Effects of Payroll Tax Cuts for Young Workers: Evidence from Swedish Retail*

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Preliminary, comments welcome

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Abstract

This paper analyses the effects of two large payroll tax cuts for young workers, implemented in two reforms in 2007 and 2009. In general, the estimated effects on job accessions, separations, hours and wages in the retail industry are small. However, for workers bound by minimum wages the estimated effects suggest substantially larger 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. Considering that most evaluations of payroll tax reductions in the literature indicate that the reforms have been costly per job created, these results may be helpful for improving the design of such reforms.

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1. Introduction

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 and wages 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, contributing to demand for labour being more sensitive to cuts in wage costs for young workers than in other industries. 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 and wage outcomes for young workers. 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.

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, 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. With higher wages, labour supply should respond positively and this could increase employment under certain circumstances. For example, to the extent that marginal groups are caught in 'poverty traps' in the social assistance system, a higher wage induced by a payroll tax reduction may create new jobs.[1]

The reasoning so far applies to the labour market in general. It is, however, far from obvious that the arguments apply with equal force to the labour market of low-skilled workers or in low-wage sectors (Nickell and Bell, 1997; Pissarides, 1998). Union bargaining as well as statutory or collectively agreed minimum wages may introduce downward wage rigidity even in the longer term. In addition, it seems plausible that selective payroll tax reductions generate less upward pressure on wages than across-the-board cuts. Consequently, the OECD (2003) has argued that reductions of payroll taxes should be targeted to marginal groups, such as the low-paid, young workers, the work disabled and the long-term unemployed. With binding minimum wages, employment may be positively affected by a payroll tax cut to the extent that minimum wages are not increased by as much in percentage terms as the tax is reduced. With collectively agreed minimum wages, as in Sweden, it remains an open question how these rates evolve in response to changes in payroll taxes.

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. 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). Much of the evidence based on general reductions of 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

[1] Skedinger (2010) demonstrates the existence of poverty traps for single-parent households with children receiving social assistance above the level of minimum wages in Sweden.
[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.

employment effects of the substantial reduction of payroll taxes in France – up to 15 percentage points – for workers on or close to the 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. In the analytical framework of Kramarz and Philippon (2001), it is difficult to disentangle the effects on employment from payroll taxes from those of minimum wages. As payroll taxes were reduced, minimum wages increased.

The most closely related study to the present one is Egebark and Kaunitz (2012), who examine the effects of the 2007 reform of payroll taxes in Sweden. They find evidence of a small increase in employment, but little impact on wages. Unlike me, they are able to study heterogeneous effects with respect to country of birth and education (but find that neither matters for employment and wage outcomes). My analysis differs from the one in Egebark and Kaunitz (2012) in several additional ways: only those employed in a specific industry are included, rather than all employees; the effects of both reforms, in 2007 and 2009, are examined; the analysis differentiates between entry into and exit from employment and also considers effects on hours per worker; and an analysis of heterogeneity in treatment effects for workers bound by minimum wages is undertaken.

The paper is organized 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 discussed in Section 3. Section 4 deals with 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 concludes the paper.

2. The Swedish payroll tax system and the reforms of 2007 and 2009

Swedish payroll taxes 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.[3] 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 18. 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).[4] The government's motives for abolishing the lower age limit for eligibility was

[3] Strictly speaking, the cost to the employer in the legally mandated system includes both social security contributions and a payroll tax. In this paper, 'payroll taxes' will be used to mean the sum of these two components. Data on agreed fees often include the special tax on pension costs and this convention is followed here as well. Thus 'payroll fees' refer to the sum of the two components in the collective agreements.

[4] The original intention of the government for the second reform, launched in a bill in October 2007, had been to reduce payroll taxes in specific service sectors susceptible to home production and underground work, like restaurants, car repair shops, laundries and hairdressers. The proposal met with objections from the EU Commission which approved of it only under the provision that the reduction apply to small and medium-sized

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 payroll tax. Earnings above certain thresholds (varying depending on component and related to ‘basic amounts’) do not generate additional benefits, but since these thresholds rarely apply to young workers my conclusion is that most of the payroll taxes are linked to benefits for this particular group.[5]

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 (32.42–10.21)/2 = 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 (31.42–10.21)/ 0.75 = 15.9 percentage points. Since 2009 the formula, and the reduction in

firms only. As the government came to conclusion that this provision would reduce the impact of the reduction on employment, distort competition and contribute to red tape costs, the bill was withdrawn on 27 March 2008. At the same time, the government announced the main ideas of an alternative proposal, which then materialized in the bill of 25 September in the same year.

[5] Du Rietz (2008) argues that, on average and considering the benefit ceilings, about 50 per cent of Swedish payroll taxes constitutes a pure tax.

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.[6] 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 persons up to age 26.

Collectively agreed payroll fees for blue- and white-collar workers in the private sector, including the special tax for pension costs, are presented in Figure 2. Apart from the special tax, all components are benefit-linked. The rates, which are consistently higher for white-collars, have evolved quite differently during the period of payroll tax reform. In 2008, rates for white-collars were substantially reduced, from 17.1 to 10.7 per cent, as, inter alia, fees for old-age and family pension were temporarily cut (National Mediation Office, 2012). Reduced sickness absence among both white- and blue-collar workers has contributed to lower payroll fees for the two categories during later years. The data in Figure 2 are based on average rates across all age groups. Since some of the components in the payroll fees for white-collar workers increase progressively with earnings, the rates for young workers are likely to be lower than the average.

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[7] – is that the payroll tax cut applied to persons aged 19–25 when it was first implemented on 1 July 2007. That is, persons aged 25, but not the 18-year-olds, were in fact part of the age group subject to the cut. A similar formulation was

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

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 half 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. Misconceptions in this respect could also have been generated by the title of the bill to the Swedish Parliament (Regeringens proposition 2006/07:84), which simply says: "Reduction of payroll taxes for persons who have turned 18 but not 25 years of age". The annual reports of the National Mediation Office, a government authority responsible for mediation in labour market disputes and for public wage statistics, consistently give the information that the first reform applied to 18–24-year-olds and the second one to persons below the age of 26 (see, for example, National Mediation Office, 2012, pp. 109–110). The evaluation of the first payroll tax reform published by the National Audit Office (2008, pp. 61, 65–66) erroneously states that the reform applied to 18–24-year-olds (and compares the evolution of employment to that of a group including 25-year-olds). It is not difficult to find press reports containing the same messages. However, the information provided to employers by the Swedish Tax Agency seems not to invite misinterpretations, as it refers to the employee's birth year in a clear and distinct way (thus for 2012, the website of the agency states that persons born in 1986 or later, i.e., those up to 26 years of age, are subject to the payroll tax cut).

It is difficult to assess how widespread any misunderstanding have been in practice 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 non-take-up among firms: (i) unawareness of the reduction; (ii) perceptions that the reduction is temporary; and (iii) perceptions that take-up is associated with large 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.

3. Other reforms 2007–2010

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

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. Due to the absence of suitable control groups, it has proven difficult to evaluate the effects of the reform in a credible way (Edmark et al., 2012). 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. From the analysis of Edmark et al. (2012) this cannot be established with any certainty.

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 regulation may have had an impact on the employment of the young, among whom temporary work is relatively more widespread. 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 is optional, allowing 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 (2012) 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.

In conclusion, it cannot be ruled out that other reforms during the period of study had an impact on the labour market for the young, but there is little evidence to suggest that this should seriously distort my evaluation of the payroll tax reform.

4. Wage formation in retail

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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 – 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 organization, 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.[8] 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 agreements mentioned above specify contractual wage increases as well as minimum wage levels at the industry level for various categories of workers.[9] 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, both contractual wage increases and minimum wages are differentiated by age and experience.[10] Typically, most age groups receive the same increase in SEK per hour or month, but different amounts in general and minimum wage increases, so younger and more inexperienced workers usually get a higher increase in percentage terms. 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, 2011). 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 covered by the *Unionen* agreement, wage formation is more decentralized than for blue-collars as the agreement specifies a ‘wage pot’ for local distribution supplemented with rules guaranteeing increases also at the individual level.

The design of the wage bargaining system for blue-collar workers makes it rather unlikely that the selective payroll tax reduction should be shifted into contractual wage increases for

[8] 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).
[9] 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).
[10] Different scales apply for workers aged 16, 17, 18 and 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).

young workers only. A minimum wage hike seems to be a more plausible outcome. The more decentralized 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. Under these circumstances a more viable union strategy may be to try to raise wages for *all* workers, regardless of age. The size of such wage increases is, however, likely to be smaller than with an across-the-board payroll tax cut.

