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Effects of Payroll Tax Cuts for Young Workers

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by

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Abstract

In response to high and enduring youth unemployment, large payroll tax cuts for young workers were implemented in two Swedish reforms in 2007 and 2009. This paper analyses the effects of the reforms on worker outcomes and firm performance in the retail industry, an important employer of young workers. In general, the estimated effects on job accessions, separations, hours and wages, are small. For workers close to the minimum wage the estimates suggest larger, but still modest, effects on the probability of job accession. There is also some evidence on increasing profits in a subsample of firms that employed relatively many young workers before the first reform, with estimated effects commensurate with small behavioural effects of the payroll tax cuts. The conclusion is that reducing payroll taxes is a costly means of improving employment prospects for the young.

Keywords: Tax subsidy, labour costs, minimum wages, retail industry
JEL-codes: H21, H25, H32, J38

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Against a backdrop of high and rising youth unemployment, the Swedish government adopted two payroll tax reforms, in 2007 and 2009. The purpose of the reforms was to increase the opportunities for young workers to gain entry to the labour market. The payroll tax reduction was relatively large – 11.1 percentage points after the first reform and 15.9 percentage points after the second one – and targeted towards young workers. The size of the reduction and the fact that it was not generally applied to all segments of the labour force should help in identifying the effects of the reforms.

This paper analyses the effects of the payroll tax reductions on employment, wages and profits in a specific industry, namely retail. There are many young workers in this industry and the share of labour costs in relation to total costs is high. The detailed payroll data used in this study also allow an analysis of the extent to which minimum wages play a role in how payroll taxes affect employment for young workers. Collectively agreed minimum wages are binding for blue-collar workers in retail, which speaks for the possibility that workers with the lowest wages may be affected differently than other workers. For a subset of firms, the payroll data have been linked with a database containing accounting information which makes it possible to analyse whether the reforms also affected firms' profits.

Standard theory on payroll taxation predicts that the consequences for employment depend on the extent to which such a tax, if levied on the employers, is shifted onto employees in the form of wage increases. However, a number of institutional factors might prevent such shifting. In the short run wages may be fixed by collective bargaining for a number of years, so that payroll tax reductions will translate into higher wages only in the long run, ultimately eroding any increases in employment. Even in the long run wage adjustment may be prevented for some marginal workers by the presence of statutory minimum wages. It has often been argued that payroll tax cuts should be targeted towards marginal groups, such as youth, the low-skilled, the work disabled and the long-term unemployed (see, for example, the OECD, 2003). With binding minimum wages, a case can be made for such a policy, since it is less likely that changes in payroll taxes will affect wages.

A number of empirical studies have investigated the links between payroll taxes, employment and wages. Reductions of payroll taxes in regional 'support areas' in the Nordic countries have been examined by Bennmarker et al. (2009), Korkeamäki and

Uusitalo (2009) and Korkeamäki (2011). None of the studies finds any evidence that employment increased in the target regions as a consequence of the payroll tax cuts, which amounted to 10 percentage points in Sweden and 3–6 percentage points in Finland.[1] However, wages seem to have increased in the support areas according to these studies (with the exception of Korkeamäki, 2011, in which the effects are mostly insignificant). Huttunen et al. (2013) examine a payroll tax reduction – up to 14 percentage points depending on the wage – targeted towards older, low-wage workers in Finland. They report no effects on employment or wages, but a slight increase in hours worked among those already in employment before the reform. Much of the evidence based on reductions of general, flat rate payroll taxes yields similar conclusions, namely partial shifting of wages and weak employment effects.[2] These empirical studies thus support the predictions of the standard theory.

Few studies, however, consider reductions targeted towards groups that may be especially susceptible to labour market rigidities. Kramarz and Philippon (2001) analyse the employment effects of the substantial reduction of payroll taxes in France – up to 15 percentage points – for workers on or close to the statutory minimum wage, most of whom are young. Their results indicate that increases in wage costs (including payroll taxes) were associated with more transitions from employment to non-employment. Results for decreasing wage costs were less clear cut; the effect on transitions from non-employment to employment seems to have been dampened by labour-labour substitution, in favour of workers whose wage costs were reduced in connection with the cut in payroll taxes.

There seems to be only one previous empirical study examining how payroll taxes affect firm performance, namely Korkeamäki (2011). He finds negligible effects on profits of the Finnish regional payroll tax reduction, which is puzzling considering the small employment and wage effects reported in the study.

The most closely related study to the present one is Egebark and Kaunitz (2013), which also examines the effects of the 2007 and 2009 reforms of payroll taxes in

[1] The Swedish payroll tax cut studied by Bennmarker et al. (2009) was in fact rather small, since the full reduction only applied to wage bills not exceeding SEK 852,000 per year, which roughly corresponds to 3 full-time blue-collar workers.

[2] See, for example, Cruces et al. (2010) for Argentina, Gruber (1997) for Chile, Bauer and Riphahn (2002) for Germany, Holmlund (1983) and Pencavel and Holmlund (1988) for Sweden, and Anderson and Meyer (1997) and Murphy (2007) for the US. An exception is Kugler and Kugler (2009), who find modest wage effects and relatively large decreases in employment following payroll tax increases in Colombia.

Sweden. They find evidence of a modest increase in employment, but little impact on wages. Unlike me, they are able to study heterogeneous effects with respect to country of birth. My analysis differs from the one in Egebark and Kaunitz (2013) in several additional ways: only those employed in a specific industry are included, rather than all employees; the analysis differentiates between entry into and exit from employment and also considers effects on hours per worker; an analysis of heterogeneity in treatment effects for workers bound by minimum wages is undertaken; and effects on firms' profits are considered.

Another related study is Bennmarker et al. (2013), which examines the wage effects of the payroll tax reductions of 2007 and 2009, also taking into account other labour market reforms during the period of study. Their results suggest mostly insignificant effects of the payroll tax cuts (in contrast to the wage effects found for earned income tax credits and reductions of the replacement rate).

My key finding for the post-reform period 2007–2011 is that employment in general was only modestly increased, at best, by the payroll tax reforms. For workers bound by minimum wages I estimate larger, but still modest, employment effects. Hours and wages seem to have been little affected. I find that profits increased in a subsample of firms employing many young workers before the first reform, which is consistent with my other results, but strong conclusions regarding profits cannot be drawn without a larger sample of firms. While the financial crisis obviously poses problems for an evaluation of the long-term effects, my results for 2007 and 2008 – before the crisis set in with full force – do not suggest more than slight increases in employment.

The paper is organised as follows: The next section discusses the payroll tax reforms of 2007 and 2009 in more detail as well as describing the most important features of the Swedish payroll tax system in general. Other reforms during the period of study that may have impinged on labour market prospects for young workers are considered in Section 2. Section 3 provides a brief account of the theoretical arguments regarding the effects of payroll taxes, with special attention paid to the case with minimum wages. Section 4 deals with the specifics of wage formation in retail for blue- and white-collar workers. The data for the retail industry and the empirical specification are presented in Section 5. In Section 6, the econometric results are discussed, while Section 7 summarises the results and deals with policy implications.

1 The Swedish payroll tax system and the reforms of 2007 and 2009

Swedish payroll taxes are levied on employers and are basically proportional to the wage bill. The legally mandated system of payroll taxes covers all employers. Employers bound by collective agreements with trade unions are also subject to collectively agreed payroll fees, on top of the taxes. In the private sector, there are separate agreements for blue- and white-collar workers. Separate agreements also apply for workers employed in the public sector.

The payroll tax reforms in 2007 and 2009, implying substantial reductions in the tax rates for young workers, were initiated as a response to growing concerns about rising youth unemployment. At the time, the relatively high unemployment rate among the young in comparison with other countries was often pointed out in the public debate.

The explicit purpose in the bill behind the first reform, presented to the *Riksdag* on 15 March 2007, was to increase the opportunities for young people to gain entry to the labour market (Government bill 2006/07: 84). The cut in payroll taxes, from 32.42 to 21.32 per cent for workers aged 19 to 25, gained legal force on 1 July 2007. Limiting eligibility to persons at least 19 years old was motivated by concerns that a lower age threshold would increase incentives to drop out of high school, which is normally finished in the year during which the pupils turn 19. The motivation behind the upper age limit was that young workers supposedly have gained sufficient labour market experience by the age of 25, so a tax reduction should have little import.

The second reform was implemented on 1 January 2009. The payroll tax rate was decreased further, from 21.32 to 15.52 per cent, and the lower age threshold was abolished and the upper one extended to 26. An explicit purpose in the bill of 25 September 2008 was to create permanently higher employment in the target group through the tax cut (Government bill 2008/09:7). The government's motives for abolishing the lower age limit for eligibility was that the rules would be simpler to apply and that the demand for younger workers, including those seeking holiday work, would increase. The motivation for increasing the upper age limit was rather vague, simply given as a way of 'reinforcing the efforts of getting more young people into work'.

Statutory payroll taxes consist of the following components (with the rates before the first reform for 1 January 2007, totaling 32.42 per cent, in parentheses):

- sickness insurance fee (8.78 %)
- parental insurance fee (2.20%)
- old-age pension fee (10.21 %)
- pension for surviving family members fee (1.70 %)
- labour market fee (4.45 %)
- work injury fee (0.68 %)
- employers’ fee (4.40 %)

All of the components are linked to benefits conditional on labour force participation, except the employers’ fee which then acts a pure income tax. However, as discussed by Flood et al. (2013), the link to benefits is not direct for the other components of the payroll tax. Earnings below or above certain thresholds (varying depending on component and related to ‘basic amounts’) do not generate additional benefits, but these thresholds apply to few young workers. Even for earnings between the thresholds, the link to benefits is not complete. Sometimes the collected fees have been used for other purposes than social insurance benefits and deficits have been covered by other taxes. Flood et al. (2013) estimate that for income earners between the two thresholds about 40 per cent of the payroll taxes constitute a pure income tax and this estimate may well be a good approximation also for young workers.

According to the reform implemented on 1 July 2007, the rates applying to all components except the old-age pension fee were reduced by 50 per cent for young employees. This implied a reduction of (1–0.5)(32.42–10.21) = 11.1 percentage points in total payroll taxes for this group. Since both total payroll taxes and the old-age pension fee remained the same in 2008, the formula implied an 11.1 percentage point reduction also during this year. The reduction of payroll taxes became more generous on 1 January 2009, as only 25 per cent of the components besides the old-age pension fee had to be paid, implying a reduction of (1–0.25)(31.42–10.21) = 15.9 percentage points. Since 2009 the formula, and the reduction in percentage terms, have remained the same.

The payroll tax cuts for young workers were not associated with any reductions in the benefits linked to these taxes.

Figure 1 depicts the evolution of payroll tax rates over the period 2000–2011.[3] The regular rate has not changed much – the variation over time is only 1.5 percentage points. The rate stood at 32.9 per cent in 2000 and had declined to 31.4 per cent by 2011. The first payroll tax reform of 2007 implied a rate of 21.3 per cent for 19–25-year-olds and in the second reform of 2009 the rate was reduced further, down to 15.5 per cent, and the group of eligible workers was extended to include all individuals up to age 26.

For evaluation purposes, it is some interest to examine the letter of the law and how legal formulations may have influenced public perceptions regarding the eligibility for the payroll tax reductions. The legal document specifying the details of the first reform in 2007 contains the following, core sentence: 'On the compensation to persons who *at the commencement of the year* have turned 18 years of age but not 25, the full old age pension fee but only half of the other payroll taxes should be paid' (SFS 2007:284, my translation and italics). The implication of this somewhat complicated formulation – it seems more straightforward to refer to someone's birth year instead[4] – is that the payroll tax cut applied to those who turned at least 19, but not 26 or more, during the year when the reform was first implemented on 1 July 2007. A similar formulation was used when the tax cuts were extended in 2009: 'On the compensation to persons who at the commencement of the year have not turned 26 years of age, the full old age pension fee but only a quarter of the other payroll taxes should be paid' (SFS 2008:1266).

The legal formulations may have invited misunderstandings regarding eligibility for the payroll tax cuts. For example, several press reports stated that the first reform applied to 18–24-year-olds and the second one to individuals below the age of 26. It is difficult to assess how widespread any misunderstanding has in practice been among employers in the retail industry, and to what extent take-up rates have been affected. Evidence on special payroll tax reductions for disadvantaged groups from Belgium and the Netherlands, reported in Marx (2001), suggests that mainly three factors contribute to the non-take-up among firms: (i) unawareness of the reduction; (ii) perceptions that the reduction is temporary; and (iii) perceptions that the take-up is associated with large

[3] The regional reduction of 10 percentage points, in effect from 2002, is not accounted for in the figure.
[4] This kind of formulation would require that it be changed every year, though.

administrative costs. Moreover, non-take-up turned out to be more prevalent among small firms, possibly due to the fact that fixed costs of information-gathering and administration are spread out over fewer employees than in large firms.

