remove that difference.

In the full sample, the annual estimates are already negative in 2007 (Figure 10), which weakens an explanation based entirely on the 2008–09 recession, although the recession could have contributed to the later declines.

To examine differences by firm size directly, we also allow the yearly effects to differ across four groups of firms defined by their employment in 2006 (Table A.9). Both older-worker employment coefficients become larger. The coefficient for the probability of employing an older worker rises from 0.100 to 0.112, and the Poisson coefficient for the number of older workers rises from 0.083 to 0.123. The output declines become smaller but remain statistically significant. Log sales and log value added fall by about 10 and 13 percent, compared with about 14 and 15 percent in the baseline. Among senior-led firms, they fall by about 12 and 14 percent, compared with about 19 percent, whereas the younger-led estimates are nearly unchanged. With separate year effects for each size group within each manager group, the difference between senior-led and younger-led firms is −0.09 log points for sales (standard error 0.07) and −0.06 for value added (standard error 0.06), compared with −0.16 and −0.12 in the baseline. Neither difference is statistically significant at the 10 percent level.

7 Tax Savings and Employment

7.1 The Size of the Savings

Table 4 puts the tax cut in relation to firms' payroll costs. Holding 2006 workers and wages fixed, the predicted saving in 2008 is 5.4 percent of payroll costs in high-exposure firms and 2.3 percent in medium-exposure firms, or about 64,400 and 43,300 SEK per firm, respectively. These amounts are in 2006 kronor and describe savings on unchanged workers and wages, before any employment or wage response.

For an individual eligible worker, the reduction in payroll cost at an unchanged gross wage was 12.8 percent for workers born in 1938 or later and 19.5 percent for those born in 1937 or earlier (Section 1). Both exposure groups faced these reductions. The amounts help assess the economic importance of the tax cut, but their size alone does not establish whether it led firms to hire or retain workers or to continue operating for longer.

Most of the predicted tax saving in senior-led firms comes from lower employer contributions on the registered manager's own wages. The manager appears in the 2006 wage records of 877 of the 933 senior-led firms. Across all senior-led firms, the manager's wages account for 67 percent of the predicted saving over 2007–2008: 68 percent in high-exposure firms and 63 percent in medium-exposure firms. This shows which wages generated the saving, not who ultimately benefited from it.

These calculations use the tax rules and the firms' 2006 workers and wages. They do not require the assumption that employment would have followed parallel paths without the reform. That assumption becomes necessary when interpreting the employment difference as an effect of the tax cut.

7.2 Savings per Additional Employment-Year

To calculate predicted tax savings per additional older-worker employment-year, we sum the saving differences between high- and medium-exposure firms over 2007 and 2008 and divide by the sum of their employment differences. We use each firm's predicted saving in kronor, from the calculation behind Table 4, rather than that table's weighted mean percentages. A causal interpretation requires that employment in the two groups would have followed parallel paths without the tax cut; Section 6 examines this assumption. The calculation concerns employment at the sample firms. It does not establish the change in employment across the economy or the reform's overall benefits and costs.

We estimate the two differences with the same linear regression, without sales weights, on the same firm-years over 2003–2008, with 2006 as the reference year. For each firm, the savings measure is zero before 2007 and equals its predicted saving on its 2006 workers and wages in 2007 and 2008. We measure employment by the number of older workers. For each measure, we add the 2007 and 2008 coefficients. This gives the two-year differences in predicted savings and older-worker employment-years per firm. A counted employment-year means meeting the annual earnings threshold at a firm; it need not represent a full year of full-time work or involve a new hire. Firm-years with accounts but no wage records count as zero employment and keep their predicted saving, and we also report results that exclude these firm-years from both differences. Confidence intervals come from resampling whole firms. For the ratio, we use Fieller's method, which allows the confidence set to be unbounded when the employment difference is imprecise.

Table 8 reports the results. Over 2007–2008, the predicted saving in high-exposure firms exceeds that in medium-exposure firms by about 30,800 SEK per firm, with a 95 percent confidence interval from 24,800 to 37,000 SEK. The corresponding employment difference is 0.05 older-worker employment-years per firm, with a confidence interval from −0.07 to 0.16. The ratio of the two differences is about 620,000 SEK per additional employment-year. Because the employment difference cannot be distinguished from zero, the 95 percent confidence set for the ratio has no upper bound and also includes negative values. The same holds within younger-led and senior-led firms. Excluding firm-years without wage records doubles the employment difference to 0.10, but its confidence interval still includes zero.

Table 8: Predicted Tax Savings and Older-Worker Employment, 2007–2008.

	All firm-years			Firm-years with wage records	
	All firms	Younger-led	Senior-led	All firms	Younger-led
	(1)	(2)	(3)	(4)	(5)
Saving difference	30.8	24.3	31.8	32.0	25.0
	[24.8; 37.0]	[16.8; 31.5]	[22.3; 41.9]	[25.9; 38.1]	[17.3; 32.6]
Employment difference	0.049	0.063	0.032	0.102	0.095
	[−0.069; 0.162]	[−0.101; 0.226]	[−0.148; 0.204]	[−0.007; 0.222]	[−0.064; 0.258]
Ratio	622	386	985	313	262
95% set: values outside	(−466; 194)	(−238; 113)	(−205; 149)	(−2,587; 148)	(−347; 100)
Pre-period p-value	<0.001	0.049	<0.001	<0.001	0.062
Firms	2,220	1,268	933	2,218	1,267
Firm-years	12,895	7,359	5,437	12,712	7,262

Notes: Each column compares high- and medium-exposure firms in an unweighted linear regression over 2003–2008 with firm and year fixed effects and yearly high-exposure indicators, using 2006 as the reference year. The saving and employment outcomes are estimated on the same firm-years. The saving outcome is zero before 2007 and equals the firm's predicted saving on its 2006 workers and wages (Table 4) in 2007 and 2008; kronor are nominal 2006 amounts. The employment outcome is the number of older workers. Each difference sums the 2007 and 2008 coefficients. The saving difference is in thousands of SEK per firm, the employment difference in older-worker employment-years per firm, and the ratio in thousands of SEK per employment-year. Younger-led and senior-led firms are classified by the registered manager's age in 2006; column (1) also includes firms without a recorded manager age. Columns (1)–(3) count firm-years with accounts but no wage records as zero employment and keep their predicted saving; columns (4) and (5) exclude these firm-years. Brackets show the 2.5th and 97.5th percentiles over 999 bootstrap samples drawn by firm. The 95 percent confidence set for the ratio, from Fieller's method using the bootstrap covariance of the two differences, consists of all values outside the interval shown; it has no upper bound and includes negative values. The pre-period p-value tests jointly that the employment coefficients for 2003–2005 are zero.