5. Data and empirical specification

The data set has been obtained from the Confederation of Swedish Enterprise (*Svenskt Näringsliv*) and covers all member firms of the employer organization the Swedish Trade Federation over the period 2000–2011. There are 13,000 member firms with a total of 300,000 employees in the Federation, implying a coverage of about two thirds of all employees in Swedish retail (Svensk Handel, 2011). 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 during the month of September in a given year. The data are based on payroll records and include information on employee category (manual or non-manual), various components of pay, actual and usual hours worked, gender, age, occupation, region and number of employees in the firm.

The 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). 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 accessions and separations thus are defined in relation to employment in the *industry*, not in the firm. Given that involuntary separations cannot be identified, separations defined in this way capture relatively more exits into unemployment than a firm-based measure.

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, not actual, 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.[11] The wage concept used thus excludes premiums for unsocial hours, overtime pay, bonuses and fringe benefits.

Minimum wages for blue-collar workers have been collected from the Retail Agreement (*Detaljhandelsavtalet*) and from circulars, distributed by the Federation to employers, for white-collar workers. 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.

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 are other variables in the data which could help capture the degree of attachment to the job. Besides part-time status, there is a variable in the data set indicating whether the worker is salaried and there is also information on the share of unsocial hours pay of total pay. Salaried workers are typically less mobile while workers with much unsocial hours pay are likely to be students working mainly during evenings and weekends, with little long-term attachment to the job.

Some observations have been excluded from the data: (i) observations in municipalities within the regional support areas, subject to a different payroll tax regime; (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

[11] A minority of blue-collar workers in the retail industry and most white-collars are salaried. 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.

lowest minimum wage for blue-collar workers), in order to minimize the influence of measurement errors.

I have chosen workers aged 19–25 as the benchmark treatment group, but also experiment with more narrowly defined treatment groups in terms of age. Figure 3 shows accession rates in retail for blue- and white-collar workers by age group over the period 2000–2011. Overall, rates are considerably higher among 19–25-year-olds, than in the comparison group of persons aged 25 to 27. 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. For blue-collars there is a downward trend in accession rates, reflecting the worsening 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. The years before the payroll tax reforms in 2007 and 2009 are indicated by a vertical lines. Pre-reform trends before 2007 seem reasonably parallel for the two age groups, 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, however, more accentuated in the treatment group. Then, in 2009–2010 the treatment group seems to recover after the decline, while accession rates in the comparison group remain at a low level. The picture is a bit 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 4. 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 4, 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 5 shows the evolution of weekly hours. There is a downward trend in hours for both age groups among the blue-collar workers. Post-reform development for blue-collars does not suggest that hours increased in the treatment group relative to the control. On the contrary, hours fell more markedly among the treated in 2008–2009. 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 6. Among blue-collars, wages rose by 28 per cent, for those aged 19–25, and by 21 per cent, for the 25–27 age group. Since wages increased faster among the youngest, the figures also imply wage compression between the two age groups, which is consistent with the rising minimum wages in relation to median wages in the industry that have been documented in Skedinger (2011). Real wages continued to increase for both age groups following the first payroll tax reform, and even did so 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 main sample, consisting of 19–25-year-olds and 27–29-year-olds, are shown in Table 1. The table confirms that there are considerable differences between blue- and white-collar workers. For example, the former tend to be younger, work more part-time, are employed in larger firms 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.

The empirical strategy is to use a difference-in-difference approach to compare changes in the outcome variables (accessions, separations, hours and hourly wages) before and after the changes in payroll taxes. The main treatment group in our analysis consists of the age group 19–25 and 27–29-year-olds are used as the control.

Based on the data on individuals and firms, I estimate the following regression:

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

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 time-varying individual characteristics, and z_{it} is a vector of time-varying firm characteristics. The individual and firm controls are dummies for gender, region, occupation, part-time work, salaried position, tenure, year (which controls for common shocks to the business cycle), the share of unsocial hours pay of total pay and the total number of employees in the firm and its square. 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).

A key issue in identification is using the appropriate treatment and control groups. Egebark and Kaunitz (2012) 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 (*substitution bias*). 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, employers who prefer older workers to younger ones, may start hiring relatively more of the former following a wage cost reduction for the latter which serves to underestimate the treatment effect (*income effect bias*). The income effect could be more likely in firms already employing a large share of young workers.

Third, as pointed out by Egebark and Kaunitz (2012), 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 stylized 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 7 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 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 8 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.[12] 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

[12] 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.

that 25-year-olds have been subject to 58 and 54 per cent less treatment, respectively, relative to the age group treated most intensively among blue- and white-collar workers.

Fourth, the treatment group should also in fact be *treated*, which is not self-evident if there is non-take-up among firms. The net effect of these potential biases is ambiguous a priori. Yet another source of downward bias, when examining the effects on wages of the payroll tax reforms, is that it well 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.

6. Econometric results

The before-period is 2004–2006 throughout these estimations. 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 and in the post-reform evolution of collectively agreed payroll fees, noted in previous sections, separate regressions will be run for the two groups. T-statistics have been clustered at the firm level, which is the most conservative alternative.

I have chosen workers aged 19–25 as the benchmark treatment group and 27–29-year-olds as the control, but also experiment with more narrowly defined treatment and control groups in terms of age. 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 19–25-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*), the third to the log of weekly hours among new recruits in year *t*, the fourth to the log of weekly hours among remaining 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 that the probability of entry into the retail industry increased by 2.5 percentage points in 2007, in the treated group relative to the control. The size of the effect diminishes as the after-period is extended, however, although the coefficients are always significant. For the longest observation period, 2004–2011, the coefficient implies that the probability of entry increased by 1.9 percentage points. The coefficients for the probability of exit are increasing over time, but never attain significance. A rough estimate of the long-term increase in net employment is 1.9–0.1=1.8 per cent. This implies an elasticity with regard to total wage costs of about –0.17.[13] Thus, consistent with the intentions behind the reforms, there seems to be a persistent increase in the inflow to employment in retail among the young, but the effects are small in relation to the sizeable reductions of payroll taxes that were implemented.

The results in the third and fourth columns suggest that the reforms were associated with a reduction in hours over time, mainly among new recruits. The coefficients for the period up to 2011 are relatively large, –0.061 for new recruits and –0.052 for remaining workers. There is some evidence in the fifth and sixth columns of increasing wages over time, particularly among new recruits, but the effects are small. In the short run, the coefficients suggest an upward pressure on wages in the region of 0.5 per cent and in the longer run between 0.8 and 1.1 per cent.

The regressions for white-collar workers, in the bottom panel of Table 2, tell a somewhat different story. There is a short-run increase in job accessions (0.032), but it is estimated with low precision. The results point to an increase in exits, especially in the short term. There is a short-run increase in hours that is significant only for remaining workers (with a coefficient of 0.031 for 2004–2008). In the longer run, the imprecisely estimated coefficients for hours turn negative for both groups of workers. Unlike in the regressions for blue-collars, hours appear to be more responsive to changes in payroll taxes than job accessions, but the effect on hours is not sustained over time. The differential response for the intensive and extensive margins across worker types is consistent with hiring and firing costs being higher among white-collars than among blue-collars. A potential source of differential costs is the fact that fixed-term contracts can be terminated with short notice and at little cost and these contracts may be less prevalent among white-collar workers. Wages increase, but only for the workers

[13] The elasticity, which unlike conventional elasticities is conditional on employment, is calculated as $0.018/[0.4(-11.1/132.42) + 0.6(-15.9/132.42)] = -0.17$.

who were employed over two successive years. The coefficient for the estimation period 2004–2011 is 0.021. The absence of any palpable reform effects on job accessions among white-collar workers in the long run may, at least partly, be due to a sustained increase in wages.

In 2009 the financial crisis set in with full force and this seems to have had important repercussions on the estimated reform effects in Table 2. The probability of entry and hours of work are both reduced in the estimates for 2004–2009, compared to estimates for the previous period, while there is no sign of 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.

Table 3 shows estimations for the full specification with a panel of firms and firm fixed effects. The use of a panel entails a substantial loss of observations in many of the specifications. For both blue- and white-collar workers, the results are basically robust.

The estimates of the reform effects including the period 2009–11 in Tables 2 and 3 may be less reliable than the estimates for previous periods. During 2009–11 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 2009–11 as a 27–29-year old, while a 23-year-old in 2007, part of the treatment group 2007–09, 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 2009–11 may thus be distorted.