These findings may be of relevance also in the context of the Swedish reforms, except that the associated administrative costs should be negligible in the relation to reductions in total wage costs, since no application procedures were necessary. The government never stated explicitly that the reductions were of an experimental or temporary nature, but the political parties in opposition were against them before the general elections in 2010 (which they lost). Any misperceptions regarding eligibility of payroll tax cuts in terms of age may have been more pronounced among small firms, especially those with no or few young employees before the reform. However, such misperceptions should abate over time as the likelihood of gaining access to the correct information increases.

2 Other reforms in 2007

Three additional reforms were undertaken during the period of study that potentially could impinge on labour market outcomes for young people. The reforms concerned income taxation, employment protection legislation and active labour market policy.

First, earned income tax credits were introduced in 2007, that is, in the same year as payroll tax rates were cut for the first time. The tax reductions applied to all earned income for all workers, regardless of age, and were extended in three additional stages during 2008–2010. As low-income earners received somewhat larger tax credits in relation to their income than persons with higher income, it is conceivable that employment and wages among young workers were affected in a different way than those for older workers. For example, the tax credits could have contributed to an increase in labour supply, lower wages and increased employment, and especially so among the young. If this is the case, employment estimates of the payroll tax reform could be biased upwards. From the analysis of Edmark et al. (2012) labour market effects of the tax credits cannot be established with any certainty. However, Bennmarker et al. (2013) conclude that the reforms contributed to lower wage pressure.

Second, another reform in 2007 made it easier for employers to hire workers on a temporary basis. New legislation allowed employers to use fixed-term contracts for any reason and for a period of up to 24 months (the previous maximum was 12 months). The loosening of the regulation may have had an impact on the employment of the young, among whom temporary work is relatively more widespread. It is difficult to determine the direction of the potential bias on the estimated employment effect of the payroll tax reform, since more use of temporary contracts could contribute to an increase in both hirings and firings. On paper, the reform was far-reaching. This is reflected in the OECD's index of regulation of temporary work for Sweden, which was reduced from 1.6 to 0.9 (on a scale from 0 to 6). However, as the Swedish system of employment protection legislation allows employers and unions to depart from substantial parts of the legislation in collective agreements, legal changes do not necessarily translate into changes in practice. According to Skedinger (2012b) only 4 per cent of temporary workers were employed with the new contracts in 2010, which suggests that the reform had little impact on actual hiring practices in the labour market during the period of study.

Finally, the New-Start Job scheme (*nystartsjobb*) was introduced in 2007. The scheme is targeted towards people who have been unemployed or received sickness or disability benefits for at least one year, waiving all payroll taxes for employers who hire someone in the targeted group, for as long as the non-employment spell lasted and up to 5 years. In 2009, the size of the employment subsidy doubled (amounting to 62.8 per cent of the wage). From the start, special rules applied to those aged 21–26: eligibility already after 6 months of non-employment and a maximum period of 1 year in the scheme. It is not possible to identify participants in the New-Start Job scheme in the data, which means that its employment effects could be wrongly attributed to the payroll tax reductions under investigation in the empirical analysis, implying an upwards bias. Very few young people took part in the scheme. It increased in size from 10,000 participants in July 2007 to 45,500 on average in 2011, of which only 3,900 were 18–24 years old (according to the Public Employment Service). During the period 2007–2011, at most around one per cent of all employed persons aged 18–24 participated in the New-Start Job scheme (according to Statistics Sweden). However, to the extent that the treatment and controls in my analysis are treated differently by the New-Start Job scheme this might affect the results.

In conclusion, due to the few young workers involved there is little to suggest that the loosening of regulation of fixed-term contracts or the introduction of the New-Start Job scheme should seriously distort my evaluation of the payroll tax reform. Since the tax credit reform applied to all young workers it cannot be ruled out that it had an impact on the labour market for the young.

3 Payroll taxes in economic theory

A core result in the standard theory on payroll taxation states that the consequences for employment depend on the extent to which such a tax, if levied on the employers, is shifted onto employees. If, say, a reduction of the payroll tax rate is fully shifted to employees in the form of a wage increase, equal to the payroll tax reduction in percentage terms, no impact on employment is expected. In the case of partial shifting, in which the wage increases by less than the percentage reduction in the payroll tax, the demand for labour will increase. The more closely tied payroll taxes are to benefits valued by workers, which tends to be the case for components related to social security contributions, the more shifting is likely to occur (Summers, 1989).

A number of institutional factors might prevent shifting to wages in the short run, however. For example, with collective bargaining wage rates may be set at fixed levels for a number of years and adjustment will only occur in the longer run as wages are re-negotiated when the agreement expires. In this context, the degree of shifting to workers in the longer run is also likely to depend on the bargaining power of trade unions vis-à-vis employers.

In the standard textbook model of tax incidence, with perfect competition in factor and product markets, it does not matter whether a tax subsidy is provided to the employer or employee. The equilibrium quantities of labour are determined by the elasticities of demand and supply and factor substitutability. However, if one exogenously imposes a statutory minimum wage that exceeds the equilibrium wage for a certain segment of the labour force, i.e., a binding minimum wage, the effects of income tax cuts and payroll tax cuts will no longer be similar. With binding minimum wages a shift in the supply curve induced by an income tax cut will not necessarily increase employment. For a given pre-tax wage, workers want to supply more labour than employers demand. However, if the government reduces the payroll tax the demand curve shifts so that employment increases. At any given pre-tax wage, employers want to hire more labour since it has become less expensive to do so.

It is thus far from obvious that the reasoning regarding payroll taxes for the labour market in general applies with equal force to the labour market for the low-paid (Lee and Saez, 2012; Nickell and Bell, 1997; Pissarides, 1998). With collectively agreed minimum wages, as in Sweden, it remains an open question how these rates evolve in response to changes in payroll taxes. If the payroll tax cut triggers a minimum wage hike, employment will not necessarily increase in the long run.

In effect, an implicit zero-profit condition assumes away any effects on profits in tax models that rely on perfect competition in the product market. However, in the short run and with imperfect competition lower payroll taxes may well translate into higher profits.

4 Wage formation in retail

In the Swedish retail sector, wages for blue-collar workers are determined in collective agreements between the Commercial Employees' Union (*Handelsanställdas förbund*) and the Swedish Trade Federation (*Svensk Handel*). White-collar workers in retail may be covered by different collective agreements. The employers' agreement with *Tjänstemannaförbundet HTF* (merged into *Unionen* in 2008) was the major agreement in the sector during the period of study, covering lower-level white-collar occupations requiring secondary education. Employees in white-collar occupations requiring tertiary education are covered by employers' agreements with different associations, depending on occupation, within the Swedish Confederation of Professions (SACO).

Of major interest in this study are the agreements covering the majority of young workers, namely those involving the blue-collar workers in the Commercial Employees' Union and white-collars in *Unionen*. During the reform period analysed in the study, two agreements for blue-collar workers have been effective. The first such agreement covered the period from 1 April 2007 to 31 March 2010 and the second relates to the period 1 April 2010 to 31 March 2012. The main agreements for white-collar workers were also two by number during the reform period and implemented at about the same times and with the same lengths as those for blue-collars (from 1 May 2007 to 30 April 2010 and from 1 May 2010 to 30 April 2012). According to my conversations with representatives of the employer organisation, negotiators on both sides were well aware of the forthcoming cut in payroll taxes for young workers during wage negotiations in the spring of 2007.[5] Thus it cannot be ruled out that the reform had an impact on the outcome of the negotiations even before the reform was implemented.

The two above mentioned agreements specify contractual wage increases as well as minimum wage levels at the industry level for various categories of workers.[6] Regardless of contract length, contractual wage increases and minimum wage levels are determined on a year-to-year basis. In the agreement for blue-collars, minimum wage rates are differentiated by age and experience.[7] Typically, the same minimum wage increase in SEK per hour or month applies to most age groups, so rates for younger and more inexperienced workers usually increase more in percentage terms. Similarly, contractual wages tend to increase by the same amount in SEK for all workers, regardless of age. Minimum rates for white-collar workers are conditional on age only and two different rates apply, to workers aged 20–23 and 24 or older. Minimum wages for blue-collar workers in retail are binding, with distinct spikes at the minimum wages in the wage distribution (Skedinger, 2013). As only few of the white-collar workers are thus affected by minimum wages, it seems unlikely that minimum wage increases should have any effect on actual wages for this category of workers in the retail industry.

Local wage formation is another source through which the payroll tax reforms could impact on the wages of young workers. For blue-collar workers in retail, contractual wage increases consist not only of a general increase, applying to all workers, but also a ‘wage pot’ to be distributed at the local level to all workers at least 18 years of age (National Mediation Office, 2012). Over the period 2007–2010, the amounts allotted to the wage pot have constituted 40 per cent of the total wage increase in the agreements. In the agreement for 2011, the share increased to 50 per cent. For white-collar workers

[5] The Centre-Right coalition announced their intention to reduce payroll taxes for young workers in the 2006 election campaign. The first reports in the press mentioning 1 July 2007 as a possible date for the reform seem to be dated 5 October 2006, two weeks after the coalition having won the elections (Brors, 2006).

[6] Due to high coverage of collective agreements, there are *de facto* minimum wages in Sweden, despite their absence *de jure*. Rates for blue-collar workers are in general among the highest in the world, both in terms of absolute levels and in relation to other wages in the economy (Skedinger, 2010).

[7] Different scales apply for workers aged 16, 17, 18 and those aged 19 or older. For workers aged 18 or older who have acquired industry-specific experience, rates are differentiated by such experience (1, 2 or 3 or more years).

covered by the *Unionen* agreement, wage formation is more decentralised than for blue-collars as the agreement specifies a 'wage pot' for local distribution supplemented with rules guaranteeing increases also at the individual level.

It is inherent in the design of the wage bargaining system for blue-collar workers that a larger wage increase in per cent accrues to young workers than to older ones (it seems unlikely that this is undone through the distribution of the wage pot at the local level). However, jacking up contractual wages as a means of taking advantage of a cut in payroll taxes targeted at the young is quite a blunt instrument for the union, since older workers also would receive a higher wage. A minimum wage hike seems to be a more plausible outcome.[8] The more decentralised wage bargaining system for white-collar workers implies a wider scope for firm-level bargaining to affect wages for the young, but even in this context it may be difficult for unions at the local level to implement targeted wage increases. The difficulty may apply to blue- and white-collar unions alike and arise from relative wage concerns – an increase for the young may trigger wage demands from older workers in order to keep relative wages intact. To the extent that unions instead try to raise wages for *all* workers, regardless of age, the size of such wage increases is likely to be smaller than with an across-the-board payroll tax cut.

5 Data and empirical specification

The payroll data set has been obtained from the Confederation of Swedish Enterprise (*Svenskt Näringsliv*) and covers all member firms of the employer organisation the Swedish Trade Federation over the period 2000–2011. According to the website of the Federation, there are 13,000 member firms with a total of 300,000 employees, implying a coverage of about two thirds of all employees in Swedish retail. The firms are bound by the collective agreements signed by the Federation and these cover all employees, regardless of union membership.

In the data set, workers are observed once a year, in September. Thus, a worker is included in the data only if he or she worked in a member firm in retail during the month of September in a given year. The data are based on payroll records and include information on employee category (blue- or white-collar), various components of pay,

[8] Skedinger (2012a) documents an increase in the minimum wage relative to the average wage in retail during the period 1995–2010, from 75 to 81 per cent.

actual and usual hours worked, gender, age, occupation, region and number of employees in the firm.

The payroll data set contains unique identifiers for firms and workers. The definition of accessions and separations follows standard procedures in the kind of data used here. An accession in year t is defined as the worker being present in the data in year t, but not in $t–1$, while the firm is present in both t and $t–1$ (but not necessarily during other periods). Accordingly, a non-accession in year t is defined as the worker being present in the data in both t and $t–1$, with the firm also present in both periods. Observations for workers in year t for which the firm is not present in $t–1$ are assigned missing values for the accession variable. Since some, mostly small, firms for various reasons may not report data in a given year, even though they are still members of the Federation, this procedure ensures that the employees of non-reporting firms are not erroneously classified as entrants. Analogously, a separation in year $t+1$ is defined as the worker being present in the data in year t, but not in $t+1$, while the firm was present in both t and $t+1$. It is not possible to distinguish between voluntary and involuntary separations in the data. It should be noted that transitions between firms in the retail industry are not counted as accessions or separations, only those that involve a worker entering or leaving the industry. Given that involuntary separations cannot be identified, separations defined in this way capture relatively more exits into non-employment than a measure which includes intra-industry transitions. Similarly, the measure of accessions captures relatively more of transitions from non-employment to employment.