In this specification, the employment difference was already changing before the reform. Relative to 2006, the coefficients for 2003, 2004 and 2005 are −0.18, −0.13 and −0.07, and a joint test rejects that they are zero ($p < 0.001$). After 2006 the difference changes little: the coefficients are 0.02 for 2007 and 0.03 for 2008. The Poisson estimates for the number of older workers in Figure 7 use a different model, weighting and period. They measure proportional differences, weight firms by the logarithm of

2006 sales and cover 2003–2012. The joint test does not reject that their pre-reform coefficients are zero ($p = 0.83$). As noted in Section 6.3, however, this does not establish that the groups would have followed similar paths without the reform. The calculation here requires the number of older workers to have changed by the same amount in both groups without the reform, and its pre-reform estimates do not support that assumption. Together with the uncertainty about the employment difference, this prevents a reliable causal estimate of predicted tax savings per additional older-worker employment-year.

Even if those requirements were met, the calculation would concern employment at the sample firms and use predicted rather than actual savings. Nor do we subtract from the predicted savings any revenue recovered through other taxes. Interpreting the ratio as the cost of additional employment would also require each krona of predicted saving to produce the same amount of additional employment in both groups.

8 Conclusion

We study Sweden's 2007–08 payroll tax cut by comparing firms with high and medium older-worker payroll shares in 2006. Relative to medium-exposure firms, we estimate a 10-percentage-point increase over 2007–2012 in the probability that high-exposure firms employ an older worker. The estimated relative increase in their number of older workers is about 9 percent, but is less precise. Sales and value added fall by about 14 and 15 percent relative to medium-exposure firms. Accounting for firm size reduces these declines and increases the employment estimates. Older-worker employment thus holds up better in high-exposure firms, while their sales and value added perform worse. The probability of retaining an older worker increases more than that of hiring one, but retention was already rising faster in high-exposure firms before the reform.

The role of older managers is central to interpreting these findings. In senior-led firms, about two-thirds of the predicted 2007–08 tax saving comes from lower employer contributions on the manager's own wages. Excluding the manager leaves little evidence of an increase in the probability of employing other older workers. The relative sales decline is larger in senior-led firms, but the difference between manager groups is smaller and no longer statistically significant once firm size is taken into account. Value added also falls in younger-led firms.

Comparisons in other periods make it harder to attribute these patterns to the tax cut. When we form groups in 2004 and 2010–2013, sales and value added in senior-led firms again show similar relative declines over equally long periods. No new payroll tax cut for older workers took effect in the two years following any of those years. Some employment increases also recur, although the 2004 comparison does not reproduce the senior-led increase. In several comparisons, the groups were already following different paths before the year used to form them. An alternative estimation method, which includes years with zero sales, also shows a decline after the reform among senior-led firms, but no consistent decline in the other periods. These comparisons weaken the case for attributing the output decline to the reform and limit confidence in attributing the employment increases to it. They do not tell us how much of the employment and output differences after the reform reflects the way we group firms.

These findings suggest three lessons for the design and evaluation of payroll tax cuts for older workers. First, payroll tax reforms need to be studied from both the individual and the firm perspective. Whether older workers remain employed depends on their decisions about work and retirement and on employers' decisions about hiring and retaining them. Evaluations should therefore examine both whether firms hire older workers and whether those already employed stay in their jobs. Second, because eligible workers often run small firms, a tax cut could allow a business to continue at a smaller scale, with consequences for its other employees. Our comparison of firms' disappearance from the accounts data provides no clear support for this explanation. It does not rule it out either, since disappearing from these data does not necessarily mean that a firm has closed. Third, the benefits of the tax cut

need to be assessed against its cost. A full assessment would include changes in firms' activity, other tax revenues and benefit payments. Our calculation addresses the narrower question of how much predicted tax saving corresponds to an additional year of older-worker employment. The difference in predicted savings is precisely estimated, but the employment difference in this calculation is imprecise and was already changing before the reform. We therefore cannot reliably estimate the saving per additional year of employment.

Our findings on older managers motivate further research on taxes and business activity near retirement. Following individuals could distinguish people who start a business, continue one they already run, or leave self-employment, and trace moves between wage employment, self-employment and retirement. Linking these records to information on ownership and legal form would help establish whether business activity continues when a firm changes form or disappears from the accounts. Sweden's later reintroduction and repeal of the special payroll tax offer opportunities to examine whether tax increases and cuts affect these decisions differently, while accounting for other policy changes and economic conditions. Such work could clarify whether payroll tax cuts extend working lives through employment in other firms or through continued work in a person's own business.

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Appendix

A Additional Results for the 2007–08 Reform

A.1 Sample Characteristics

Table A.1 describes firms across the four exposure quartiles before the lower two quartiles are excluded from the main comparison.

Table A.1: Summary Statistics by Quartiles, 2006.

	(1) Top 1/4	(2) Upper-mid 1/4	(3) Lower-mid 1/4	(4) Bottom 1/4
Outcome variables	Mean	Mean	Mean	Mean
Number of older workers	1.44	1.18	0.82	0.16
Total employment	4.35	6.45	8.68	9.67
Sales (ln)	8.47	8.79	9.00	9.16
Sales	7916.77	10358.24	13502.39	18848.49
Value added (ln)	7.57	7.90	8.15	8.32
Value added / 2006 sales	0.46	0.46	–	–
Total assets (ln)	8.07	8.29	8.46	8.59
Profit	611.51	674.59	686.63	567.98
Industry share				
Agriculture	0.04	0.04	0.03	0.03
Manufacturing	0.17	0.18	0.21	0.19
Construction	0.11	0.10	0.10	0.06
Trade	0.15	0.15	0.11	0.12
Transport	0.11	0.15	0.17	0.14
Real estate	0.05	0.02	0.02	0.02
Business services	0.17	0.14	0.13	0.16
Other services	0.22	0.22	0.23	0.29
Firms	1,110	1,111	1,111	1,111

Notes: Summary statistics for the reference year 2006. Each firm receives equal weight. The columns group firms by quartiles of the 2006 exposure index defined in Section 3.2. A worker counts as employed if annual earnings from the firm reach at least one price base amount. A firm enters the source sample if it paid any positive wages to a worker aged 66 or above. Mean measured older-worker employment can therefore be below one. Sales, value added, and total assets are expressed in thousands of SEK before taking logarithms. Profit is also measured in thousands of SEK and can be negative. Value added relative to 2006 sales is winsorized using the two highest quartiles pooled together and is not reported for the others. Columns (1)–(4) report means within the 4,443-firm restricted sample before the lower two quartiles are excluded from the regressions.