A number of experiments with narrowing the treatment and control groups in terms of age were performed. The specifications correspond to those of Table 2, that is, without fixed effects. The first experiment, shown in Table 4, uses those aged 25 as the treatment group and the control group includes the 27-year-olds only. Not much suggest an improvement – or deterioration, for that matter – in the employment situation for the treatment group. If substitution across age groups is important we would expect larger effects on accessions, separations and hours in the restricted sample, but that is apparently not the case. The small effects are consistent with non-take-up among firms and the relatively small expected discounted value of the tax reduction for 25-year-olds indicated in Figure 8. However, there is once again evidence of increasing wages in the longer term, but the coefficients are small in most cases. .

The second experiment, with the same control group as in Table 2 (27–29), but with the treatment group narrowed down to 22–25-year-olds, is displayed in Table 5. Again, there is not much evidence of favourable effects on employment. There is a notable decrease in hours in the long term for remaining blue-collar workers and wages seem to increase, especially among white-collar workers. The results suggest that increases in job accessions found in Table 2 have been concentrated to workers aged 19–21.

Two additional experiments with different age groups were performed, the results of which are not reported in the tables (but available from the author on request). The first one of these experiments concerns 25-year-olds as the treatment and workers aged 26 as the control. The estimation period is now restricted to end at 2007 or 2008, since the control group was treated in the second payroll tax reform in 2009. In the other unreported experiment, the treated age group consists of 26-year-olds and the control group is aged 27. This means that only the effects of the second payroll tax reform in 2009 are estimated. Consequently, the estimation period starts in 2006 and ends in 2011 when the longest period is considered. As in Tables 4 and 5, there is little evidence of improved labour market opportunities among the treated in the two experiments. Wage-increasing effects cannot be established with any confidence, which may be due to the short post-reform period and the relatively small samples involved.

The remaining tables are concerned with robustness checks of the benchmark specification in Table 2, with 19–25-year-olds as the treatment group. First, the possibility of heterogeneous treatment effects across subgroups is considered. Table 6 presents results for two different subgroups of workers. The upper panel deals with low-paid workers, specifically those who are bound by their individual minimum wage, within an interval of 5 per cent. This exercise is only feasible for blue-collar workers, as white-collars are subject to substantially lower, and non-binding minimum wages (see Skedinger, 2011). The estimates for entry are all in the region of 0.06–0.08, which indicates that the effects are substantially stronger for the low-paid than for other workers.[14] An important difference compared to the results for all workers is that the onset of the financial crisis is *not* associated with a drastic reduction in the

[14] Similar results are obtained when an interval of 1 per cent is used and when all workers with subminimum wages are excluded (not shown).

probability of entry. On the contrary, the estimated effects are larger in magnitude during and after the crisis than before. The estimates for exits are positive, but small and insignificant in most specifications. The associated long-run elasticity of net employment with respect to total wage costs is –0.57. As before, the reform is associated with a negative effect on hours, but this is confined to new recruits. (For remaining workers, the coefficients are all positive and insignificant.) Effects on wages are small, suggesting that the reform did not trigger wage hikes targeted to the treated among the low-paid, at least within the 5 per cent wage interval that defines the sample, in consistence with the large effects found for accessions. As mentioned previously, there is a distinct possibility that the minimum wages themselves were driven up as a consequence of the payroll tax reforms. If so, more workers may have been bound by minimum wages, as defined here, than without the reforms.

The bottom panel of Table 6 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 reforms (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. For blue-collar workers, the coefficients of the wage variables suggest increases of around 1 per cent in the long run, while an unexpected negative sign is obtained for new recruits among white-collars.

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 in Table 7. Placebo reforms for the years 2003, 2004 and 2005 are examined. The estimates are either significantly negative or insignificant. In only one case, out of 18 regressions for entry, is a positive and significant coefficient estimated (for white-collar workers and the 2005 placebo reform).

7. Conclusions

This paper has exploited a payroll tax reform targeted at young workers, implemented in two stages in 2007 and 2009. The results indicate that – on average – the effects on entry, exit, hours and wages in the retail industry have been small, both in absolute magnitudes and in relation to the sizeable cuts in taxes. My results 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 general or regional payroll taxes. The absence of large wage effects in my study may be explained by norms in collective wage bargaining, precluding selective payroll tax reductions from being transmitted into selective wage increases. While the possibility of general wage increases cannot be ruled out, they seem less likely to be large with a targeted payroll tax reduction than under general payroll tax reductions.

For workers bound by minimum wages the estimated effects suggest substantially larger effects on the probability of entry. The reform effects are in the region of 6–8 per cent, implying an elasticity of net employment with respect to total wage costs of around –0.57. This result is consistent with the view that high minimum wages represent a serious obstacle to labour market entry among the young. Considering that most evaluations of previous payroll tax reductions in the literature indicate that the reforms have been expensive, in terms of budgetary cost per job created, these results may be helpful for improving the design of such reforms. It should 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.

The second stage of the payroll tax reduction in 2009 coincided with the onset of the financial crisis. This makes it difficult to disentangle the effects from the crisis from the short-run effects of the second payroll tax reduction as well as the long-run effects of the first reduction. The crisis may have contributed to lowering the impact of the reform on both employment and wages. However, the results make it clear that, despite the crisis, the probability of entry among those bound by minimum wages did not drop drastically, as was indeed the case among higher-paid workers. On the contrary, the results reveal that treated workers among the low paid experienced improving labour market prospects in terms of entry during and after the crisis.

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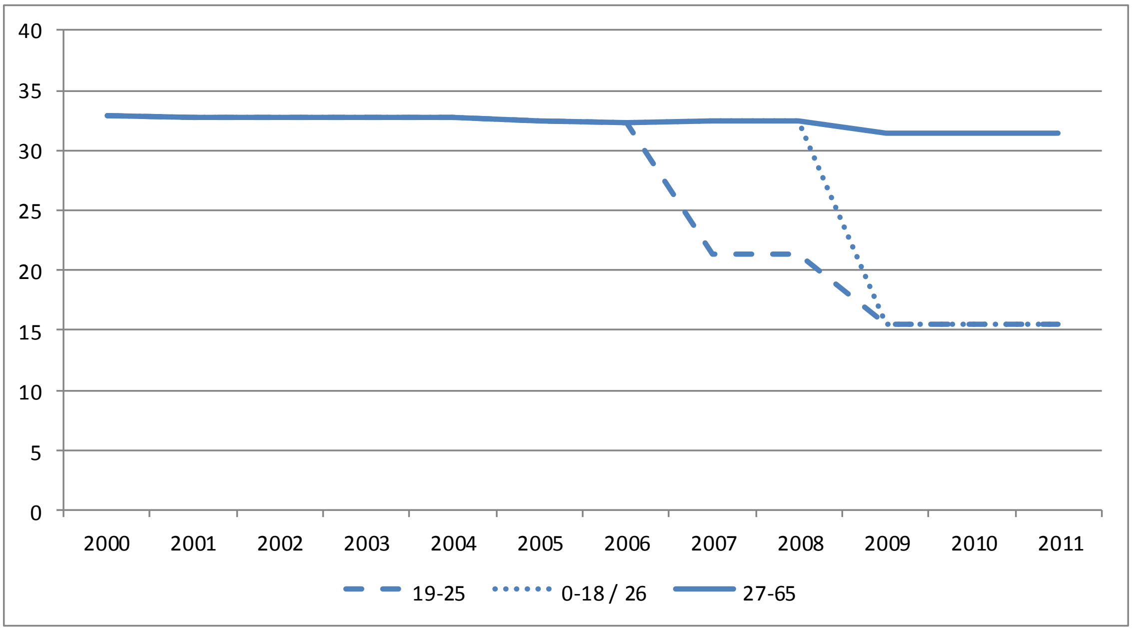
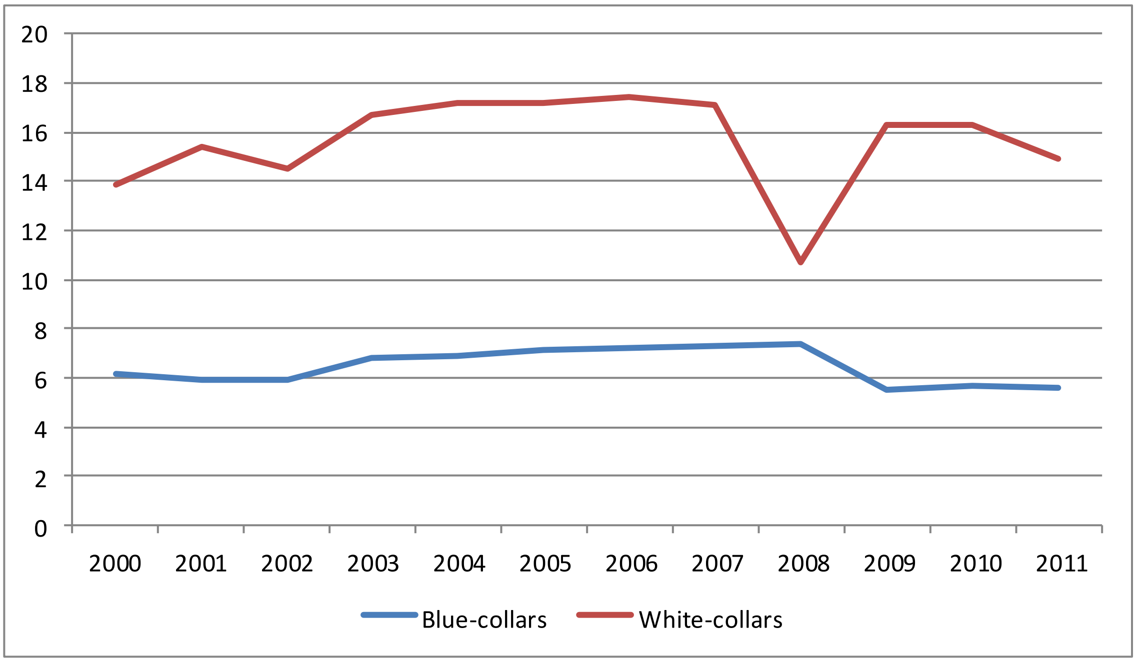
Figure 1. Payroll tax rates, by age group, 2000–2011. Per cent