For comparability across samples, the computation of hours and wages is also conditioned on the presence of the firm in the data in two subsequent years. The measure of hours is based on *usual* hours per week (which could be part-time or full-time), not actual hours during the measurement period, in order to filter out disturbances specific to the reporting month. The data contain a direct measure of the *regular* hourly wage (*fast timlön*), which is likely to be measured with little error.[9] The wage concept used thus excludes premiums for unsocial hours, overtime pay, bonuses and fringe benefits.

[9] A minority of blue-collar workers in the retail industry and most white-collars are salaried (see Table 1.a). For these workers, regular full-time monthly wages (*fast heltidsmånadslön*) have been transformed into regular hourly wages under the assumption of a 40-hour working week.

Minimum wages for blue-collar workers have been collected from the Retail Agreement (*Detaljhandelsavtalet*). Each blue-collar worker in the data set has been assigned a minimum wage, depending on the relevant personal characteristics, such as worker category, age and professional experience within the industry. This procedure was not performed for white- collar workers, since minimum wages are not binding for them.

A worker's attachment to the job is likely to influence mobility. There is unfortunately no direct information on the use of fixed-term contracts, which is widespread in the industry, but there is a variable in the data set indicating whether the worker is salaried. Salaried workers are typically less mobile, with little long-term attachment to the job.

Some observations have been excluded from the payroll data: (i) observations in municipalities within the regional support areas, subject to a different payroll tax regime (these observations were also excluded in the firm-level analysis of profits); (ii) observations for individuals with multiple jobs, due to difficulty in defining the dependent variables; and (iii) in the wage regressions, observations with very low wages (below 75 per cent of the lowest minimum wage for blue-collar workers), in order to minimise the influence of measurement errors.[10]

For the purpose of analysing firms' profits, the payroll data set has been linked with the IFN Corporate Database, a data set containing accounting information. The information has been validated by the consulting firm PAR, based on original data from the Swedish Companies Registration Office (*Bolagsverket*), a government agency that records accounting information of limited liability corporations in Sweden. Not all firms in the payroll data set had the appropriate firm identifier for linking with the IFN dataset, so it was possible to match only a subset of firms (10 per cent of firm-year observations) in the payroll data.

The profit margin, basically profits (revenue minus costs) over revenue, is a standard measure in econometric analyses of profitability. I use two different variants of profit margins, before and after financial items, in the empirical analysis. The first measure (*rörelsemarginal*) is profit margin before (i) financial items, such as interest, capital

[10] Exclusion (i) reduces the number of observations by around 4 per cent, (ii) by 1 per cent, and (iii) by 7 per cent, depending on specification. The total reduction is roughly 5 per cent in the employment and hours regressions and about 12 per cent in the wage regressions.

gains and losses; (ii) non-recurring items; and (iii) company taxes (but, of course, *after* payroll taxes). The second measure (*vinstprocent*) is profit margin before (ii) and (iii), but after (i). The two measures are intended to capture 'normal' operating profits, which may differ substantially from the bottom-line profits (*rörelseresultat*) also reported by firms. In line with common practice, two exclusions were performed. First, I discarded observations for firms with a financial year straddling the date of the first reform, 1 July 2007. Second, a small number of outlier observations on profit margins, that is, values smaller than –1 or larger than 1, were excluded.[11] Other useful variables in the IFN Corporate Database include the firm's payroll tax contributions and management salaries.

Figures 2–5 show the evolution of job accessions, separations, hours and wages in retail for the treatment group, 19–25-year-olds, in relation to 27–29-year-olds over the period 2000–2011.[12] Since the 26-year-olds were subject to treatment as a consequence of the second reform in 2009, they are not included in the comparison group. To shed light on any differences within the treatment group, I have split it into two age groups, 19–20-year-olds and 21–25-year-olds. For white-collar workers, the younger treatment group contains few observations so only the older one is included in the figures.

Figure 2 shows accession rates for blue- and white-collar workers. The years before the payroll tax reforms in 2007 and 2009 are indicated by vertical lines. The figure for blue-collar workers reveals that rates are considerably higher for the younger treatment group than for the older one and that cyclicality is more pronounced among the 19–20-year-olds. For blue-collars there is a downward trend in accession rates, reflecting the deteriorating labour market situation for the young during the recent decade. This is highlighted by the sharp decline in rates for all groups in 2009, with the onset of the financial crisis. Pre-reform trends before 2007 seem reasonably parallel for the older treatment group and the control, but the trends are quite different depending on worker category. In the first two years after the reform there is an overall decline in accession rates, but among blue-collars the decline is somewhat smaller in the treatment group. The drop in rates in 2009 is more accentuated in the younger treatment group among blue-collars. After 2009, the treatment groups seem to recover after the decline, while

[11] The exclusions resulted in losses of observations of around 14 per cent (straddling financial year) and 0.5 per cent (outliers).

[12] For consistency with the eligibility requirements in the payroll tax reforms, age in the figures and in the econometric analysis is based on birth year, and not on actual age at the time of measurement.

accession rates in the comparison group remain at a low level. The picture is somewhat different among white-collars, as the decline in rates is sharper in the treatment group than in the comparison group during 2007–2009, but the recovery in the ensuing period is stronger.

Separation rates in retail are presented in Figure 3. Pre-reform trends do not appear as parallel as was the case for accessions. It is of some interest to note that separations did not increase in connection with the financial crisis initiated in 2009. On the contrary, exits *declined* during this year (note that separations refer to year *t+1* in Figure 3, so the observation for 2008 indicate whether the individual was separated in 2009). The brunt of adjustment during the crisis thus fell on the new recruits in retail, rather than on the young people already employed there.

Figure 4 shows the evolution of weekly hours. Once more, the 19–20-year-olds exhibit high cyclicality relative to the other age groups. Post-reform development for blue-collars does not suggest that hours increased in the treatment groups relative to the control. Among white-collar workers, pre-reform trends diverge sharply. Whereas hours in the control group remained stable (and were close to full-time work on average), hours declined in the treatment group. A partial recovery occurred just before the reform, in 2006, and continued up until 2008. In 2009, there was a decline in hours among both groups, and especially among the treated. The relative decline continued into 2010–2011.

Over the period 2000–2011 there were increases in real hourly wages in the retail industry, as evidenced in Figure 5. Among blue-collars, wages rose by 30 per cent for those aged 19–20, by 25 per cent for the 21–25 age group and by 21 per cent for the 27–29-year-olds. Since wages increased faster among the youngest, the figures also imply wage compression across the three age groups, which is consistent with the rising minimum wages in relation to median wages in the industry that have been documented in Skedinger (2013). Real wages increased more in percentage terms after the first reform than before for all age groups and even continued to increase at the onset of the financial crisis in 2009. In the wake of the crisis, wages remained rather stable, with a small decrease in 2011. There was no wage compression across age groups among white-collar workers, although wages increased overall over the period. In connection with the crisis, white-collars exhibited more of wage moderation than blue-collars, which suggests important differences in wage formation between the two worker categories.

Descriptive statistics in the individual-level sample of 19–25-year-olds and 27–29-year-olds are shown in Table 1.a. The table confirms that there are considerable differences between blue- and white-collar workers. For example, the former tend to be younger and have a smaller proportion of males (although males form the minority also among white-collars). Moreover, blue-collars are bound by minimum wages, which is not the case for white-collar workers. About 65 per cent among the youngest blue-collars have a wage that is at most 5 per cent above the minimum wage that is relevant to that individual (dependent on age and experience in the industry). For the older age group, the corresponding figure is 47 per cent. Almost 40 per cent of the younger and 20 per cent of the older workers have a wage that is at most 1 per cent above the minimum wage. Table 1.b presents descriptive statistics for the firm data, separately for matched and non-matched firms. Profit margins in matched firms are 3–4 per cent on average, depending on definition. The subset of matched firms represents merely 10 per cent of all firm-year observations in the data, but accounts for 42 per cent of total employment, since the firms are relatively large on average. The matched and non-matched firms are similar in the pre-reform intensity of using workers aged 19–25 in their workforces, with shares on average amounting to 22 and 24 per cent, respectively.

The empirical strategy in the analysis of worker outcomes is to use a difference-in-difference (d-i-d) approach to compare changes in the variables (accessions, separations, hours and hourly wages) before and after the changes in payroll taxes. The high cyclicality of the workers aged 19–20, revealed in the previous figures, makes it problematic to use them as a treatment group in the empirical analysis of the payroll tax reforms. I have chosen to instead use workers aged 21–25 as the benchmark treatment group, with 27–29-year-olds as the control, but also experiment with both larger and more narrowly defined treatment and control groups in terms of age.

Based on the data on individuals and firms, I estimate the following linear regression for worker outcomes:

$$Y_{it} = \alpha_0 + \alpha_1(\text{Treated\_Age\_Group})_{it} + \alpha_2 \text{Post}_t + \alpha_3(\text{Treated\_Age\_Group} * \text{Post})_{it} + x'_{it}\beta + \varepsilon_{it}, \quad (1)$$

where subscripts *i* and *t* represent the individual worker and time, respectively. In the analyses of accessions, the dependent variable is equal to one if a worker is newly hired in the industry at time *t* and zero otherwise. In regressions on separation behaviour, the dependent variable equals one if an individual is separating from the industry at time *t+1* and zero otherwise. *Treated_Age_Group* is a dummy variable for belonging to the treated age group at time *t*, *Post* is a dummy variable for the post-reform period, and *Treated_Age_Group*Post* is an interaction term between *Treated_Age_Group* and *Post*. The coefficient for the interaction term is the d-i-d estimate of the reform effect, reflecting the differential effect on the age group affected by the change in payroll taxes. The d-i-d estimator allows for both group-specific and time-specific effects.

Furthermore, x_{it} is a vector of individual characteristics, namely dummies for gender, age, region, occupation, salaried position and year (which controls for common shocks to the business cycle). The additional explanatory variables account for the possibility that characteristics are systematically different between the age groups before and after the policy change (compositional bias), but should not be affected by the treatment.

Using the appropriate treatment and control groups is a key issue in identification. Egebark and Kaunitz (2013) contains a useful discussion of this issue in the context of the payroll tax reforms under study. The ideal control group should be as similar to the treatment group as possible, but should not be affected by the treatment. The usual approach in evaluations of policies targeted towards young workers is to use slightly older workers as a control.

First, there is the well-known argument that if employers substitute young workers for slightly older ones in response to the payroll tax cut, estimates of the treatment effect will be biased upwards due to a *substitution effect*. The magnitude of this effect will depend on the extent to which employers regard workers of different ages in the two age groups as close substitutes in production. From a policy viewpoint, some substitution may be acceptable as long as employment in the targeted group increases, but the fact remains that estimates of the reform will be distorted.

Second, the reduction in the cost of a factor of production also results in a *scale effect*. Under plausible assumptions this effect implies an expansion of output, which could potentially result in the employers hiring more of older and more productive workers than of younger workers. If this is the case, the scale effect counteracts the substitution effect. A scale effect of this type could be more likely in firms already employing a large share of young workers, but it is probably small in relation to the substitution effect.

Third, as pointed out by Egebark and Kaunitz (2013), treatment is not uniform across age groups within the treatment group. On the one hand, a younger worker is subject to treatment over a longer period than an older worker, which increases incentives to hire the former instead of the latter in the presence of fixed costs of recruiting a new worker, due to hiring and training costs. On the other hand, it is a stylised fact that quits are relatively more common among younger workers, which strengthens incentives to hire older workers within the treatment group. In general, the expected present value to the employer of the payroll tax reduction will be larger for younger workers, unless quit rates among them are not too high. Figure 6 illustrates separation rates (from the firm, not the industry) by age at the time of hiring and tenure before the payroll tax reforms, as an average over the period 1998–2005. Separation rates are consistently higher among workers with shorter tenure and among blue-collar workers. Among the latter, separation rates decline more steeply with tenure for younger workers than what is the case for older ones. This implies, for example, that younger workers with at least three years' tenure in most cases exhibit lower separation rates than older workers with the same tenure. Figure 7 attempts to describe how these differences in separation rates impinge on the expected present value of receiving the payroll tax subsidy (set to unity for simplicity) at the time a worker is hired, depending on the age of the worker.[13] The age profiles reveal that the expected present value of the payroll tax subsidy reaches its maximum for 20-year-olds among blue-collar workers and for 22-year-olds among white-collars. The present values then diminish for older workers among both worker categories, and especially so for 25-year-olds. Taken at face value, these calculations imply that 25-year-olds have been subject to 58 and 54 per cent less treatment,

[13] The expected present value is calculated according to the formula $EPV_{j,s} = S\sum_{k=1}^{7}\beta^{k}\prod_{i=1}^{k}(1-\delta_{i,j,s})$, where index *j* represents type of worker (blue-collars or white-collars), *s* represents age at the time of hiring, S is the payroll tax reduction, set to unity, β is the discount factor, set to 0.95 and δ is the separation rate. The sum is calculated for tenures of length *i*, between 1 and 7 years, depending on the age at the time of hiring. By using historically observed separation rates in the calculations, it is assumed that relative separation rates across treated age groups are not affected by the payroll tax reduction.

respectively, relative to the age group treated most intensively among blue- and white-collar workers.