Table A.2: Cumulative Disappearance from the Panel by Exposure Group, 2006 and 2012 Cohorts.

	Firms	By 2007	By 2008	By 2009
Panel A: 2006 cohort, all firms				
High exposure	1,104	1.8	4.3	8.5
Medium exposure	1,107	1.6	4.2	7.2
Difference	2,211	0.2	0.2	1.3
		(0.6)	(0.9)	(1.1)
Panel B: 2006 cohort, senior-led firms				
High exposure	618	2.1	5.7	9.5
Medium exposure	310	3.2	7.1	10.0
Difference	928	−1.1	−1.4	−0.5
		(1.2)	(1.7)	(2.1)
Panel C: 2006 cohort, younger-led firms				
High exposure	476	1.3	2.5	7.1
Medium exposure	788	0.8	2.5	5.5
Difference	1,264	0.5	0.0	1.7
		(0.6)	(0.9)	(1.4)
	Firms	By 2013	By 2014	By 2015
Panel D: 2012 cohort, all firms				
High exposure	1,815	1.9	4.7	6.8
Medium exposure	1,815	1.3	3.2	5.7
Difference	3,630	0.7	1.5**	1.2
		(0.4)	(0.6)	(0.8)
Panel E: 2012 cohort, senior-led firms				
High exposure	1,083	1.5	4.2	6.3
Medium exposure	618	1.0	2.8	5.7
Difference	1,701	0.5	1.5*	0.6
		(0.5)	(0.9)	(1.2)
Panel F: 2012 cohort, younger-led firms				
High exposure	683	2.5	5.0	6.9
Medium exposure	1,181	1.2	3.1	5.4
Difference	1,864	1.3*	1.8*	1.5
		(0.7)	(1.0)	(1.2)

Notes: Percentage of firms that have disappeared from the panel by each year, with each firm receiving equal weight. A firm disappears in the year after its last appearance in the accounts data; later appearances are checked through 2012 for the 2006 cohort and through 2018 for the 2012 cohort. Firms that disappear and return within these periods are excluded (10 firms in the 2006 cohort and 7 in the 2012 cohort). Firms without manager information enter only Panels A and D. "Difference" is the coefficient on a high-exposure indicator in a linear probability model with one observation per firm, in percentage points, with robust standard errors in parentheses. Differences can deviate slightly from the gap between the rounded group shares. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Manager Groups

Section 2.2 presents the descriptive figures on manager ages and older-worker counts. Table A.3 reports the triple-difference regression results for sales and value added. Table A.4 reports the probability that a senior-led firm employs an older worker other than its manager. The disappearance comparison by manager age appears in Section 5.3.

Table A.3: Triple-Difference Estimates for Sales and Value Added, by Manager Type.

	(1) Sales (ln)	(2) Sales (Poisson)	(3) Value added (ln)	(4) Value added / 2006 sales
High × Senior × Post	-0.16**	-0.25**	-0.12*	-0.03
	(0.07)	(0.12)	(0.06)	(0.02)
High × Post	-0.05	-0.02	-0.10**	-0.04***
	(0.05)	(0.08)	(0.04)	(0.02)
Senior × Post	-0.00	0.02	0.01	-0.02
	(0.05)	(0.08)	(0.04)	(0.02)
Observations	20,227	20,603	19,831	20,607
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by firm are in parentheses. High denotes high rather than medium exposure; Senior denotes a registered manager aged 66 or above in 2006; Post denotes 2007–2012. The manager classification remains fixed, and firms with missing manager age are excluded. The triple interaction compares the high-minus-medium change across manager groups. All models include firm and year fixed effects and are weighted by the logarithm of firms' 2006 sales. Columns 1 and 3 use OLS on positive outcomes. Column 2 uses Poisson, retaining zero sales but excluding negative values. Column 4 uses OLS for value added divided by 2006 sales, retaining zero and negative value added and winsorizing the ratio at the 1st and 99th percentiles within year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Employment of Non-Manager Older Workers in Senior-Led Firms.

	Employment probability
High exposure × Post	0.010
	(0.031)
Observations	8,637
Firm FE	Yes
Year FE	Yes

Notes: Standard errors clustered by firm are in parentheses; observations are firm-years. The sample contains 933 firms classified as senior-led in 2006, observed over 2003–2012. The person registered as the firm's manager in 2006 is excluded from its employment measure in every year. A linear probability model estimates the change in the probability that a firm employs at least one other worker aged 66 or above. Workers must meet the same annual earnings threshold as in the baseline. Firm-years without such a worker remain in the sample with the outcome equal to zero. Weights are the logarithm of CPI-adjusted 2006 sales, measured in thousands of SEK. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.3 Placebo Comparisons

Table A.5 reports the employment and firm-performance comparison discussed in Section 6.3.

Table A.5: Outcomes Around 2013, with Exposure Measured in 2012.

Panel A: Firm performance, by manager type

	Senior-led		Younger-led	
Outcome	Estimate (SE)	Pre-period p-value	Estimate (SE)	Pre-period p-value
Sales (ln)	-0.154*** (0.038) N = 9,922	0.365	-0.039 (0.033) N = 10,653	0.089
Sales (Poisson, zeros included)	0.087 (0.080) N = 9,980	0.460	-0.038 (0.045) N = 10,696	0.867
Sales (Poisson, positive only)	0.087 (0.080) N = 9,922	0.451	-0.035 (0.044) N = 10,653	0.814
Zero sales (LPM)	0.003 (0.004) N = 9,986	0.126	0.002 (0.003) N = 10,699	0.884
Value added (ln)	-0.107*** (0.038) N = 9,751	0.073	-0.042 (0.031) N = 10,527	0.385
Value added / 2012 sales	-0.058*** (0.014) N = 9,986	0.007	-0.021 (0.014) N = 10,699	0.041

Panel B: Older-worker employment, all firms

Outcome	Estimate (SE)	Pre-period p-value
Any older worker (LPM)	0.039*** (0.014) N = 21,036	0.002
Number of older workers (Poisson)	0.027 (0.031) N = 21,024	0.945

Notes: Each estimate compares changes in high-exposure firms with changes in medium-exposure firms during 2013–2014 relative to 2009–2012. No new payroll tax cut occurred in 2013. Exposure groups are the highest and next-highest quartiles of the older-worker payroll share in 2012. Senior-led firms have a manager aged 66 or above in 2012; younger-led firms have a manager below 66. Both classifications remain fixed. Panel B includes all high- and medium-exposure firms, including those without a recorded manager age; the probability of employing any older worker uses a linear probability model, the number of older workers uses a Poisson model, and firm-years without wage records count as zero employment. All models include firm and year fixed effects, are weighted by the logarithm of CPI-adjusted 2012 sales, measured in thousands of SEK, and cluster standard errors by firm. Log outcomes use OLS on positive values. Sales Poisson either retains zeros or uses positive values only; both exclude negative sales. Zero sales uses a linear probability model. Value added divided by 2012 sales uses OLS and is winsorized at the 1st and 99th percentiles within year, retaining zero and negative value added. The pre-period p-values test the 2009–2011 coefficients jointly, with 2012 as reference. N denotes the total number of firm-year observations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tables A.6 and A.7 report the placebo comparisons by grouping year discussed in Section 6.4.