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

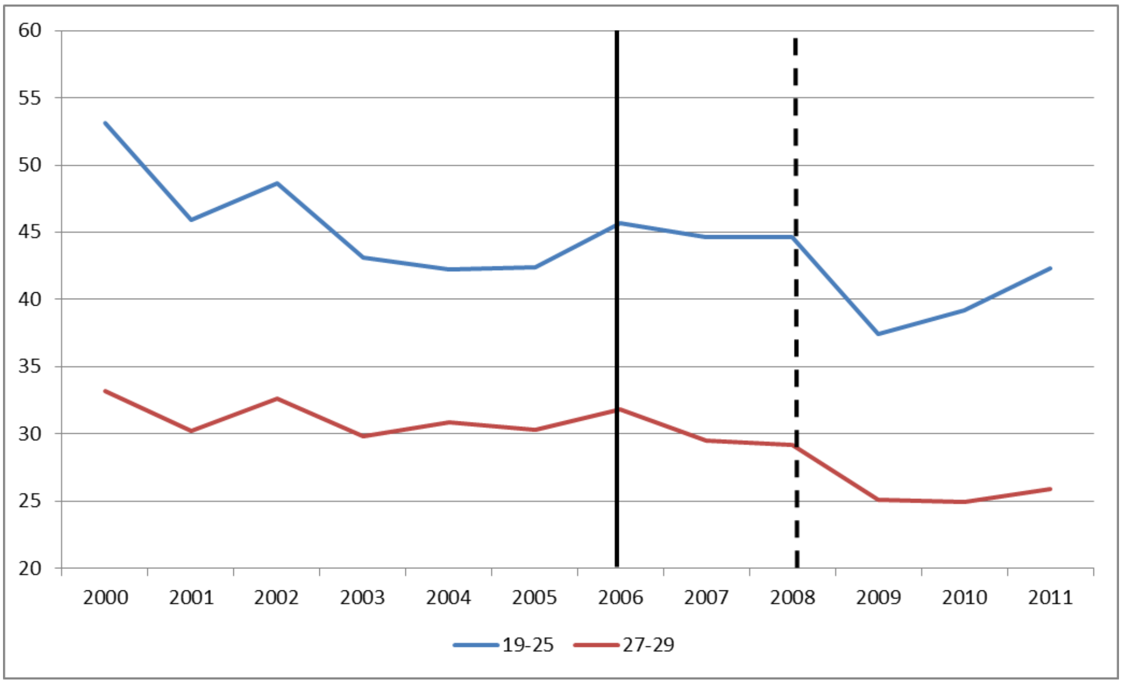
Figure 2. Collectively agreed payroll fees in the private sector, by worker category, 2000–2011. Per cent



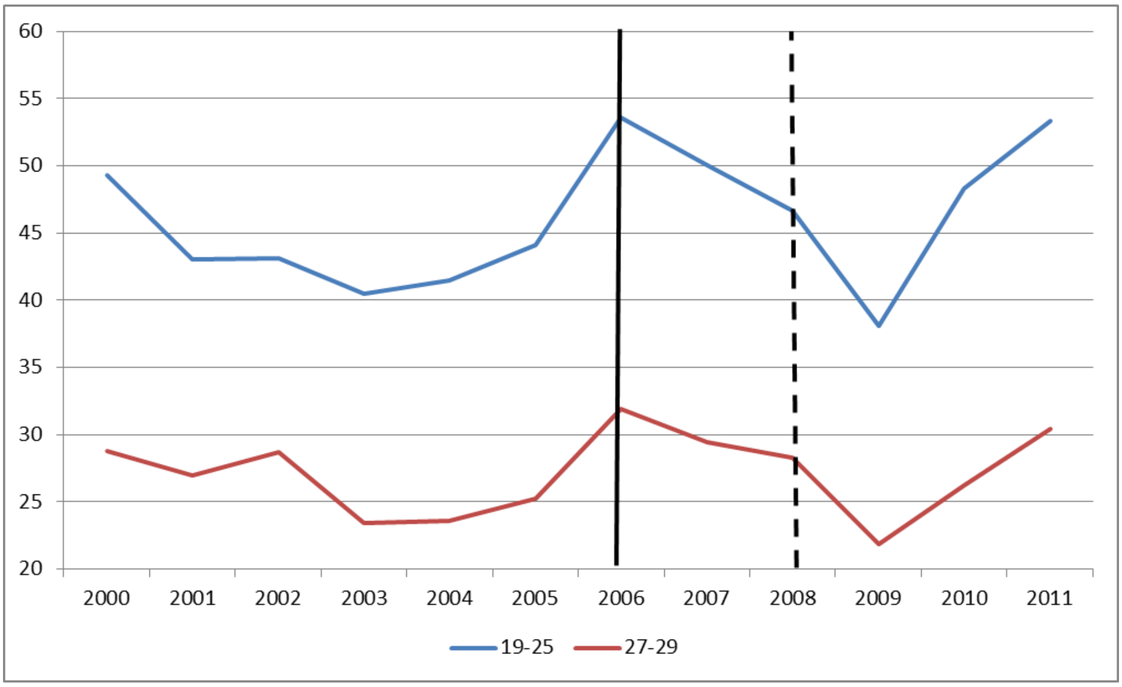
Note: The fees include the special tax on pension costs.
Source: National Mediation Office.