When examining the effects on wages of the payroll tax reforms, it may be the case that wages for *all* workers are driven up, subject or not subject to tax reductions. Then it is only possible to capture the relative wage effect with the methodology used in this paper.

A somewhat different approach than in (1) is used in the examination of profits. Since the vast majority of all firms are treated, in the sense of employing at least one young worker, there is no suitable control group. The characteristics of the data thus limit the usefulness of d-i-d but allow an analysis exploiting the variation in treatment intensity across firms, as in the linear specification below:

$$Y_{it} = \alpha_0 + \alpha_1(\text{Pre-reform\_Treatment\_Intensity})_j + \alpha_2\text{Post}_t + \alpha_3(\text{Pre-reform\_Treatment\_Intensity * Post})_{jt} + z'_{jt}\delta + \varepsilon_{jt}, \qquad (2)$$

with subscript j referring to the firm. The variable *Pre-reform_Treatment_Intensity* is the percentage of workers aged 19–25 in the firm, calculated as an average over the pre-reform period 2004–2006. The reform effect, namely the extent to which pre-reform treatment intensity is reflected in higher profits after the reform, is captured by the coefficient of the interaction *Pre-reform_Treatment_Intensity * Post*, although its interpretation is less straightforward than in the d-i-d approach. Firm characteristics include the share of blue-collar workers, to account for the skill-intensity of the firm, and dummies for region and year.

6 Econometric results

This section is divided into two parts. The first part contains regressions at the individual level, dealing with effects on job accessions, separations, hours and wages, while the firm-level analysis in the second part is concerned with profits, managerial pay and payroll tax contributions. The before-period is 2004–2006 throughout the estimations in this section. Using 2004 as the starting year is suitable since occupational codes changed during that year.

6.1 Employment and wages

The after-period is prolonged successively by one year, so the first regression refers to the estimation period 2004–2007, the second one to 2004–2008, and so on up to 2011. Due to the differences in wage formation between blue- and white-collar workers separate regressions will be run for the two groups. T-statistics have been clustered at the firm level, which is the most conservative alternative.

When interpreting the potential employment effects of the accession and separation variables, it is important to keep the following in mind. On the one hand, it is not unlikely that young entrants into the industry transit from non-employment to employment to a larger extent than slightly older entrants. On the other hand, separations to non-employment may well be relatively more prevalent among the treated. To the extent that such asymmetries are time-invariant, the d-i-d approach ensures that the estimated effects on entry and exit will not be distorted. However, if the treated are more cyclically sensitive than the control, estimates may be biased. The upshot of this is that estimates for the period including the financial crisis (from 2009) are more problematic to interpret than estimates for preceding years.

To save space, only the estimate of the most relevant variable, the d-i-d estimator (*Treated_Age_Group*Post*) is presented (full regressions are available from the author upon request). Table 2 shows regressions with the benchmark, 21–25-year-olds, as the treated age group and 27–29-year-olds as the control. The first column refers to entry into the industry (job accessions in year *t*), the second to exit from the industry (job separations in year *t+1*). Concerning hours and wages, it seems useful to distinguish between effects for new hires and effects for incumbent workers, with superscripts *new* and *inc*, respectively, since the effects are not necessarily identical. Thus, the third column refers to the log of weekly hours among new recruits in year *t*, the fourth to the log of weekly hours among incumbent workers in year *t* (who were employed both in *t* and *t–1*), the fifth to log of hourly wages among new recruits in year *t* and, finally, the sixth column refers to the log of hourly wages among workers in year *t* (who were employed both in *t* and *t–1*).

The estimates for blue-collar workers, in the upper panel, indicate modest effects on the probability of entry into the retail industry, regardless of the time period considered. The short-run estimate for 2007 is 0.017, which is significant only at the 10 per cent level. This implies that the probability of job accession increased by 1.7 percentage points, in the treated group relative to the control. For the longest observation period, 2004–2011, the coefficient indicates that the probability of entry increased by 1.5 percentage points. The coefficients for the probability of exit are also small, but are increasing over time and reach 0.015 by 2011. A rough estimate of the short-term increase in net employment is 1.7–0.2=1.5 per cent and zero in the long run.

The results in the third and fourth columns do not suggest that the reforms were associated with more hours worked, either among new recruits or incumbents. On the contrary, the estimates are mostly negative, although predominantly insignificantly so. The short-run effect for 2007 indicates no effect at all on hours. There is some evidence in the fifth and sixth columns of increasing wage pressure over time, but the effects are small. In the short run, the estimated effect is close to zero and for the longest observation period it is around 0.5 per cent.

The combined results in Table 2 for blue-collar workers imply a short-run elasticity with regard to total wage costs of about –0.19.[14] The magnitude of this elasticity is within the range of those estimated for all industries by Egebark and Kaunitz (2013) for the more narrowly defined treatment group of 25-year-olds (–0.14) and the wider group aged 19–25 (–0.30). Thus, consistent with the intentions behind the reforms, there seems to be an increase in employment in retail among the young, but the effects are not persistent and small in relation to the sizeable reductions of payroll taxes that were implemented.

The smaller samples for white-collar workers, in the bottom panel of Table 2, make the estimates less precise. The results are hardly more encouraging, though. There is no discernible positive effect on net employment even in the short term. Hours among incumbent workers increased in the first few years after the 2007 reform, but this result must be viewed with some skepticism due to the non-parallel pre-reform trends exhibited in Figure 4. For incumbent workers there is also a notable increase in wages already in the short run, which is consistent with more decentralised wage setting among white-collar workers than among blue-collars.

In 2009, the financial crisis erupted and this seems to have had repercussions on the estimated reform effects in Table 2. The probability of entry and hours of work are both

[14] The elasticity, which is based on a zero wage effect and includes the union-negotiated payroll fee for blue-collar workers, is calculated as 0.015/[–11.1/(132.42+7.1)] = –0.19. Unlike conventional elasticities, it is conditional on employment.

reduced in the estimates for 2004–2009, compared to estimates for the previous period, while there is no sign of more wage restraint. The additional payroll tax cut implemented in 2009 may have mitigated the decline of labour market prospects for young workers during the crisis but did certainly not eliminate it. However, the estimates of the reform effects including the period 2009–2011 may be less reliable than the estimates for previous periods, and not only because of the financial crisis. During 2009–2011 the control group includes some workers previously treated, namely those aged 23–25 in 2007. For example, a 25-year-old treated in 2007 is included in the control 2009–2011 as a 27–29-year old, while a 23-year-old in 2007, part of the treatment group 2007–2009, turns up in the control in 2011, aged 27. To the extent that previous treatment affects subsequent labour market outcomes, the estimated reform effects for 2009–2011 may thus be distorted.

The results in Table 2 were subject to several robustness checks, not reported in full due to space constraints (but available from the author). First, I estimated the same specifications with a panel of firms and firm fixed effects. I also tried specifications without the control variables for region, occupation and salaried position, in response to the concern that these variables may be endogenous. As a check on cyclicality, I added the output gap, that is, the difference between actual GDP and an estimate of potential GDP, interacted with the age dummies, since the employment of young workers is typically more volatile over the business cycle than the employment of older workers.[15] Estimations excluding the year 2007 were also tried. Finally, specifications including workers in regional support areas were estimated. These exercises did not alter the conclusions from Table 2 in any substantial way.[16]

Table 3 presents a number of experiments with extending and narrowing the treatment and control groups in terms of age and their impact on entry and exit. The table also tests for heterogeneous treatment effects depending on position in the wage distribution and firm size. Since it can be argued that the most credible estimates pertain to 2004–2007 – no previously treated workers are included in the control group and the crisis had not yet gained momentum – I restrict the estimates to this period and I am then also able to include 26-year-olds in the control group since they were not treated

[15] The source for the data on the output gap is the National Institute of Economic Research (*Konjunkturinstitutet*).
[16] With the output gap included, the effect on hours turns positively significant in some estimations for blue-collars (new recruits 2004–2007).

until 2009. The first rows repeat the analysis for the benchmark group in Table 2 and the others show results for the following age groups in the treatment and control: 19–25 versus 26–29, 22–25 versus 26–28 and 25 versus 26.

Two patterns relating specifically to blue-collars emerge in Table 3. The first one is that the impact on entry is larger the wider the definition of the included age groups; the estimates vary between 0.022 (19–25 versus 26–29) and –0.001 (25 versus 26) and the latter is not significant. It is not obvious how one should interpret these results. It is difficult to ascertain whether the difference is due to the reform or because the youngest workers did better anyway because their being more responsive to an improving labour market in 2007. On the one hand, to the extent that closer age groups are more comparable in unobserved productive characteristics, one may regard the results for these groups as yielding more reliable identification of the effects of the payroll tax reform. On the other hand, the smaller effects are consistent with the relatively lower expected discounted value of the tax reduction for older workers in the treatment group, indicated in Figure 7. If substitution across age groups is important we would expect larger effects in the more restricted samples, but that is apparently not the case.

The second pattern is that workers close to the minimum wage – with a wage up to 5 per cent above the minimum – seem to be affected more than other workers. The estimates for entry turn out to be around 0.030, with the exception of the most narrowly defined group for which there is no significant effect. However, the effect on entry must still be considered modest and net employment is not necessarily more favourably affected, since exits are also more prevalent than in the full samples. For white-collar workers, there is little evidence of differential impact on entry across age groups (and minimum wages are not relevant as white-collars are subject to substantially lower, and non-binding rates, see Skedinger, 2013). The results for white-collars also indicate that the (positive) effect on exits disappears with more narrow age groups. Endogeneity of minimum wages is a concern that was discussed in Section 3, but since minimum wages are never negotiated at the firm level, it seems reasonable to assume that firms take minimum rates as given when deciding on hirings and firings.

Table 3 also looks at workers in small firms, namely those with 50 employees or less. Somewhat surprisingly, neither entry nor exit seems to have been much affected by the 2007 reform (a conclusion which is robust to using lower thresholds to define small firms). It is conceivable that non-take-up is more prevalent among small firms, but it seems unlikely that this is the whole explanation.

One method to check for parallel trends, a crucial assumption behind the d-i-d estimator, is to use placebo periods. By using data on prior periods, the d-i-d regressions can be re-estimated by studying the years during which there were no payroll policy changes. If the placebo estimators are statistically significant there is a risk that the estimated d-i-d coefficients are biased. As a check for robustness, a large number of different placebo regressions for entry and exit have been estimated for the age groups in Table 3. Placebo reforms for the years 2004, 2005 and 2006 are presented in Table 4. For blue-collar workers in the regressions with 19–25-year-olds and 21–25-year-olds as treatment groups there is evidence of ‘pre-treatment’, which casts some doubt on the regressions for these groups in Tables 2 and 3. However, there is much less or no such evidence in the placebo regressions using those aged 22–25 and 25 as the treatment. In an attempt to make the treatment and controls more similar, I experimented with using various restrictions on weekly hours – at least 10 hours, 20 hours and 35 hours – in the regressions with 19–25-year-olds and 21–25-year-olds as controls, but this had little effect on the placebo estimates. Based on the exercises in Table 4, I conclude that my estimates should be viewed as *at best* indicating modest effects on entry and exit following the 2007 reform.

6.2 Payroll tax contributions, managerial pay and profits

In this section, the after-period is the entire period of 2007–2010, and it is not prolonged successively by one year, since profits typically contain a great deal of idiosyncratic year-to-year variation. As in the previous section, only the estimate of the reform effect is shown. There are two model specifications, one without and one with firm fixed effects.

Table 5 displays results for payroll tax contributions, managerial pay and profits. To begin with, it is necessary to demonstrate that payroll tax contributions actually decreased more in those firms employing relatively many young workers before the reform in 2007. This procedure serves as a check on the quality of the data and also indicates whether non-take-up is a serious problem. To this end, the first columns in Table 5 show regressions with the firm’s payroll tax contributions including pension costs, measured as a percentage of total wage and pension costs, as the dependent variable. The evidence provided in the table is clear: The larger the share of young workers in the firm before the reform, the smaller the payroll tax contributions in relation to the wage bill after the reform. The estimates are robust across specifications and significant throughout, suggesting that a firm with a one percentage point larger share of 19–25-year-olds than another firm experienced a decrease in the share of contributions by 0.038 to 0.045 percentage points, relative to the other firm.