Table A.6: Placebo Comparisons by Grouping Year, Younger-Led Firms.

	Grouping year					
Outcome	2004	2006	2010	2011	2012	2013
Any older worker (LPM)	0.053*	0.083***	0.003	0.015	0.034*	0.049**
	(0.028)	(0.027)	(0.022)	(0.022)	(0.020)	(0.019)
Pre-period p-value	0.780	0.474	0.501	0.065	0.275	0.027
Observations	6,905	7,359	9,160	9,990	10,699	10,836
Number of older workers (Poisson)	0.012	0.082*	0.007	0.034	0.050	0.142***
	(0.047)	(0.048)	(0.096)	(0.060)	(0.056)	(0.039)
Pre-period p-value	0.636	0.461	0.917	0.744	0.891	0.047
Observations	6,806	7,306	9,154	9,990	10,693	10,830
Sales (ln)	−0.019	−0.042	−0.083**	−0.068*	−0.039	−0.050
	(0.042)	(0.036)	(0.040)	(0.037)	(0.033)	(0.030)
Pre-period p-value	0.239	0.831	0.275	0.337	0.089	0.021
Observations	6,870	7,309	9,100	9,938	10,653	10,776
Sales (Poisson, zeros included)	−0.129*	0.020	−0.023	−0.075	−0.038	−0.054
	(0.077)	(0.050)	(0.070)	(0.047)	(0.045)	(0.057)
Pre-period p-value	0.606	0.767	0.951	0.808	0.867	0.775
Observations	6,905	7,358	9,159	9,989	10,696	10,832
Zero sales (LPM)	0.007	−0.002	0.005	0.004	0.002	−0.001
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.004)
Pre-period p-value	0.132	0.111	0.409	0.682	0.884	0.197
Observations	6,905	7,359	9,160	9,990	10,699	10,836
Value added (ln)	−0.017	−0.080**	−0.081**	−0.085**	−0.042	−0.042
	(0.036)	(0.035)	(0.035)	(0.034)	(0.031)	(0.030)
Pre-period p-value	0.313	0.558	0.414	0.749	0.385	0.023
Observations	6,781	7,238	8,989	9,845	10,527	10,618
Value added / sales in grouping year	−0.012	−0.030**	−0.037***	−0.031**	−0.021	−0.011
	(0.015)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)
Pre-period p-value	0.285	0.825	0.516	0.100	0.041	0.116
Observations	6,905	7,359	9,160	9,990	10,699	10,836

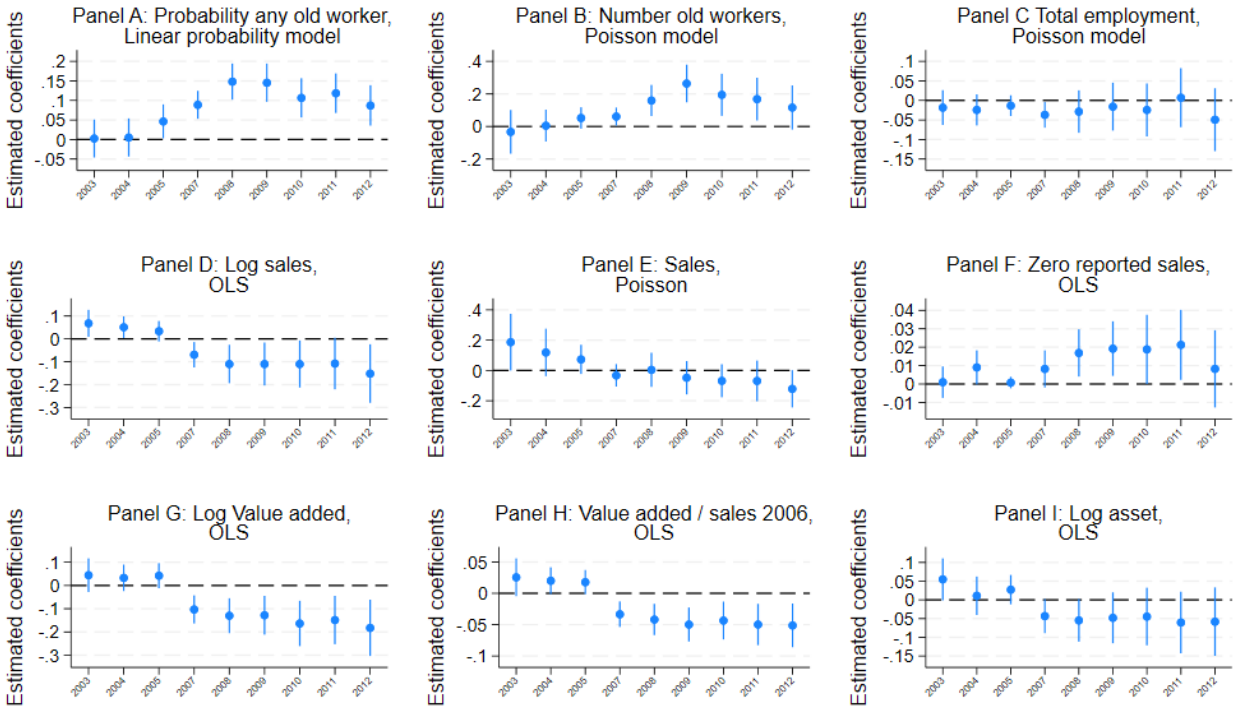
Notes: Each column compares high- and medium-exposure firms among firms whose registered manager was below age 66 in the grouping year. Firms are selected, assigned to exposure groups and classified by manager type in the grouping year, as in the main analysis, and these classifications remain fixed. Each estimate compares the two years after the grouping year with the four years up to and including it. For the 2004 groups, the six-year period is 2001–2006. For the 2006 groups, this compares 2007–2008 with 2003–2006, re-estimating the reform over a shorter period than in the main analysis. For the 2010, 2011, 2012 and 2013 groups, the six-year periods are 2007–2012, 2008–2013, 2009–2014 and 2010–2015, respectively. No new payroll tax cut for older workers took effect in the two years following the grouping years 2004 and 2010–2013; the 2007 cut was announced in October 2006, and employer contributions for young workers changed in 2015, within the window for the 2013 groups. The 2004 groups are the earliest that can be split by manager type, because the manager register begins in 2004. The 2012 column corresponds to the comparison around 2013 in Table A.5. All models include firm and year fixed effects, are weighted by the logarithm of CPI-adjusted sales in the grouping year, and cluster standard errors by firm. Log outcomes use OLS on positive values. The Poisson sales model retains zero sales and excludes negative sales. Value added divided by sales in the grouping year is winsorized at the 1st and 99th percentiles within year. The pre-period p-value tests jointly that the coefficients for the three years before the grouping year are zero. Observations are firm-years. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Placebo Comparisons by Grouping Year, Senior-Led Firms.