Figure 3. Accession rates in the retail industry, by age group, 2000–2011. Per cent

a) Blue-collar workers



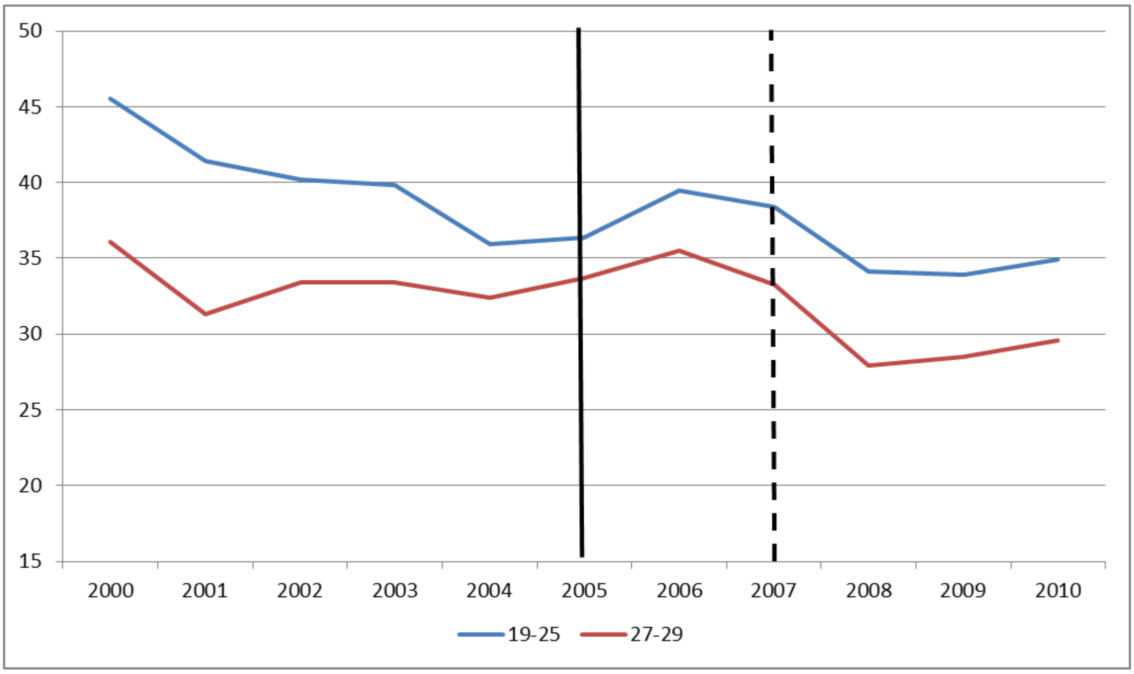
b) White-collar workers



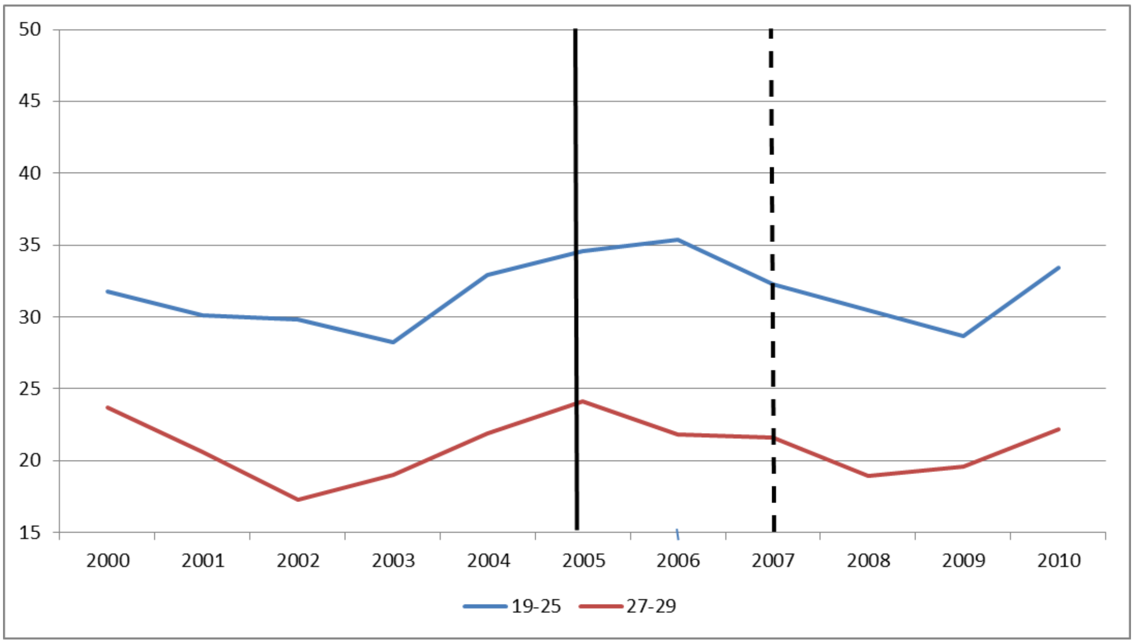
Note: Accession rates refer to year *t*. The year before the payroll tax reform is indicated by a vertical line.
Source: Own calculations.

Figure 4. Separation rates in the retail industry, by age group, 2000–2010. Per cent

a) Blue-collar workers



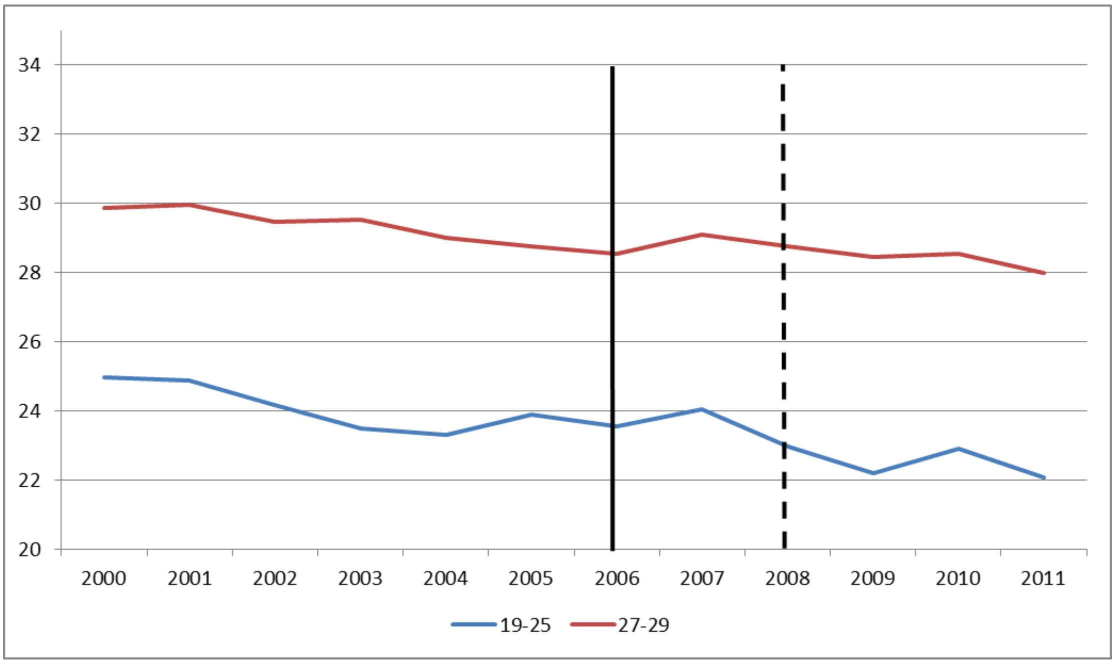
b) White-collar workers



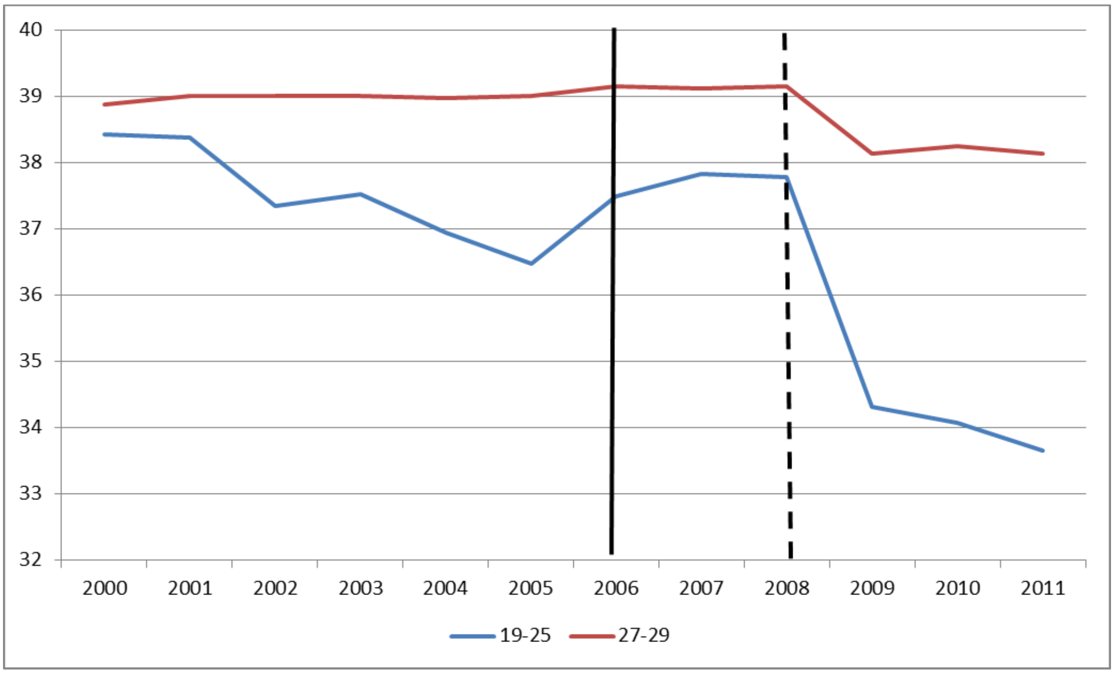
Note: Separation rates refer to year *t+1*. Two years before the payroll tax reform is indicated by a vertical line.
Source: Own calculations.