The regressions for the share of managerial pay, including performance pay, in the wage bill do not indicate that salaries of managers increased following the reform, over and above any increases of wages for non-managerial staff. The measure of managerial pay is crude, though, since the number of employees among managers and other staff is not taken into account.

Turning to the effects on profit margins in the final columns of Table 5, it should be recalled that two different measures are used, namely before and after financial items. The two variants of profit margins yield similar results. The estimates are always positive, but only borderline significant in one case with fixed effects. It is conceivable that firm-specific factors, such as location, are important for profit margins in retail and these factors may also be related to the intensity of using young workers. With fixed effects the magnitudes of the estimates are reduced, from 0.050 to 0.029 and 0.036, depending on specification. Accordingly, a firm with a one percentage point higher pre-reform share of young workers than that in another firm increased its profit margin by an additional 0.03–0.05 percentage points after the reform in 2007. Unlike Korkeamäki (2011), I thus find some evidence of increasing profits following the payroll tax reform, although the estimates are less precise in the fixed-effects specification.

A concern with the approach in Table 5, which captures an intention to treat rather than actual treatment, is that it may bias the effect on profits if firms adjust their intensity of using young workers after the reform. However, since the reforms seem to have had little effect on employment, the estimates probably represent a good approximation. The results are also consistent with previous results regarding worker outcomes in this paper, showing modest effects. In the absence of behavioural effects, it can be argued that the estimated reform effect on profits should be equivalent to the share of the wage bill in total revenue.[17] Examining the data and adjusting the wage bill with the average intensity of using young workers, this share turns out to be 0.160 x 0.221 = 0.035, which is within the range of estimates presented in Table 5.[18]

The two placebo tests in Table 6 assume that reforms were undertaken in 2003 and 2004, respectively. All estimates are very close to zero, indicating no spurious effects on profit margins whatsoever.

A potential problem with the subset of firms used in the examination of profits is that it includes relatively large firms on average, although it accounts for over 40 per cent of all observations of workers, implying that the results may not be representative for smaller firms. To shed light on this issue, I split the sample into matched and non-matched firms and re-ran the benchmark regressions in Table 2 for the period 2004–2010 (the results are unreported for brevity). There were no significant differences in the reform effects for matched and non-matched firms, with one exception: for blue-collar workers the increase in wages was smaller and close to zero in the non-matched sample. These results speak against the possibility that the reform effect on profits in the non-matched firms is much different than the one that was found for matched firms. However, when I ran separate regressions for firms operating in the regional support areas, as a robustness check, the estimated effects on profits turned out to be negative, which is difficult to explain. This result along with the fact that I only cover a subset of firms in the industry cautions against far-reaching conclusions as to the wider applicability of my findings regarding profits.

7 Conclusions

This paper has exploited a Swedish payroll tax reform targeted at young workers, implemented in two stages in 2007 and 2009. The analysis considers effects on worker outcomes as well as firm performance in the retail industry.

Using a d-i-d approach, with slightly older workers as the control, the results on worker outcomes indicate that – on average – the effects on entry, exit, hours and wages have been small, both in absolute magnitudes and in relation to the sizeable cuts in

[17] See Draca et al. (2011), who could not reject this hypothesis in their analysis of the introduction of the National Minimum Wage in the UK.

[18] The share of the wage bill being within the range of estimates may be regarded as a permissive criterion, since the confidence intervals are rather large.

taxes. My results on worker outcomes are in accordance with much of the previous literature on the employment effects of changes in payroll taxes, which mostly has concerned itself with reforms of flat rate or regionally differentiated payroll taxes. The findings are also similar to those obtained by Egebark and Kaunitz (2013), who examine the effects of the same payroll tax reforms as in this paper, but for the entire labour market. It is worth noting that they use a different evaluation method, combining d-i-d with matching, and analyse effects on net employment instead of gross flows. Egebark and Kaunitz (2013) perform a cost-benefit analysis of the 2007 reform and estimate that each new job in the age group 19–25 is associated with a cost of SEK 0.9–1.5 million (USD 140,000–230,000). Their conclusion that reducing payroll taxes is a costly means of improving employment prospects for the young is likely to hold also for the industry I analyse.

For workers bound by minimum wages, the estimated effects of the payroll tax reforms in my study suggest larger, but still modest, effects on the probability of entry. This result is consistent with the view that high minimum wages represent a serious obstacle to labour market entry among the young. Since cutting payroll taxes for the youngest and the lowest-paid is also less expensive than cuts for other young workers, this result could be helpful for improving the design of payroll tax reforms. However, the findings may at least partly be explained by higher cyclical sensitivity among the youngest and among minimum wage workers. It should also be kept in mind that the results derive from a particular industry, with high and binding minimum wages, so any policy implications from this study do not necessarily carry over to industries with different characteristics in this respect.

This study is one of the first to examine the effect on firm performance from payroll tax cuts. The analysis is based on comparing firms with marginally larger pre-reform shares of young workers with performance after the reform. There is some evidence of increasing profit margins following the reform, for the subset of firms analysed. While the subset accounts for a substantial part of all observations of workers in the industry, it covers relatively large firms on average, so the conclusions do not necessarily hold for other firms in retail or for other industries, especially those with a less intensive use of young workers. It should also be noted that I do not explicitly consider the effects of the payroll tax reforms on the entry or exit of firms, due to data limitations. This is unlikely to be a major problem in the short run, but in the long run profits could be eroded if more firms enter the industry or survive longer. Despite these shortcomings, the findings regarding profits are commensurate with the absence of large behavioural effects of the payroll tax cuts demonstrated in this study. This includes absence of a large general wage increase, affecting not only treated workers, which is difficult to rule out in the d-i-d analysis of relative wage increases for the treated in the first part of the paper.

My analysis has considered several outcomes for workers and firms but is still far from complete. Non-wage personnel costs, like training, may have been affected by the reforms and these costs have not been accounted for. Retail prices, on which I have no suitable information, may also have adjusted in response to the payroll tax cuts.

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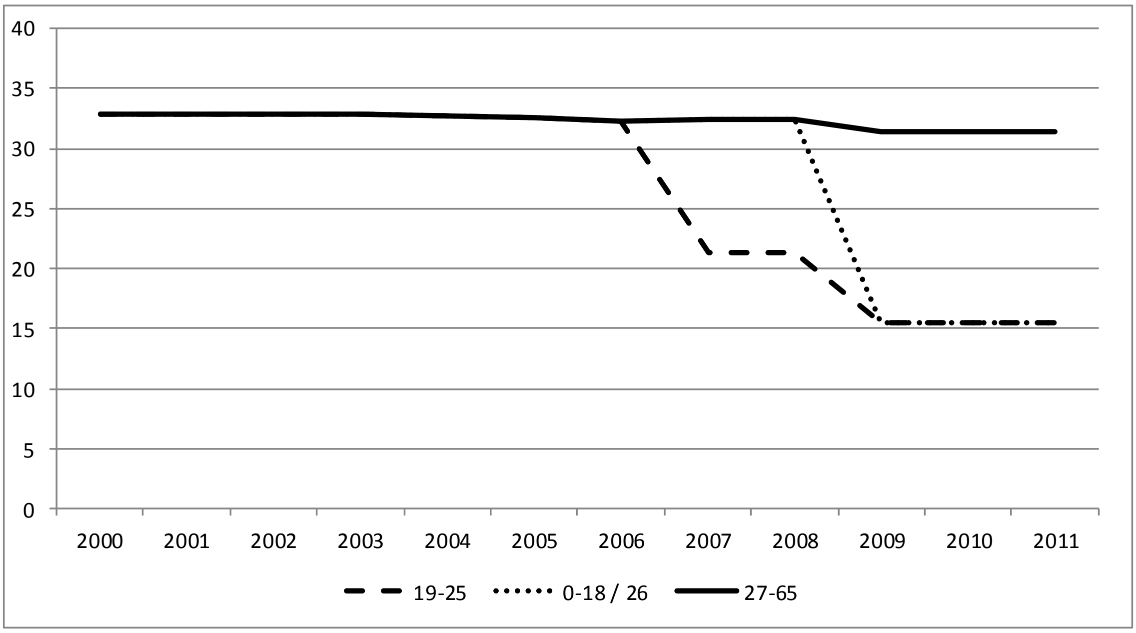
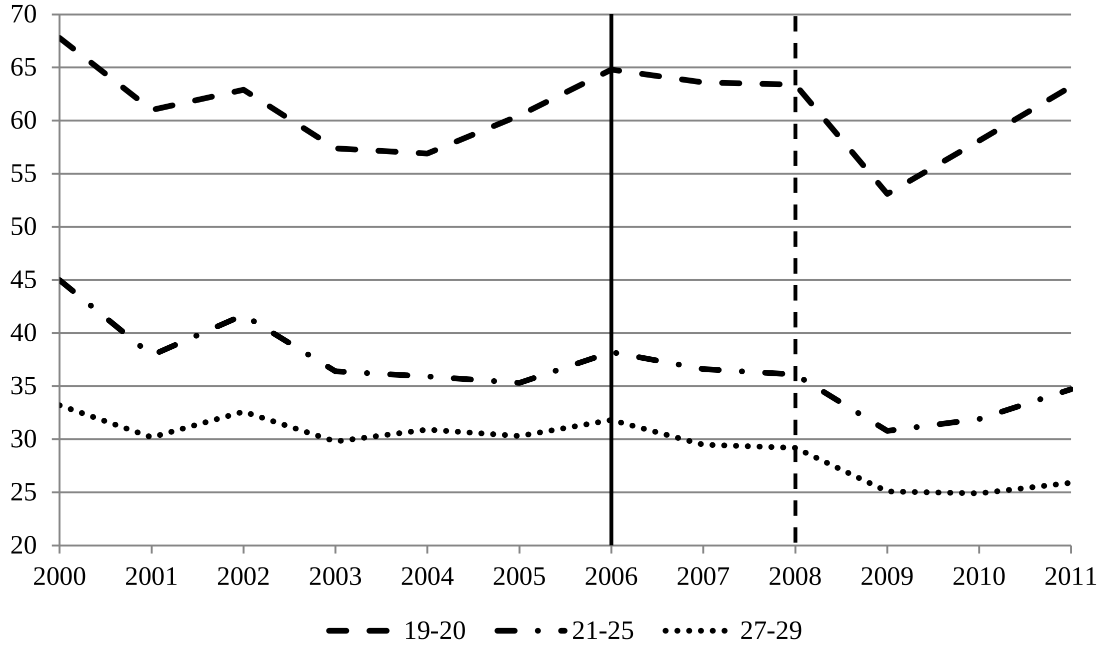


Figure 1 Payroll tax rates, by age group, 2000–2011. Per cent

Notes: Regional reductions, implemented in 2002 in mainly the northern parts of Sweden, are not accounted for.
Source: National Mediation Office.