	Grouping year					
Outcome	2004	2006	2010	2011	2012	2013
Any older worker (LPM)	0.007	0.119***	0.023	0.003	0.060***	−0.008
	(0.032)	(0.033)	(0.023)	(0.022)	(0.021)	(0.019)
Pre-period p-value	0.209	0.090	0.101	0.020	0.119	0.001
Observations	4,579	5,437	8,465	9,225	9,986	10,844
Number of older workers (Poisson)	−0.026	0.089	−0.042	−0.041	0.051	−0.086***
	(0.058)	(0.056)	(0.038)	(0.037)	(0.038)	(0.031)
Pre-period p-value	0.108	0.073	0.035	0.917	0.161	0.481
Observations	4,568	5,425	8,465	9,225	9,986	10,844
Sales (ln)	−0.141***	−0.171***	−0.119***	−0.131***	−0.154***	−0.081**
	(0.049)	(0.047)	(0.042)	(0.041)	(0.038)	(0.033)
Pre-period p-value	0.272	0.673	0.515	0.095	0.365	0.045
Observations	4,553	5,390	8,401	9,162	9,922	10,762
Sales (Poisson, zeros included)	−0.184**	−0.224***	−0.075*	0.010	0.087	0.176*
	(0.079)	(0.074)	(0.043)	(0.052)	(0.080)	(0.103)
Pre-period p-value	0.011	0.615	0.773	0.322	0.460	0.524
Observations	4,578	5,436	8,461	9,221	9,980	10,841
Zero sales (LPM)	0.002	0.010	−0.002	0.000	0.003	0.016***
	(0.007)	(0.008)	(0.006)	(0.005)	(0.004)	(0.004)
Pre-period p-value	0.385	0.735	0.839	0.343	0.126	0.334
Observations	4,579	5,437	8,465	9,225	9,986	10,844
Value added (ln)	−0.083*	−0.129***	−0.136***	−0.106***	−0.107***	−0.104***
	(0.047)	(0.040)	(0.036)	(0.035)	(0.038)	(0.031)
Pre-period p-value	0.294	0.873	0.785	0.298	0.073	0.001
Observations	4,492	5,328	8,303	9,018	9,751	10,596
Value added / sales in grouping year	−0.052***	−0.056***	−0.056***	−0.067***	−0.058***	−0.053***
	(0.019)	(0.016)	(0.015)	(0.013)	(0.014)	(0.014)
Pre-period p-value	0.154	0.759	0.411	0.262	0.007	0.001
Observations	4,579	5,437	8,465	9,225	9,986	10,844

Notes: Each column compares high- and medium-exposure firms among firms whose registered manager was aged 66 or above in the grouping year. Firms are selected, assigned to exposure groups and classified by manager type in the grouping year, as in the main analysis, and these classifications remain fixed. Each estimate compares the two years after the grouping year with the four years up to and including it. For the 2004 groups, the six-year period is 2001–2006. For the 2006 groups, this compares 2007–2008 with 2003–2006, re-estimating the reform over a shorter period than in the main analysis. For the 2010, 2011, 2012 and 2013 groups, the six-year periods are 2007–2012, 2008–2013, 2009–2014 and 2010–2015, respectively. No new payroll tax cut for older workers took effect in the two years following the grouping years 2004 and 2010–2013; the 2007 cut was announced in October 2006, and employer contributions for young workers changed in 2015, within the window for the 2013 groups. The 2004 groups are the earliest that can be split by manager type, because the manager register begins in 2004. The 2012 column corresponds to the comparison around 2013 in Table A.5. All models include firm and year fixed effects, are weighted by the logarithm of CPI-adjusted sales in the grouping year, and cluster standard errors by firm. Log outcomes use OLS on positive values. The Poisson sales model retains zero sales and excludes negative sales. Value added divided by sales in the grouping year is winsorized at the 1st and 99th percentiles within year. The pre-period p-value tests jointly that the coefficients for the three years before the grouping year are zero. Observations are firm-years. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Effects of the 2007–08 Reform on Employment and Firm Performance, Restricting to Firms with Zero Payroll for Workers Aged 19–25.