Figure 5. Weekly hours in the retail industry, by age group, 2000–2011

a) Blue-collar workers



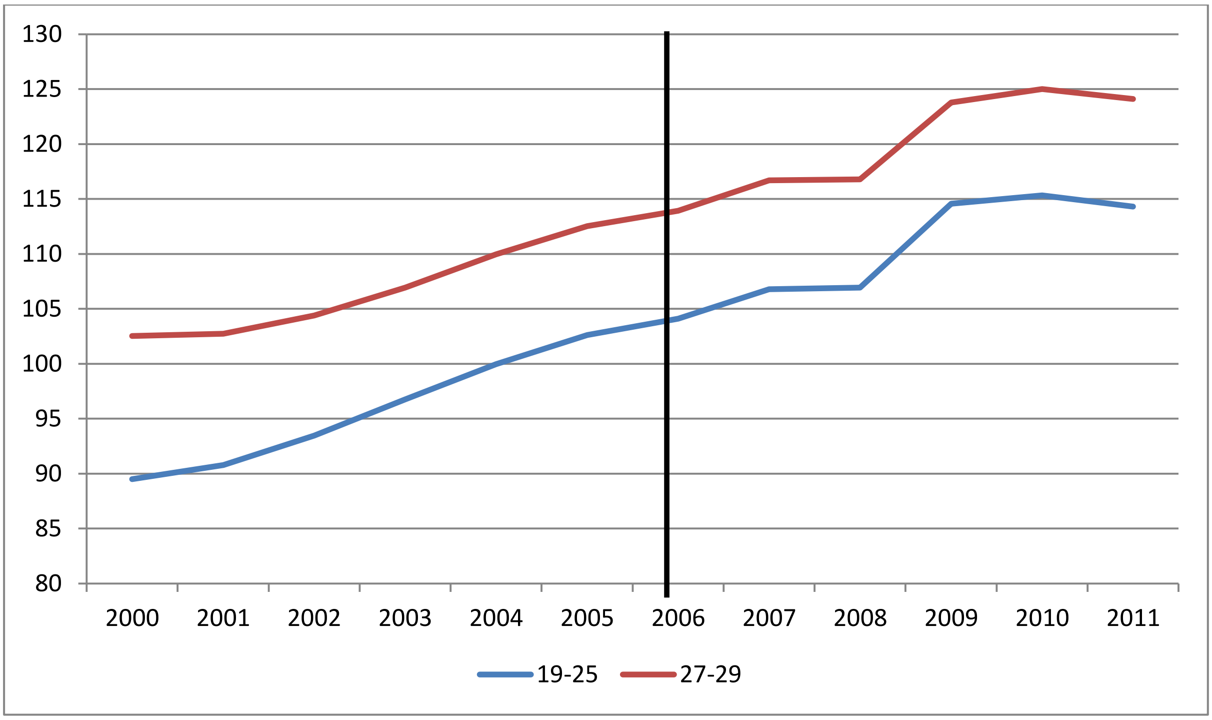
b) White-collar workers



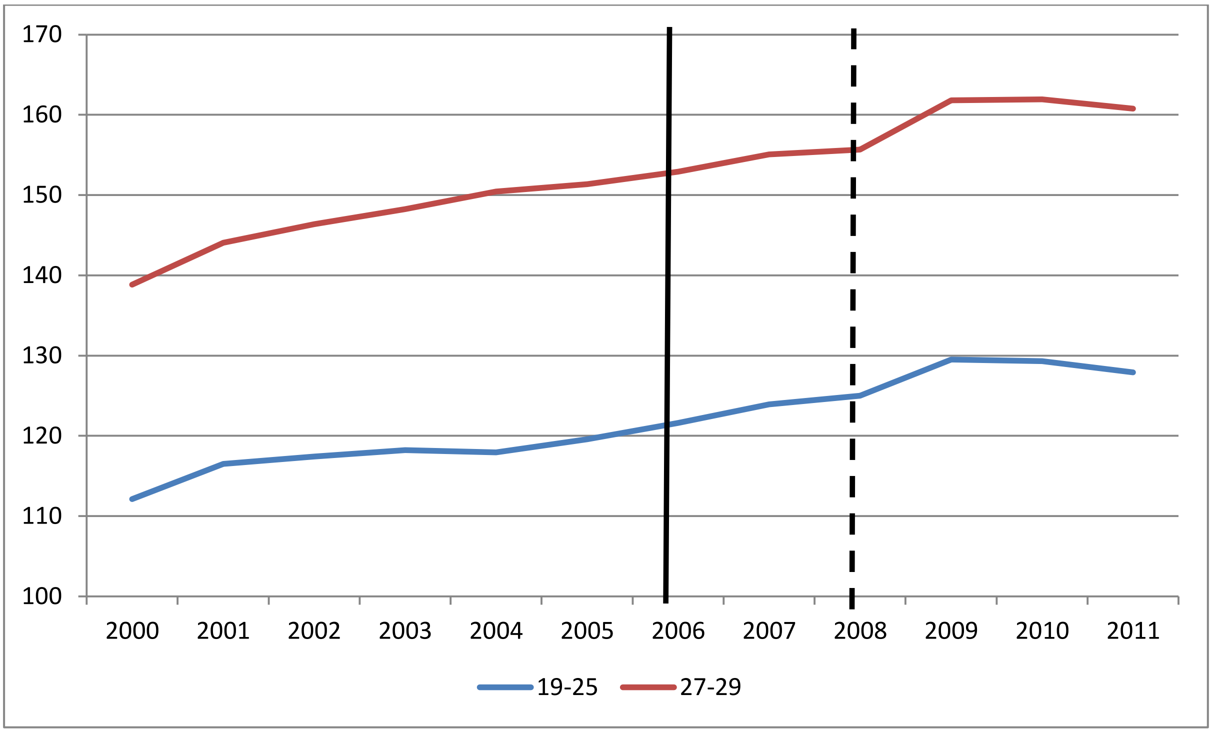
Note: The year before the payroll tax reforms is indicated by a vertical line.
Source: Own calculations.