a) Blue-collar workers



b) White-collar workers

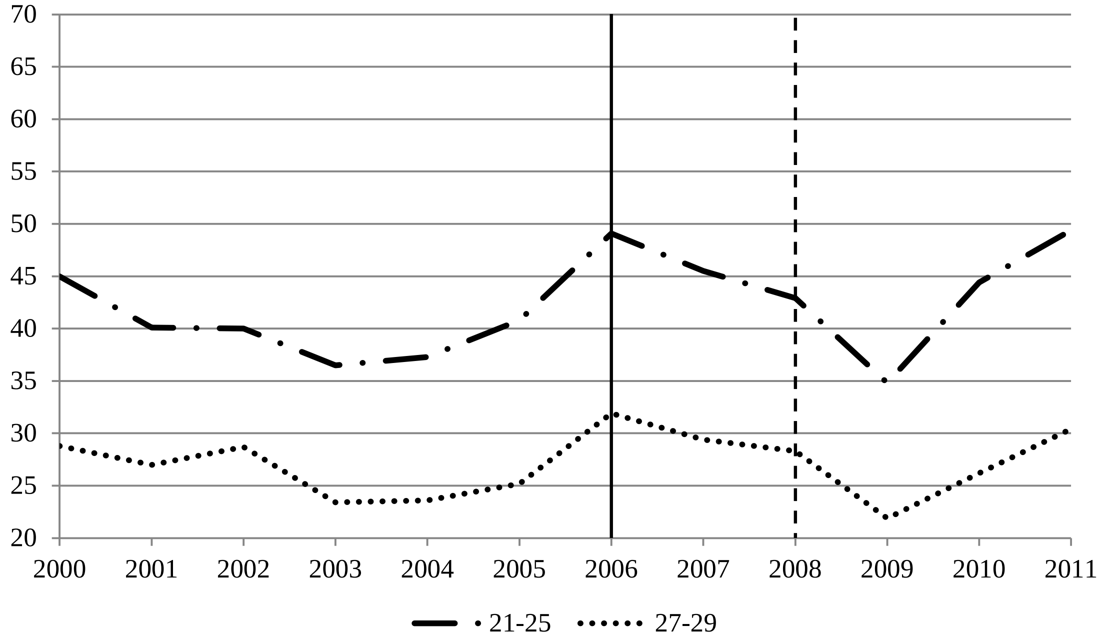
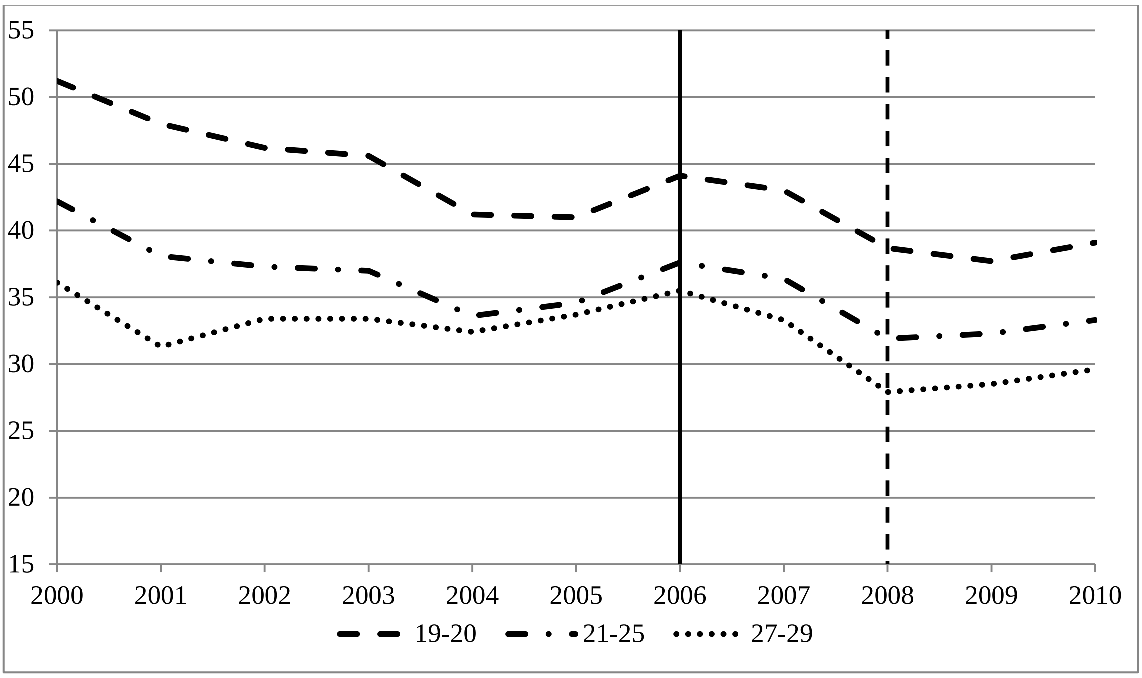


Figure 2 Accession rates in the retail industry, by age group, 2000–2011. Per cent

Notes: Accession rates refer to year *t*. The year before a payroll tax reform is indicated by a vertical line.
Source: Own calculations.

a) Blue-collar workers



b) White-collar workers

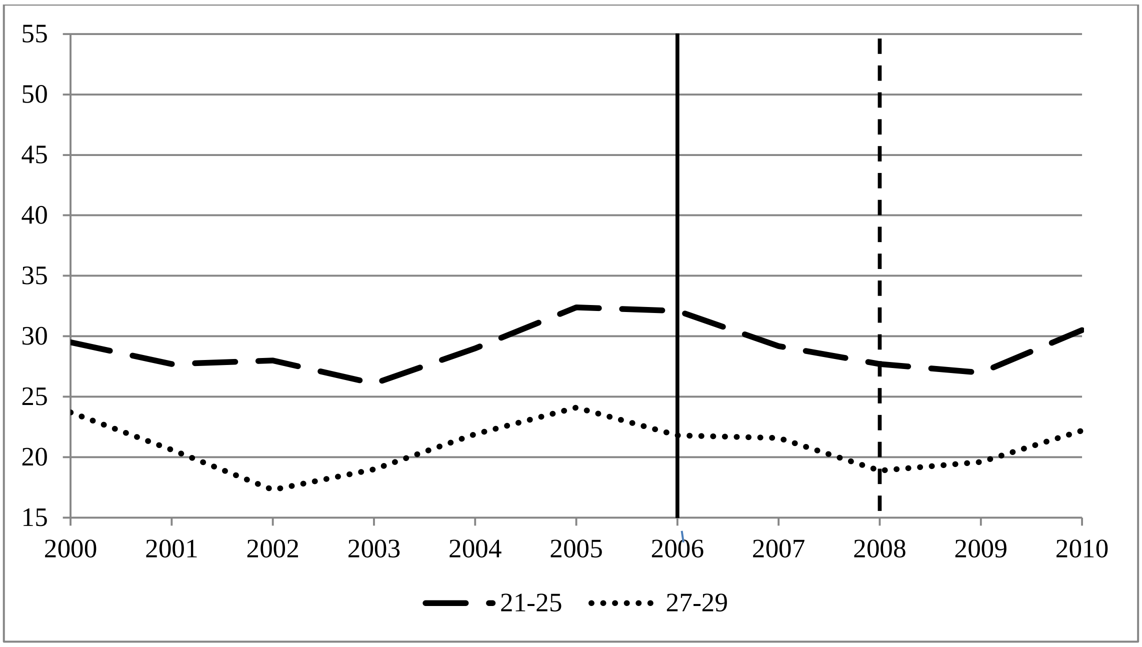
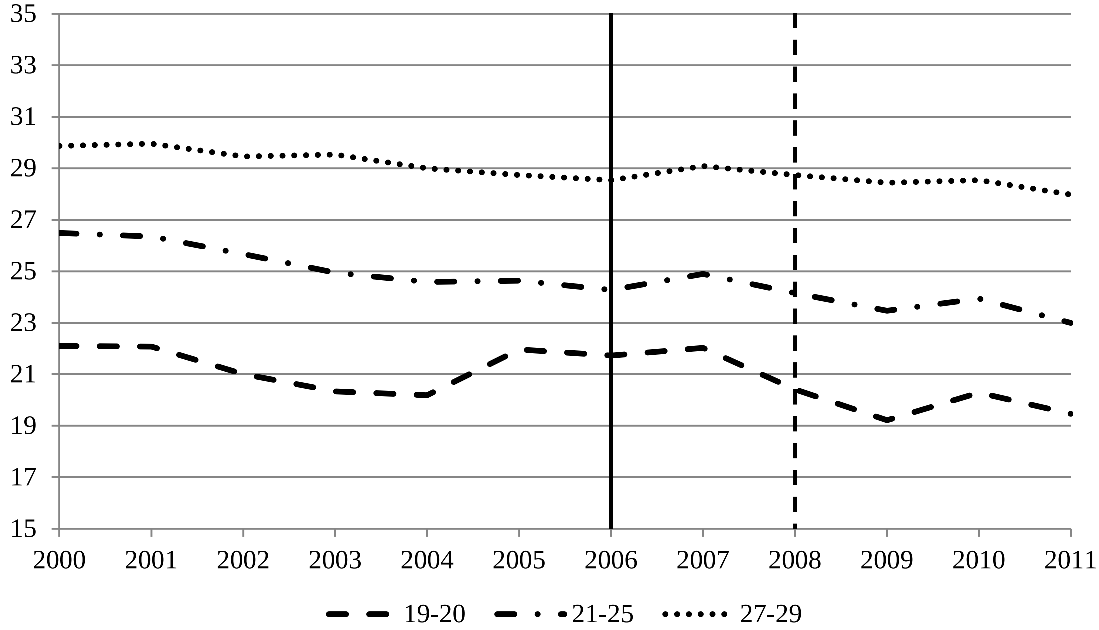


Figure 3 Separation rates in the retail industry, by age group, 2000–2010. Per cent

Notes: Separation rates refer to year t+1. The year before a payroll tax reform is indicated by a vertical line.
Source: Own calculations.

a) Blue-collar workers



b) White-collar workers

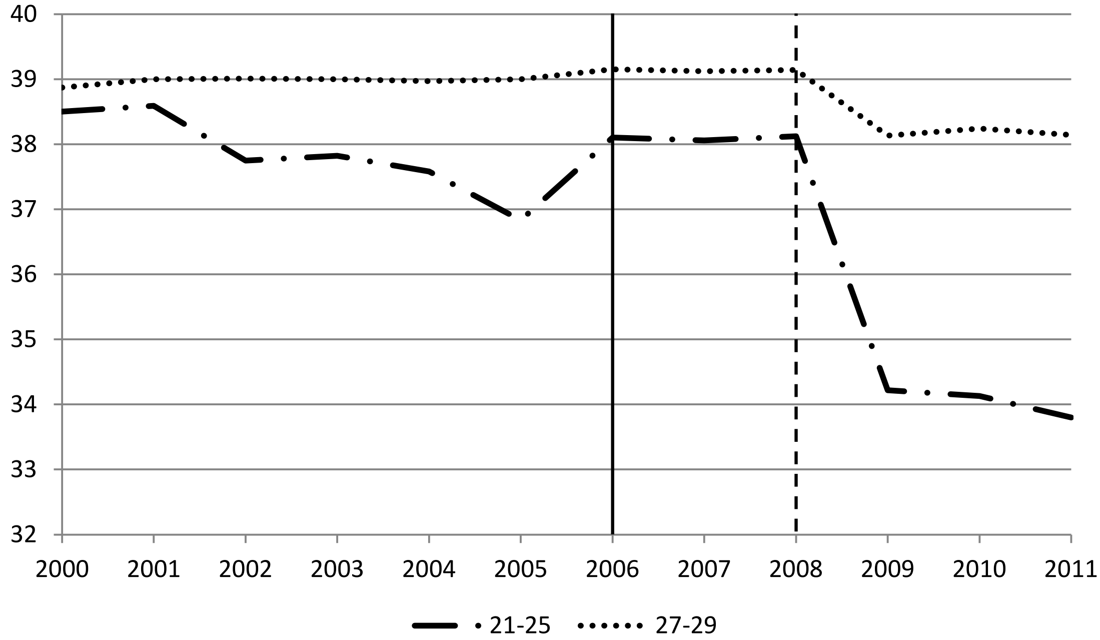
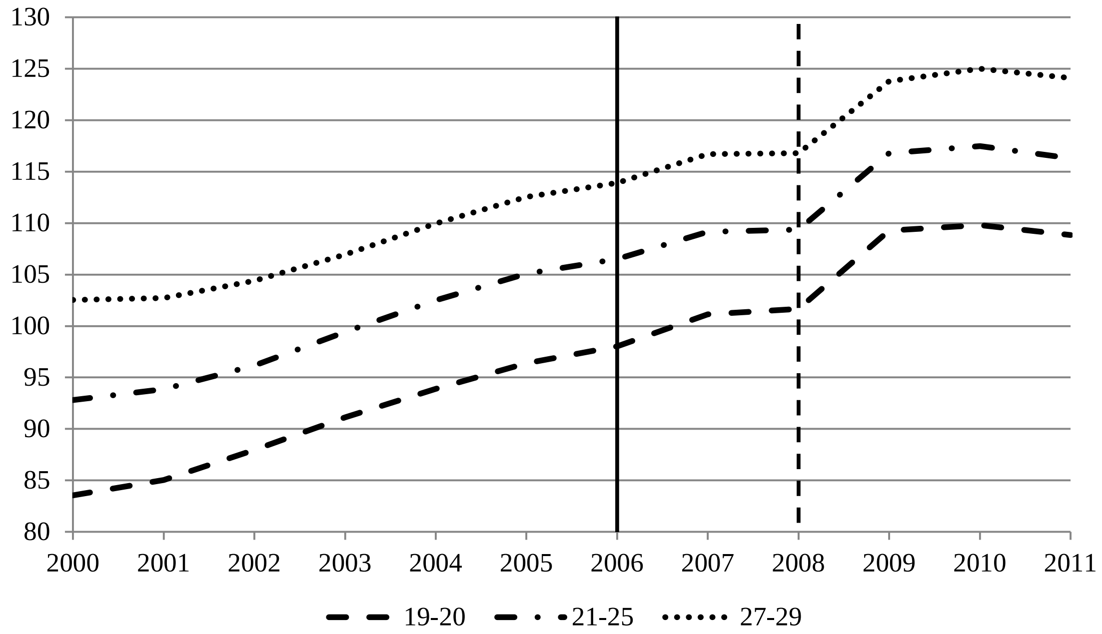