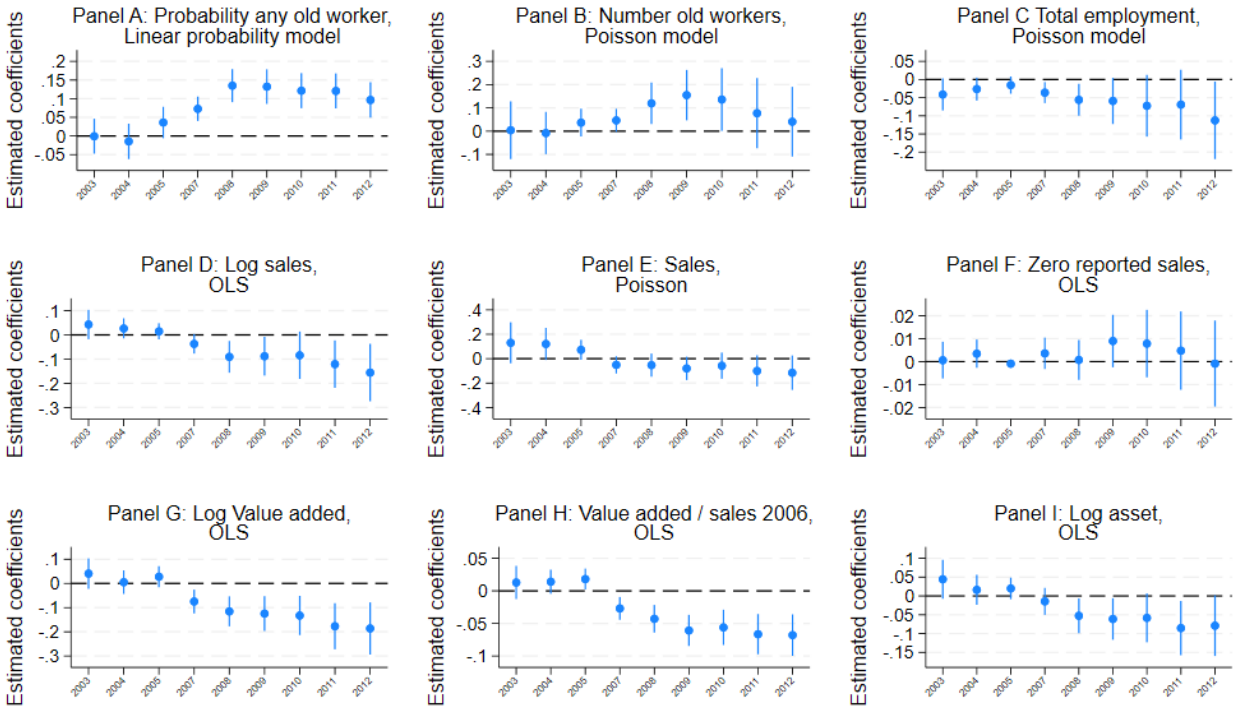


Notes: The sample is restricted to firms with no payroll for workers aged 19–25 in 2006. Firms are ranked by their older-worker payroll share within this sample, giving 883 high-exposure and 883 medium-exposure firms. Regressions are weighted by the logarithm of CPI-adjusted 2006 sales, measured in thousands of SEK. Each panel reports yearly coefficients comparing high- with medium-exposure firms, relative to 2006, with 95% confidence intervals. The outcomes are the probability of employing any older worker (Panel A), the number of older workers (B), total employment (C), log sales (D), sales (E), zero sales (F), log value added (G), value added relative to 2006 sales (H), and log total assets (I). Panels B, C, and E report Poisson coefficients rather than changes in outcome levels. Standard errors are clustered at the firm level. Joint pre-reform test p-values (Panels A–I): 0.106, 0.203, 0.666, 0.130, 0.229, 0.173, 0.451, 0.229, 0.094. The numbers of observations in Panels A–I are 16,454, 16,385, 16,454, 16,138, 16,449, 16,454, 15,788, 16,454 and 16,450.

A.4 Alternative Samples and Fixed Effects

These comparisons show how the estimates change when we exclude firms with payroll for workers aged 19–25 or restrict the sample to firms observed in every year. We also allow annual changes to differ across industries, municipalities or groups of firms of different size in 2006.

Figure A.2: Effects of the 2007–08 Reform on Employment and Firm Performance, Firms Observed in Every Year, 2003–2012.



Notes: The sample contains 1,752 firms that report sales, value added, and total assets in every year from 2003 to 2012. Of these, 863 are high-exposure firms and 889 are medium-exposure firms. Individual regressions can exclude further observations, including nonpositive values in the log specifications. Each panel reports yearly coefficients comparing high- with medium-exposure firms, relative to 2006, with 95% confidence intervals. The outcomes are the probability of employing any older worker (Panel A), the number of older workers (B), total employment (C), log sales (D), sales (E), zero sales (F), log value added (G), value added relative to 2006 sales (H), and log total assets (I). Panels B, C, and E report Poisson coefficients rather than changes in outcome levels. Standard errors are clustered at the firm level. Joint pre-reform test p-values (Panels A–I): 0.061, 0.407, 0.296, 0.536, 0.292, 0.413, 0.299, 0.173, 0.237. The numbers of observations in Panels A–I are 17,520; 17,460; 17,520; 17,261; 17,518; 17,520; 16,957; 17,520 and 17,519.

Table A.8: Robustness of the 2007–08 Reform Results to Alternative Fixed Effects Specifications.

Outcome	(1)	(2)	(3)	(4)
			Municipality-by-year effects	
	Year effects	Industry-by-year effects	Location each year	Location in 2006
Employment outcomes				
Any older worker	0.100***	0.100***	0.092***	0.092***
	(0.021)	(0.021)	(0.022)	(0.022)
	N = 20,754	N = 20,754	N = 20,361	N = 20,362
Number of older workers	0.083*	0.106**	0.109**	0.112**
	(0.047)	(0.045)	(0.045)	(0.045)
	N = 20,667	N = 20,667	N = 19,919	N = 19,930
Total employment	-0.056*	-0.048	-0.036	-0.032
	(0.033)	(0.030)	(0.026)	(0.026)
	N = 20,754	N = 20,754	N = 20,349	N = 20,356
Firm outcomes				
Sales (ln)	-0.146***	-0.148***	-0.151***	-0.145***
	(0.036)	(0.036)	(0.038)	(0.039)
	N = 20,358	N = 20,358	N = 19,955	N = 19,955
Sales (Poisson)	-0.175**	-0.163***	-0.146***	-0.145***
	(0.070)	(0.062)	(0.054)	(0.054)
	N = 20,749	N = 20,749	N = 20,354	N = 20,357
Zero sales	0.008	0.008	0.004	0.005
	(0.005)	(0.006)	(0.006)	(0.006)
	N = 20,754	N = 20,754	N = 20,361	N = 20,362
Value added (ln)	-0.164***	-0.168***	-0.174***	-0.169***
	(0.031)	(0.031)	(0.034)	(0.034)
	N = 19,954	N = 19,954	N = 19,551	N = 19,556
Value added / 2006 sales	-0.065***	-0.066***	-0.069***	-0.067***
	(0.011)	(0.011)	(0.013)	(0.013)
	N = 20,754	N = 20,754	N = 20,361	N = 20,362
Total assets (ln)	-0.083***	-0.086***	-0.084***	-0.085***
	(0.027)	(0.027)	(0.029)	(0.030)
	N = 20,750	N = 20,750	N = 20,356	N = 20,357
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Industry-year FE	No	Yes	No	No
Municipality-year FE	No	No	Yes	Yes

Notes: Standard errors clustered by firm are in parentheses; N is the number of firm-years. Column 1 uses the baseline specification reported in Tables 3 and 6; column 2 replaces the year effects with industry-by-year effects, and columns 3 and 4 replace them with municipality-by-year effects. Industry-by-year effects use eight broad industry groups based on firms' 2006 industry codes. Municipality-by-year effects use each year's municipality in column 3 and each firm's 2006 municipality in column 4. Outcomes and estimators are defined as in Tables 3 and 6; all models include firm fixed effects and are weighted by the logarithm of firms' 2006 sales. Estimation samples vary by outcome and specification. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Allowing Yearly Effects to Differ by Firm Size in 2006.