Figure 6. Real hourly wage in the retail industry, by age group, 2000–2011. SEK

a) Blue-collar workers



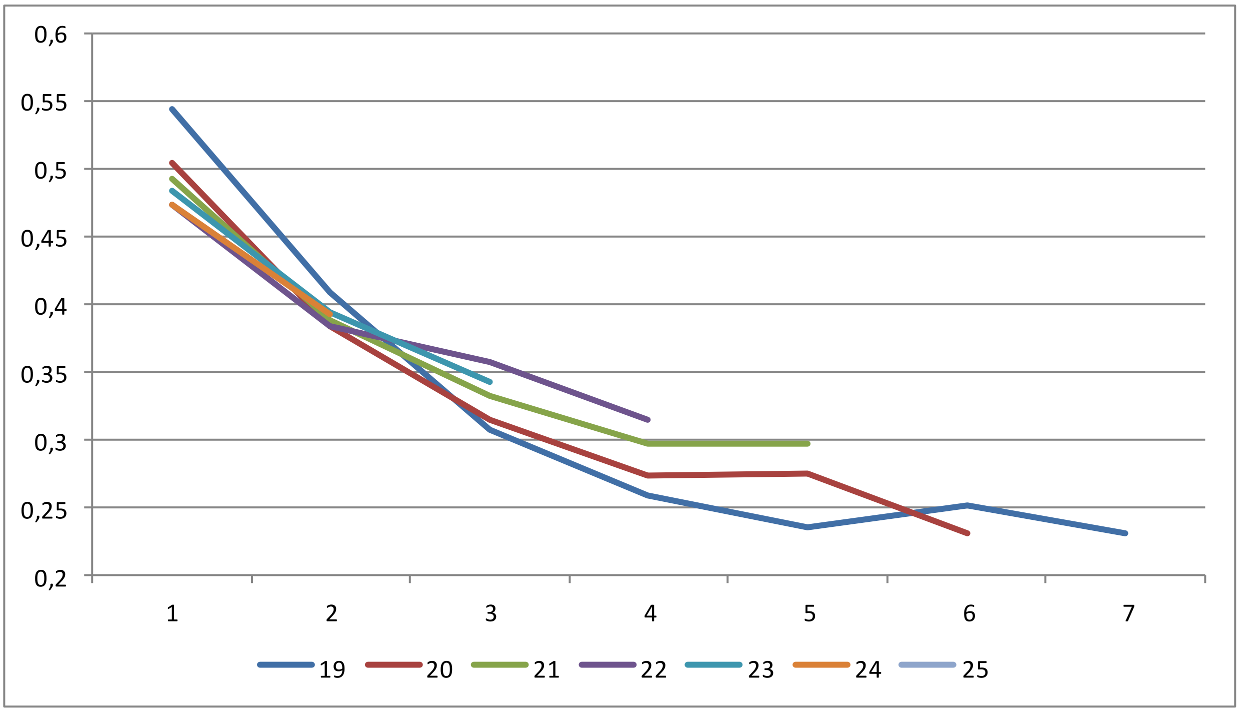
b) White-collar workers



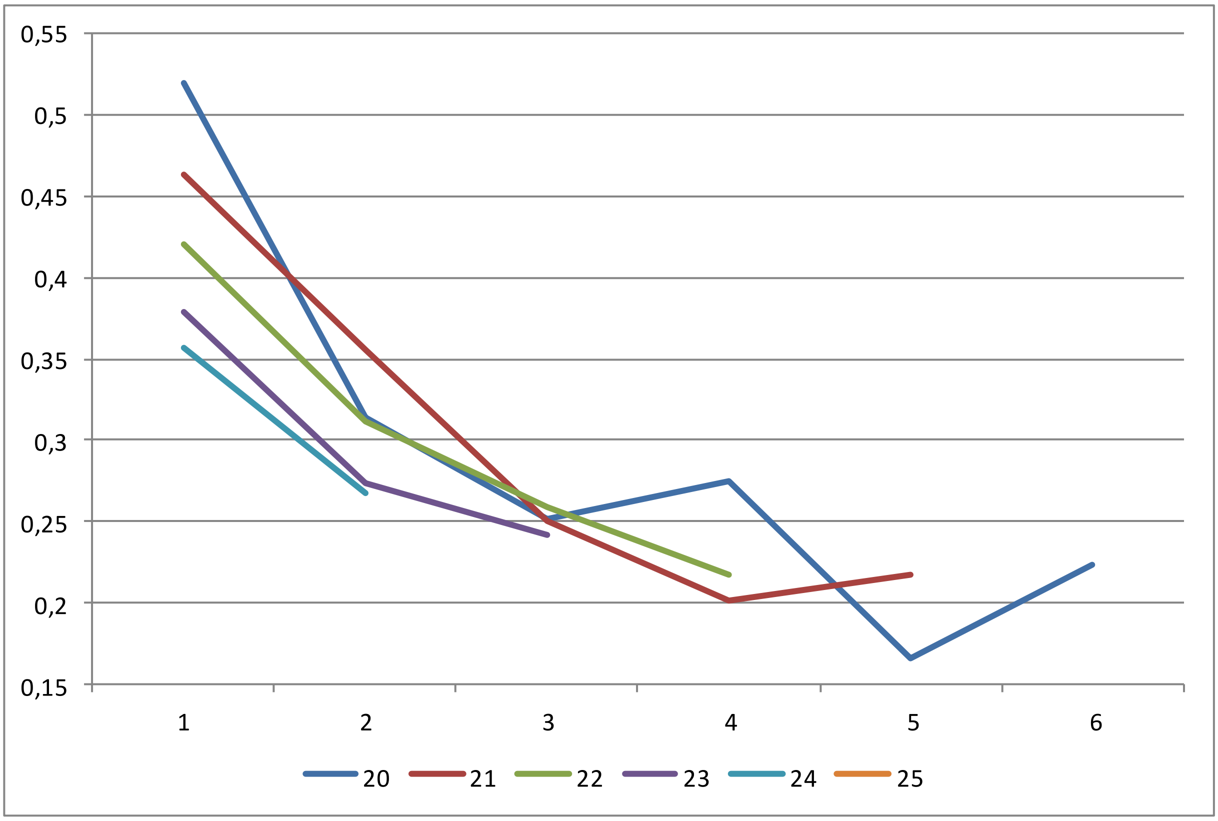
Note: 2011 prices. The year before the payroll tax reforms is indicated by a vertical line.
Source: Own calculations.

Figure 7. Separation rates by age at hiring, conditional on tenure. 1998–2005

a) Blue-collar workers



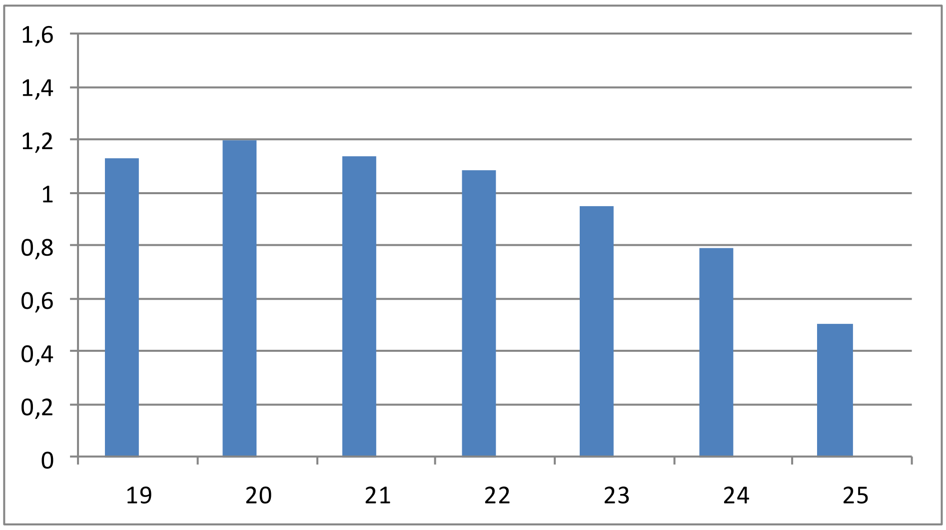
b) White-collar workers



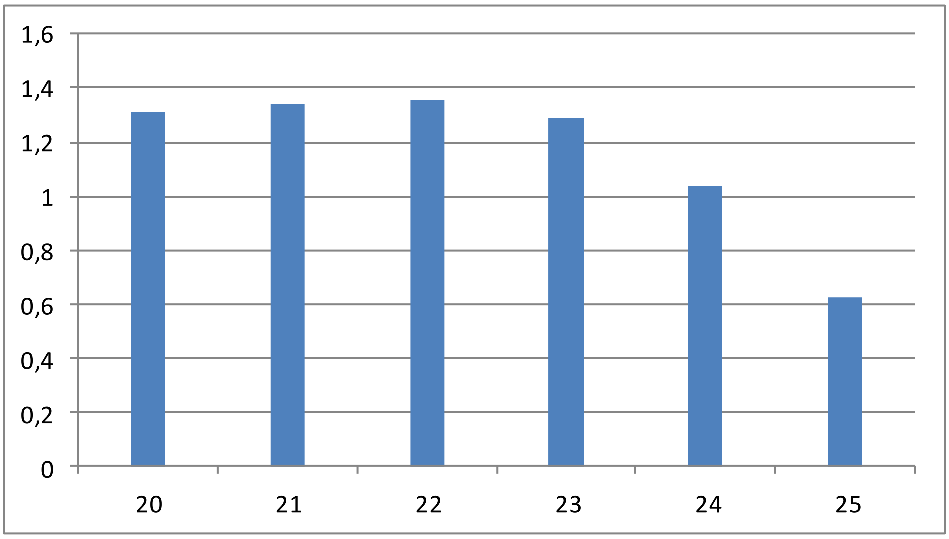
Source: Own calculations.