Figure 4 Weekly hours in the retail industry, by age group, 2000–2011

Notes: The year before a payroll tax reform is indicated by a vertical line.
Source: Own calculations.

a) Blue-collar workers



b) White-collar workers

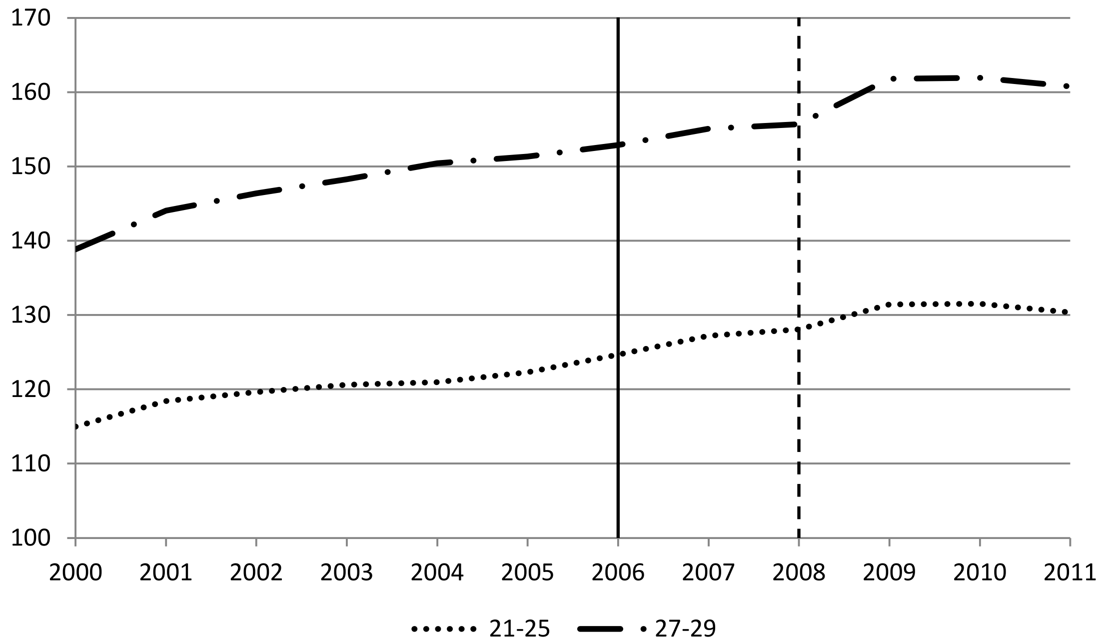
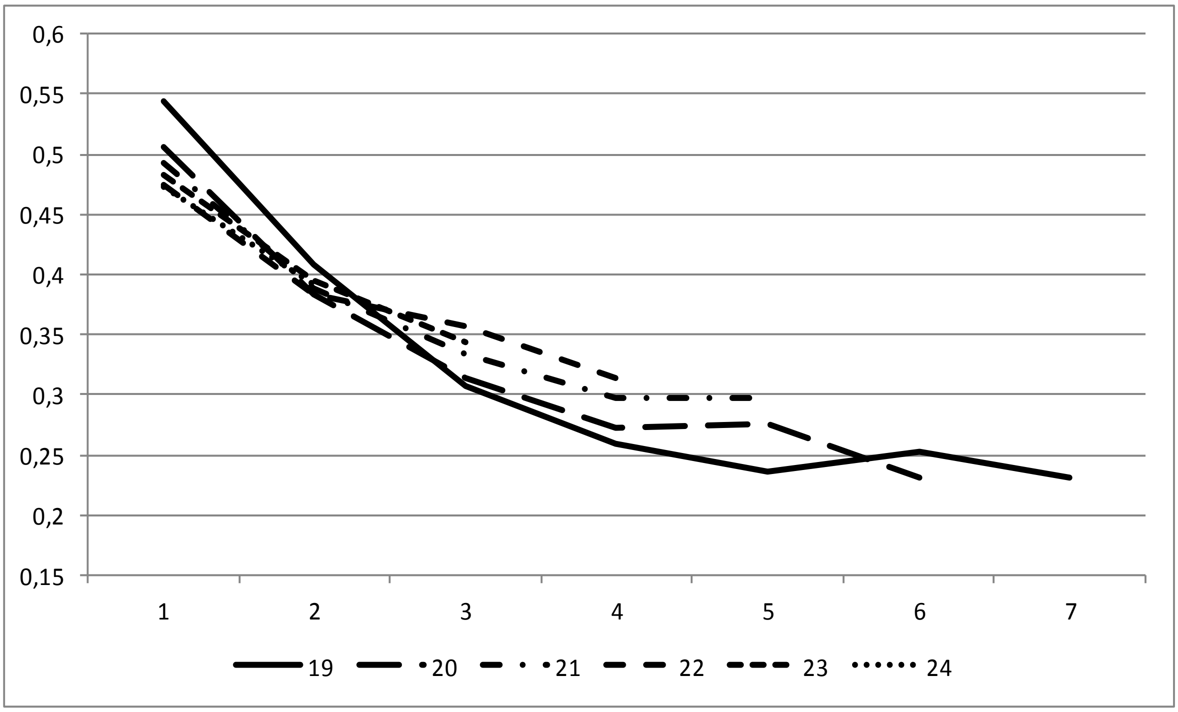


Figure 5 Real hourly wage in the retail industry, by age group, 2000–2011. SEK

Notes: 2011 prices. The year before a payroll tax reform is indicated by a vertical line.
Source: Own calculations.

a) Blue-collar workers



b) White-collar workers

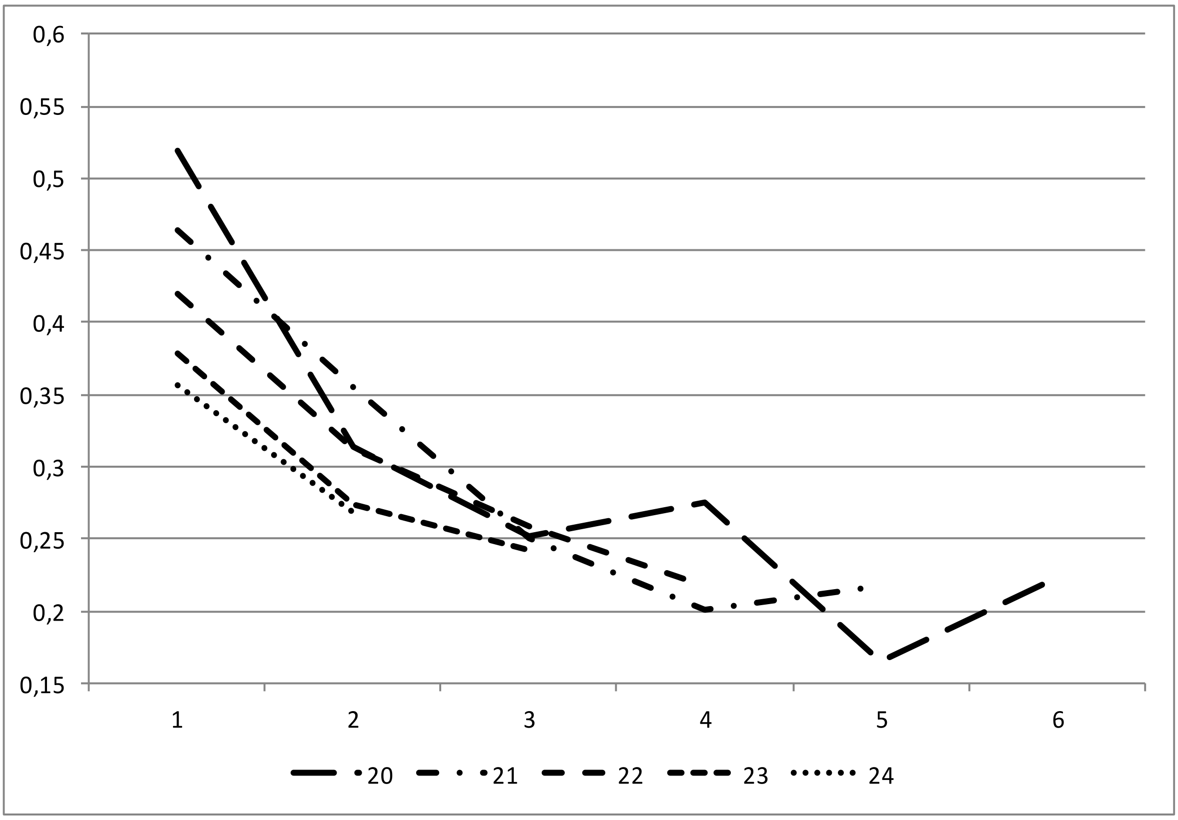


Figure 6 Separation rates by age at hiring, conditional on tenure. 1998–2005
Source: Own calculations.

a) Blue-collar workers

b) White-collar workers

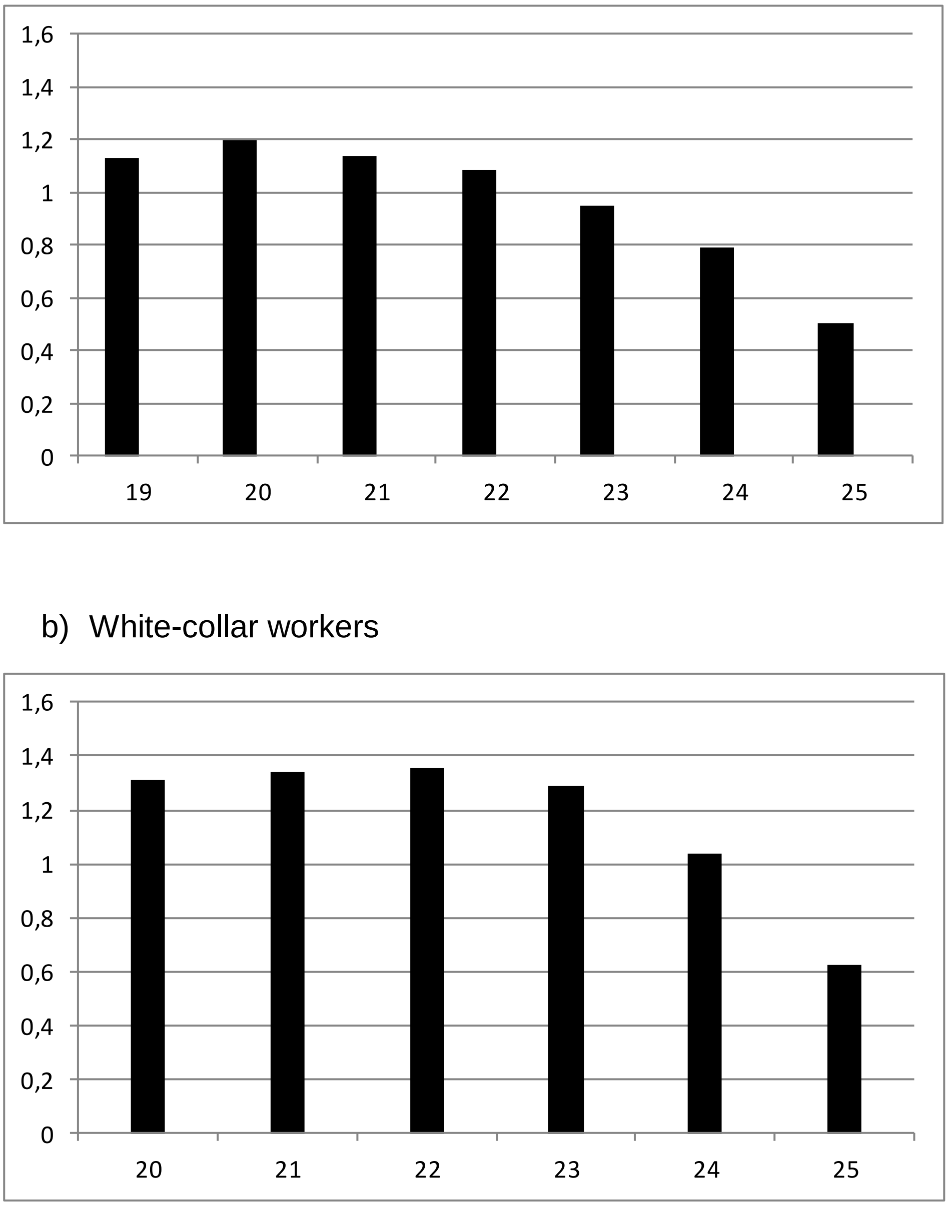


Figure 7 Expected discounted value of payroll tax cut, by age

Notes: See text for details about the calculations.
Source: Own calculations.