Outcome	(1) Year effects	(2) Size-by-year effects
All firms		
Any older worker	0.100*** (0.021) N = 20,754	0.112*** (0.022) N = 20,754
Number of older workers	0.083* (0.047) N = 20,667	0.123*** (0.046) N = 20,667
Sales (ln)	-0.146*** (0.036) N = 20,358	-0.111*** (0.035) N = 20,358
Value added (ln)	-0.164*** (0.031) N = 19,954	-0.141*** (0.031) N = 19,954
Senior-led firms		
Sales (ln)	-0.213*** (0.055) N = 8,428	-0.131** (0.057) N = 8,428
Value added (ln)	-0.215*** (0.046) N = 8,242	-0.154*** (0.047) N = 8,242
Younger-led firms		
Sales (ln)	-0.051 (0.047) N = 11,799	-0.042 (0.047) N = 11,799
Value added (ln)	-0.096** (0.042) N = 11,589	-0.094** (0.042) N = 11,589
Senior-led minus younger-led		
Sales (ln)	-0.16** (0.07) N = 20,227	-0.09 (0.07) N = 20,227
Value added (ln)	-0.12* (0.06) N = 19,831	-0.06 (0.06) N = 19,831

Notes: Standard errors clustered by firm are in parentheses; N is the number of firm-years. Column 1 repeats the baseline estimates in Tables 3, 6 and 7. Column 2 replaces the common year effects with separate year effects for four groups of firms defined by total employment in 2006: 3, 4–5, 6, and 7 or more workers, with cut-offs at the quartiles of the sample. Employment is counted as in the baseline. We estimate the probability of employing any older worker with a linear probability model and the number of older workers with a Poisson model. For sales and value added, we use OLS on the logarithms of positive values. Senior-led firms have a registered manager aged 66 or above in 2006 and younger-led firms a manager below 66; firms without a recorded manager age enter only the all-firm panel. The last panel reports the triple-difference estimate from a pooled regression of both manager groups; in column 2, the year effects for each size group differ between the manager groups, so this estimate equals the gap between the separate senior-led and younger-led estimates. All models include firm fixed effects and are weighted by the logarithm of firms' 2006 sales. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Firms and the Share Employing a Counted Older Worker, by Year and Exposure Group.

	High-exposure firms			Medium-exposure firms		
Year	Firms	Weighted share	Unweighted share	Firms	Weighted share	Unweighted share
Panel A: Main sample, groups formed in 2006						
2003	1,031	0.425	0.422	1,032	0.417	0.416
2004	1,065	0.545	0.541	1,062	0.538	0.536
2005	1,092	0.736	0.734	1,093	0.695	0.690
2006	1,109	0.999	0.999	1,111	0.978	0.977
2007	1,089	0.879	0.874	1,093	0.784	0.776
2008	1,056	0.717	0.715	1,062	0.575	0.569
2009	1,014	0.634	0.630	1,028	0.491	0.486
2010	984	0.584	0.579	1,003	0.466	0.458
2011	956	0.570	0.565	986	0.442	0.435
2012	928	0.524	0.516	960	0.417	0.411
Panel B: Comparison around 2013, groups formed in 2012						
2009	1,676	0.474	0.471	1,657	0.407	0.401
2010	1,725	0.598	0.595	1,712	0.535	0.529
2011	1,784	0.778	0.774	1,787	0.732	0.726
2012	1,815	1.000	1.000	1,818	0.994	0.994
2013	1,781	0.892	0.887	1,794	0.832	0.824
2014	1,730	0.817	0.811	1,757	0.715	0.706

Notes: The share is the proportion of firms employing at least one older worker counted as employed, that is, a worker aged 66 or above with earnings from the firm of at least one price base amount; firm-years without wage records count as having no older worker. Each panel uses the firm-years in the corresponding employment-probability regression: Table 3 for Panel A and Table A.5, Panel B, for Panel B. Weighted shares use the logarithm of CPI-adjusted sales in the grouping year, as in the regressions.

B Later Changes in the Tax Treatment of Older Workers

This appendix describes the subsequent changes in the tax treatment of older workers and explains why their effects need not mirror those of the 2007–08 abolition. Figure 1 shows that the 2007–08 abolition was followed by a partial reversal. On January 1, 2016, Sweden introduced a special payroll tax of 6.15 percent on pay to workers who had turned 65 before the start of the calendar year, that is, workers turning 66 or older during the year. The 10.21 percent pension contribution continued to apply to those born in 1938 or later. Employers therefore paid combined charges of 16.36 percent for these workers, while earlier cohorts faced only the special tax. The 6.15 percent tax was abolished for payments made after June 30, 2019. Annual outcomes for 2019 therefore combine six months with the tax and six months without it.[28]

The National Audit Office has evaluated the 2016 increase with a difference-in-differences design, comparing people who turned 65 in October to December of the preceding year with people who turned 65 in January to March of the study year, among those with earnings two years earlier (Riksrevisionen, 2020). It finds little effect on wage employment and some evidence of a decline in the probability of being self-employed, which the report cautions is difficult to interpret, and it notes that the effect of introducing the tax need not mirror the effect of abolishing it.

The later increase and repeal provide a different comparison from the original abolition. The tax change was smaller, and it applied after firms had operated under the exemption for several years. Introducing the tax raises the cost of employing an eligible worker, and removing it lowers that cost. Tax increases and tax cuts need not change employment or output by equal amounts. Employers may have adjusted their workforce in the intervening years, and an employment relationship that ends after a tax increase need not be restored when the tax is removed.

The timing also matters for a comparison based on firms' payroll shares. A proposal to restore the special tax had already appeared in the budget for 2015, although Parliament rejected it.[29] Firms' 2015 payrolls could therefore reflect anticipated taxation as well as their earlier response to the exemption. Comparisons across the two episodes must also account for differences in firm size, workforce composition, and the ages of managers.

Other tax changes accompanied these episodes. The earlier reduction in employer contributions for young workers ended on June 1, 2016. In 2016, the higher basic allowance for people over 65 also increased, and a high-income phaseout of both the ordinary and older-worker earned income tax credits was introduced. In 2019, the ordinary credit increased, as did the higher basic allowance for older people.[30] These changes affected different parts of the workforce and different earnings levels. Year fixed effects account for changes shared by all firms, but not tax changes that affect firms differently because of the workers they employ. The later reforms therefore also require distinguishing changes in employers' costs from changes in what workers gain financially by continuing to work.