Table 1 Descriptive statistics

a) Individual-level data, 2004–2011

Variable	Blue-collars		White-collars	
	19–25	27–29	19–25	27–29
Accession rate	0.422	0.281	0.473	0.271
Separation rate	0.385	0.340	0.358	0.250
Weekly hours	23.1	28.6	35.9	38.7
Real hourly wage (SEK)	100.9	110.5	121.8	155.7
Age	21.9	27.9	23.1	28.1
Male	0.331	0.371	0.421	0.464
Sales work	0.832	0.822	0.179	0.085
Salaried	0.154	0.342	0.809	0.973
Close to minimum wage: At most 5 % above	0.657	0.467	–	–
Close to minimum wage: At most 1 % above	0.387	0.202	–	–
Metropolitan counties	0.601	0.598	0.623	0.645
Forest counties	0.108	0.116	0.111	0.074
Other counties	0.291	0.286	0.266	0.281
Year 2004	0.094	0.092	0.094	0.103
Year 2005	0.107	0.104	0.102	0.109
Year 2006	0.119	0.117	0.113	0.116
Year 2007	0.124	0.119	0.118	0.117
Year 2008	0.135	0.130	0.133	0.134
Year 2009	0.136	0.139	0.132	0.140
Year 2010	0.138	0.146	0.140	0.136
Year 2011	0.147	0.152	0.166	0.145
No. of employees in firm	957.9	1098.1	819.7	808.6
No. of obs.	213,536	52,168	19,688	26,896

b) Firm-level data, 2004–2010

Variable	Matched firms	Non-matched firms
Profit margin, before financial items	0.032	–
Profit margin, after financial items	0.039	–
Payroll tax contributions relative to wage bill	0.289	–
Managerial pay relative to wage bill	0.037	–
Pre-reform share of workers aged 19–25	0.221	0.242
Share of blue-collar workers	0.491	0.727
Metropolitan counties	0.681	0.547
Forest counties	0.055	0.120
Other counties	0.263	0.334
Year 2004	0.120	0.167
Year 2005	0.177	0.177
Year 2006	0.185	0.170
Year 2007	0.027	0.136
Year 2008	0.159	0.125
Year 2009	0.178	0.116
Year 2010	0.154	0.110
No. of employees	144.0	21.4
No. of firms	354	2,996
No. of obs.	1,229	11,308

Table 2 Effects on employment, hours and wages. Treated age group: 21–25. Control age group: 27–29

	P(Entry)	P(Exit)	ln H^{new}	ln H^{inc}	ln W^{new}	ln W^{inc}
Blue-collars						
2004–2007	0.017*	0.002	0.001	0.009	0.003	0.001
	(1.80)	(0.28)	(0.04)	(0.48)	(0.94)	(0.35)
	[89,989]	[87,273]	[31,081]	[57,816]	[28,375]	[53,130]
2004–2008	0.015*	0.008	–0.015	–0.012	0.004	0.001
	(1.82)	(1.15)	(0.46)	(0.68)	(1.32)	(0.29)
	[116,579]	[112,948]	[40,112]	[75,076]	[36,772]	[69,200]
2004–2009	0.010	0.012**	–0.031	–0.028	0.006**	0.003
	(1.32)	(2.04)	(0.94)	(1.59)	(2.29)	(1.36)
	[144,209]	[139,948]	[48,131]	[94,392]	[44,192]	[87,252]
2004–2010	0.011	0.014**	–0.026	–0.030	0.007***	0.004*
	(1.57)	(2.26)	(0.85)	(1.64)	(2.65)	(1.71)
	[173,104]	[168,092]	[56,698]	[114,432]	[51,943]	[105,925]
2004–2011	0.015**	0.015***	–0.029	–0.034*	0.006**	0.004*
	(2.23)	(2.59)	(1.04)	(1.79)	(2.51)	(1.69)
	[204,045]	[197,846]	[66,606]	[135,067]	[61,171]	[125,222]
White-collars						
2004–2007	0.015	0.031*	0.010	0.023**	0.008	0.018**
	(0.73)	(1.82)	(0.34)	(2.16)	(0.73)	(2.31)
	[19,334]	[19,781]	[6,453]	[12,716]	[6,282]	[12,604]
2004–2008	0.009	0.019	0.002	0.024***	0.005	0.016**
	(0.57)	(1.42)	(0.08)	(2.73)	(0.50)	(2.01)
	[25,245]	[25,424]	[8,452]	[16,593]	[8,239]	[16,448]
2004–2009	–0.006	0.018	–0.033	–0.006	0.005	0.019**
	(0.40)	(1.46)	(0.90)	(0.32)	(0.46)	(2.25)
	[31,395]	[32,069]	[10,093]	[21,049]	[9,857]	[20,872]
2004–2010	–0.000	0.013	–0.025	–0.016	0.012	0.021**
	(0.02)	(1.04)	(0.62)	(0.65)	(1.16)	(2.21)
	[37,508]	[38,059]	[12,123]	[25,074]	[11,864]	[24,876]
2004–2011	0.004	0.008	–0.056	–0.017	0.014	0.023**
	(0.29)	(0.72)	(1.16)	(0.67)	(1.28)	(2.40)
	[44,309]	[44,278]	[14,737]	[29,227]	[14,327]	[29,005]

Notes: Only the estimated reform effects in the OLS regressions are shown. In the regressions for exits, the estimation periods are 2003–2006, 2003–2007 and so on until 2003–2010. The regressions include dummies for the treated group, the post period, gender, age, occupation, salaried position, region and year. For hours and wages, different regressions are performed for new hires and incumbent workers. Absolute, robust t-statistics, clustered at firm level, within parentheses. Number of observations within brackets. * denotes significance at 10%, ** significance at 5%, *** significance at 1%.

Table 3 Effects on employment, 2004–07. Various age groups and subgroups

	All		Bound by minimum wage		In small firm	
	P(Entry)	P(Exit)	P(Entry)	P(Exit)	P(Entry)	P(Exit)
T: 21–25, C:27–29						
Blue-collars						
	0.017*	0.002	0.033**	0.028**	0.015	0.019
	(1.80)	(0.28)	(2.33)	(2.39)	(0.88)	(1.12)
	[89,989]	[87,273]	[43,377]	[39,129]	[19,381]	[19,785]
White-collars						
	0.015	0.031*			0.013	0.065*
	(0.73)	(1.82)			(0.35)	(1.91)
	[19,334]	[19,781]			[4,187]	[4,624]
T: 19–25, C:26–29						
Blue-collars						
	0.022**	–0.002	0.030***	0.017*	0.006	0.001
	(2.57)	(0.32)	(2.61)	(1.65)	(0.38)	(0.07)
	[127,342]	[124,131]	[69,330]	[63,612]	[28,938]	[29,651]
White-collars						
	0.016	0.037**			0.004	0.064**
	(0.90)	(2.32)			(0.11)	(2.02)
	[23,518]	[23,934]			[5,101]	[5,560]
T: 22–25, C:26–28						
Blue-collars						
	0.012	0.002	0.028**	0.019	–0.004	–0.012
	(1.43)	(0.29)	(2.27)	(1.63)	(0.25)	(0.67)
	[78,819]	[76,067]	[36,258]	[32,403]	[16,471]	[16,776]
White-collars						
	0.014	0.021			0.010	0.035
	(0.74)	(1.15)			(0.24)	(0.94)
	[17,359]	[17,656]			[3,739]	[4,103]
T: 25, C:26						
Blue-collars						
	–0.001	–0.008	–0.009	–0.004	–0.012	–0.094***
	(0.07)	(0.60)	(0.45)	(0.18)	(0.36)	(2.77)
	[21,363]	[20,562]	[9,309]	[8,184]	[4,338]	[4,396]
White-collars						
	0.011	0.007				
	(0.38)	(0.26)				
	[5,596]	[5,655]				

Notes: T denotes treatment and C control. There were too few observations for T:25, C:26, white-collars in small firms. See also notes to Table 2.

Table 4 Placebo tests. Effects on employment, t–3 – t

	Placebo reform, t=2004		Placebo reform, t=2005		Placebo reform, t=2006	
	P(Entry)	P(Exit)	P(Entry)	P(Exit)	P(Entry)	P(Exit)
T: 21–25, C:27–29						
Blue-collars						
	–0.030***	–0.023**	–0.024**	–0.037***	0.009	–0.021**
	(2.94)	(2.11)	(2.50)	(4.42)	(0.96)	(2.26)
	[73,870]	[71,093]	[78,884]	[74,506]	[84,145]	[80,428]
White-collars						
	0.001	–0.010	0.016	–0.032**	0.031	–0.014
	(0.07)	(0.66)	(0.92)	(2.08)	(1.58)	(0.75)
	[18,517]	[19,205]	[18,537]	[19,419]	[18,789]	[19,598]
T: 19–25, C:26–29						
Blue-collars						
	–0.020**	–0.017*	0.000	–0.040***	0.020**	–0.019**
	(2.19)	(1.74)	(0.00)	(5.32)	(2.02)	(2.22)
	[107,466]	[104,914]	[113,175]	[108,330]	[119,565]	[115,417]
White-collars						
	–0.009	–0.011	0.014	–0.031*	0.036**	–0.019
	(0.50)	(0.80)	(0.86)	(1.88)	(2.02)	(1.03)
	[22,121]	[22,993]	[22,208]	[23,231]	[22,754]	[23,508]
T: 22–25, C:26–28						
Blue-collars						
	–0.015	–0.001	–0.009	–0.036***	–0.000	–0.017
	(1.44)	(0.09)	(1.00)	(4.14)	(0.02)	(1.64)
	[63,028]	[60,554]	[68,000]	[63,589]	[73,259]	[69,422]
White-collars						
	–0.014	–0.009	0.026	–0.042**	0.034*	–0.015
	(0.75)	(0.61)	(1.52)	(2.50)	(1.71)	(0.84)
	[16,320]	[17,009]	[16,381]	[17,161]	[16,776]	[17,358]
T: 25, C:26						
Blue-collars						
	0.013	0.005	0.013	–0.018	–0.002	0.014
	(0.77)	(0.36)	(0.90)	(1.06)	(0.17)	(0.93)
	[16,543]	[15,854]	[18,151]	[16,687]	[19,818]	[18,552]
White-collars						
	–0.029	0.006	0.033	–0.050*	0.028	–0.024
	(0.94)	(0.21)	(1.24)	(1.77)	(1.01)	(0.84)
	[5,071]	[5,248]	[5,200]	[5,293]	[5,420]	[5,478]

Notes: See notes to Table 2 and 3.

Table 5 Effects on firms' payroll tax contributions, management salaries and profits, 2004–2010

Payroll taxes		Management pay		Profit margin, before financial items		Profit margin, after financial items	
w/o FE	FE	w/o FE	FE	w/o FE	FE	w/o FE	FE
–0.038***	–0.045***	0.012	–0.013	0.050**	0.036*	0.050**	0.029
(4.23)	(6.34)	(0.98)	(1.17)	(2.38)	(1.72)	(2.29)	(1.63)
[1,221]	[787]	[1,221]	[787]	[1,229]	[792]	[1,225]	[789]
{354}	{196}	{354}	{196}	{354}	{196}	{354}	{196}

Notes: Only the estimated reform effects, defined as the pre-reform percentage of 19–25-year olds in the firm interacted with a dummy for the post-reform period, are shown. The models also include a dummy for the post-reform period, the pre-reform share of 19–25-year olds in the firm, the share of blue-collar workers, and dummies for year and region. Payroll tax contributions include pension costs and are measured as a percentage of total wage and pension costs. Management pay includes performance pay and is measured as a share of pay of all employees including performance pay. Profit margin is profits, before taxes and non-recurring items, divided by sales. The end of the financial year corresponds to the year used for other variables in the regressions. The models are estimated without and with firm fixed effects (FE). The time-invariant pre-reform share of 19–25-year olds is excluded in the fixed-effects regressions. Absolute, robust t-statistics, clustered at firm level, within parentheses. Number of observations within square brackets, number of firms within curly brackets. See also notes to Table 2.

Table 6 Placebo tests. Effects on firms' profits

a) Placebo reform 2003, 2000–2006

Profit margin, before financial items		Profit margin, after financial items	
w/o FE	FE	w/o FE	FE
–0.014	–0.013	–0.008	–0.009
(0.52)	(1.07)	(0.30)	(0.69)
[1,038]	[734]	[1,035]	[734]
{269}	{163}	{269}	{163}

b) Placebo reform 2004, 2001–2006

Profit margin, before financial items		Profit margin, after financial items	
w/o FE	FE	w/o FE	FE
–0.002	–0.008	0.000	–0.001
(0.06)	(0.66)	(0.01)	(0.05)
[943]	[712]	[940]	[712]
{273}	{182}	{273}	{182}

Notes: See notes to Table 5.