A longer period after the repeal includes the pandemic and changes in retirement-related rules. The employment-protection age rose from 67 to 68 in January 2020 and to 69 in January 2023. The older-worker earned income tax credit increased in 2023. In 2023, workers qualified for reduced employer contributions from the year they turned 67, rather than 66.[31] These changes make 2023 a distinct policy year, rather than simply another year after the repeal.

[28] See Prop. 2018/19:82, Sections 4–6, the parliamentary decision, 2018/19:SkU18, and the Swedish Tax Agency's 2019 contribution schedule.

[29] See 2015/16:SkU1y, the discussion of the special payroll tax on older workers.

[30] See 2015/16:SfU12 for the youth contribution rates, 2015/16:SkU1y for the EITC phaseout, Prop. 2015/16:1 for the basic allowance, and Government Communication 2023/24:42, Appendix Table 1, for the income-tax chronology. The higher basic allowance applies to pension income as well as earnings.

[31] See Prop. 2018/19:91 for employment protection, Government Communication 2023/24:42 for the credit, and the Swedish Tax Agency's 2023 schedule for employer contributions. The reduced rate applied from the year workers turned 67 in 2024 and 2025 as well.

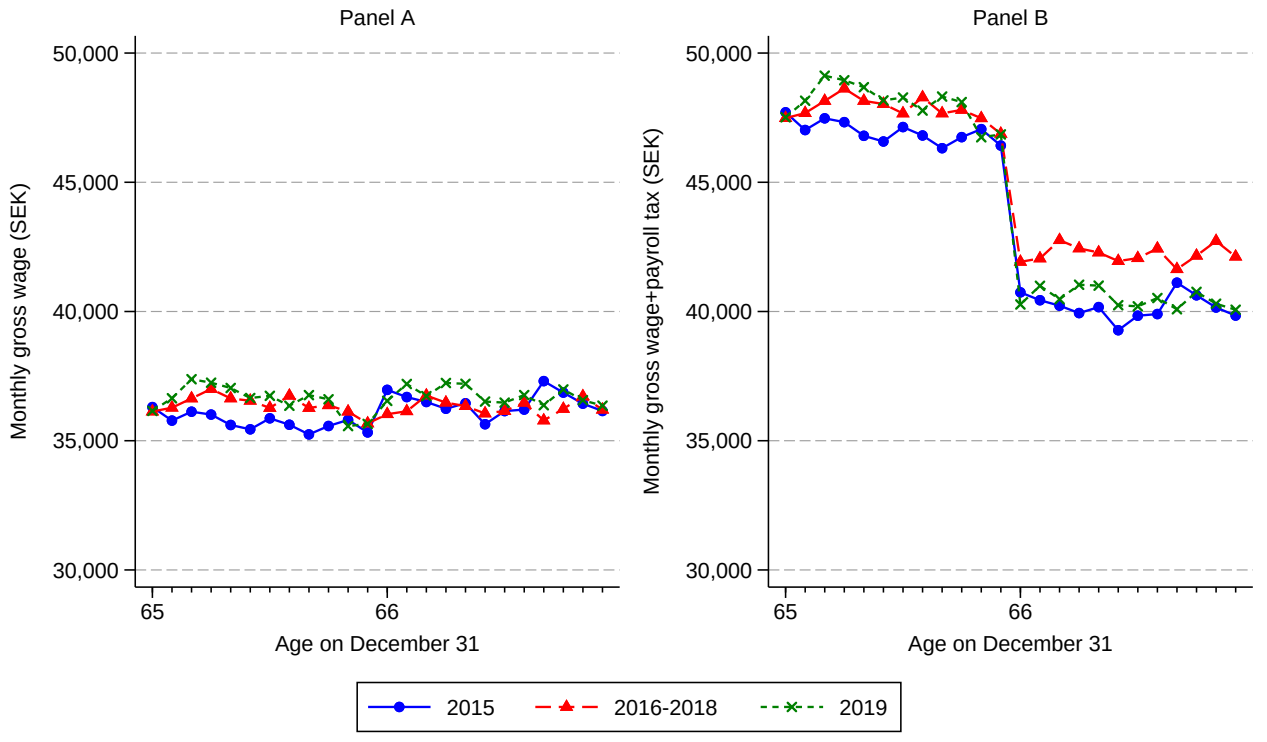
Evidence from the later reforms can help assess the explanations for the 2007–08 findings, but the comparison must account for differences in the policies, firms, and workers. Similar output declines after both a tax cut and a tax increase would not, by themselves, show that the declines had the same cause or rule out the explanations discussed in Section 5.3.

C Gross Wages and Payroll Taxes around Age 66

Figure C.1 compares average monthly wages around age 66 in 2015, 2016–2018 and 2019. Panel A shows gross monthly wages, while Panel B adds the age-specific employer payroll tax. Wages are measured on a full-time-equivalent basis and expressed in 2019 prices. During these years, employers paid lower payroll taxes from the year a worker turned 66; Appendix B describes the changes in the tax rate across periods.

Gross wages vary around age 66, but do not show an increase large enough to offset the lower payroll tax. Wages including payroll tax therefore fall visibly at the age threshold. At ages 66 and above, the 2019 tax-inclusive profile lies close to the 2015 profile; both use a contribution rate of 10.21 percent. The figure illustrates how lower employer contributions can reduce the cost of employing older workers without a fully offsetting increase in gross wages. These are descriptive age profiles among observed wage earners, not estimates of the wage response to the 2007–08 reform.

Figure C.1: Monthly Wages with and without Payroll Tax around Age 66.



Notes: The figure shows average monthly wages by age in 2015, 2016–2018 and 2019 using Statistics Sweden's Structure of Earnings Survey, covering workers in all sectors. The averages do not use the survey's sampling weights, so each observation counts equally. Wages are defined as full-time-equivalent contracted monthly wages. Observations with monthly wages below SEK 20,000 are excluded, and wages are winsorized at the 99th percentile. Plotted wages are adjusted for inflation using the CPI (base year 2019). The 2016–2018 series combines annual means using weights proportional to each year's number of observations. Age is measured as of December 31 and is based on year and month of birth. Panel A shows gross wages. For the ages shown, Panel B adds employer contributions of 31.42 percent below age 66; at age 66 and above, the rates are 10.21 percent in 2015, 16.36 percent in 2016–2018, and 10.21 percent in 2019. The 2019 rate applies after the July tax change, when the survey measures wages. Appendix B describes these statutory changes.