Figure 8. Expected discounted value of payroll tax cut, by age

a) Blue-collar workers



b) White-collar workers



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

Table 1. Descriptive statistics, 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
Tenure 0 years	0.414	0.223	0.500	0.280
Tenure 1 year	0.272	0.197	0.250	0.244
Tenure 2 years	0.151	0.151	0.125	0.170
Tenure ≥ 3 years	0.163	0.429	0.125	0.306
Unsocial hours pay, % of total pay	0.195	0.159	0.025	0.008
Salaried	0.154	0.342	0.809	0.973
Part-time	0.551	0.361	0.119	0.029
Close to minimum wage: At most 5 % above	0.657	0.467	–	–
Close to minimum wage: At most 1 % above	0.387	0.202	–	–
No. of employees in firm	957.9	1098.1	819.7	808.6
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 obs.	213,536	52,168	19,688	26,896

Table 2. Treated age group: 19–25. Control age group: 27–29

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.025	–0.008	0.000	–0.009	0.006	0.003
	(2.63)	(0.92)	(0.01)	(0.59)	(1.73)	(1.29)
	[117,414]	[114,627]	[47,763]	[68,111]	[43,720]	[62,723]
2004–08	0.026	–0.001	–0.022	–0.022	0.006	0.004
	(3.13)	(0.13)	(0.67)	(1.42)	(2.30)	(1.32)
	[153,003]	[148,441]	[62,420]	[88,601]	[57,384]	[81,815]
2004–09	0.015	0.004	–0.054	–0.042	0.009	0.006
	(2.09)	(0.72)	(1.60)	(2.43)	(3.43)	(2.51)
	[189,242]	[184,653]	[74,948]	[111,881]	[68,990]	[103,652]
2004-10	0.015	0.005	–0.058	–0.046	0.010	0.007
	(2.19)	(0.82)	(1.81)	(2.66)	(4.05)	(2.97)
	[226,377]	[221,546]	[88,225]	[135,311]	[81,047]	[125,524]
2004-11	0.019	0.005	–0.061	–0.052	0.011	0.008
	(2.83)	(0.85)	(2.01)	(3.04)	(4.00)	(3.07)
	[265,704]	[259,821]	[103,360]	[158,976]	[95,186]	[147,671]
White-collars						
2004–07	0.032	0.035	0.045	0.026	–0.005	0.009
	(1.61)	(2.10)	(1.00)	(1.55)	(0.44)	(1.19)
	[20,407]	[20,816]	[7,279]	[12,969]	[7,080]	[12,841]
2004–08	0.020	0.027	0.022	0.031	–0.004	0.007
	(1.33)	(2.01)	(0.45)	(2.13)	(0.46)	(0.97)
	[26,645]	[26,781]	[9,514]	[16,930]	[9,266]	[16,768]
2004–09	0.002	0.027	–0.010	0.006	–0.003	0.014
	(0.14)	(2.05)	(0.18)	(0.27)	(0.30)	(1.63)
	[33,005]	[33,774]	[11,313]	[21,441]	[11,036]	[21,243]
2004-10	0.008	0.019	0.001	–0.004	0.006	0.017
	(0.59)	(1.47)	(0.02)	(0.12)	(0.61)	(1.83)
	[39,411]	[39,962]	[13,577]	[25,516]	[13,264]	[25,296]
2004-11	0.013	0.014	–0.034	–0.005	0.009	0.021
	(0.98)	(1.16)	(0.51)	(0.16)	(0.83)	(2.17)
	[46,584]	[46,468]	[16,495]	[29,725]	[16,016]	[29,476]

Notes: Only the estimated reform effects in the regressions are shown. In the regressions for exits, the estimation periods are 2003–06, 2003–07 and so on until 2003–2010. The regressions for entry include dummies for the treated group, the post period, gender, occupation, part-time work, salaried position, region, and year as well as continuous variables for the share of unsocial hours pay of total pay, number of employees at the firm and its square. The exit regressions also include dummies for tenure. The hours and wage regressions for new recruits include the same variables as the entry regressions, except that part-time work is excluded in the hours regressions. The hours and wage regressions for remaining workers include the same variables as the exit regressions, except that part-time work is excluded in the hours regressions. Absolute, robust t-statistics, clustered at firm level, within parentheses. Number of observations within brackets.

Table 3. Treated age group: 19–25. Control age group: 27–29. Firm panel and firm fixed effects

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.024	–0.007	–0.025	–0.015	0.004	0.007
	(2.49)	(0.75)	(0.75)	(0.96)	(1.18)	(2.70)
	[97,712]	[90,172]	[39,151]	[57,477]	[35,338]	[52,406]
2004–08	0.021	–0.008	–0.036	–0.024	0.005	0.008
	(2.15)	(1.05)	(1.21)	(1.46)	(1.93)	(2.75)
	[119,158]	[106,725]	[47,625]	[70,187]	[43,032]	[63,827]
2004–09	0.012	–0.000	–0.070	–0.046	0.008	0.010
	(1.40)	(0.06))	(2.31)	(2.60)	(2.79)	(3.84)
	[138,577]	[123,716]	[53,403]	[83,630]	[48,140]	[76,057]
2004-10	0.011	0.001	–0.063	–0.052	0.009	0.011
	(1.24)	(0.17)	(2.13)	(3.23)	(3.11)	(4.00)
	[155,013]	[139,107]	[58,641]	[94,689]	[52,463]	[85,922]
2004-11	0.016	0.001	–0.067	–0.056	0.009	0.012
	(1.81)	(0.099	(2.34)	(3.51)	(2.86)	(4.04)
	[173,474]	[152,935]	[65,050]	[106,471]	[58,148]	[96,486]
White-collars						
2004–07	0.030	0.051	0.003	0.025	0.007	0.002
	(1.45)	(3.22)	(0.09)	(1.27)	(0.68)	(0.36)
	[17,377]	[16,735]	[6,163]	[11,084]	[5,991]	[10,964]
2004–08	0.011	0.042	–0.002	0.032	0.006	0.005
	(0.68)	(3.34)	(0.06)	(1.91)	(0.70)	(0.75)
	[21,314]	[19,922]	[7,624]	[13,531]	[7,412]	[13,378]
2004–09	–0.005	0.031	0.024	0.034	–0.001	0.011
	(0.31)	(2.77)	(0.52)	(1.74)	(0.17)	(1.83)
	[23,592]	[22,902]	[8,000]	[15,410]	[7,770]	[15,223]
2004-10	0.004	0.022	0.004	0.019	0.002	0.010
	(0.28)	(1.82)	(0.21)	(1.61)	(0.19)	(1.67)
	[25,025]	[24,400]	[8,342]	[16,490]	[8,092]	[16,297]
2004-11	0.011	0.020	0.011	0.021	0.000	0.014
	(0.98)	(1.66)	(0.46)	(1.50)	(0.04)	(2.10)
	[26,489]	[25,312]	[8,746]	[17,532]	[8,461]	[17,322]

Table 4. Treated age group: 25. Control age group: 27

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.001	0.003	–0.016	0.010	0.007	0.003
	(0.04)	(0.19)	(0.40)	(0.39)	(1.34)	(0.84)
	[20,016]	[19,258]	[6,280]	[13,530]	[5,733]	[12,409]
2004–08	–0.004	0.009	0.007	0.014	0.005	0.003
	(0.30)	(0.87)	(0.17)	(0.72)	(1.27)	(1.25)
	[26,037]	[24,915]	[8,062]	[17,716]	[7,397]	[16,284]
2004–09	–0.002	0.012	–0.013	0.004	0.007	0.006
	(0.14)	(1.29)	(0.34)	(0.24)	(1.78)	(2.51)
	[32,174]	[31,017]	[9,692]	[22,162]	[8,929]	[20,493]
2004–10	–0.002	0.015	–0.001	–0.010	0.007	0.006
	(0.18)	(1.58)	(0.04)	(0.53)	(1.97)	(2.46)
	[38,561]	[37,302]	[11,301]	[26,869]	[10,387]	[24,898]
2004–11	–0.001	0.012	0.007	–0.007	0.006	0.005
	(0.08)	(1.31)	(0.20)	(0.37)	(1.79)	(2.26)
	[45,332]	[43,888]	[13,189]	[31,672]	[12,144]	[29,420]
White-collars						
2004–07	0.026	0.013	0.076	0.002	–0.003	0.021
	(0.89)	(0.56)	(2.59)	(0.13)	(0.16)	(1.84)
	[6,100]	[6,224]	[2,082]	[3,976]	[2,038]	[3,944]
2004–08	–0.005	0.002	0.048	0.011	–0.002	0.014
	(0.21)	(0.12)	(2.02)	(1.28)	(0.13)	(1.54)
	[7,905]	[7,990]	[2,730]	[5,124]	[2,674]	[5,083]
2004–09	–0.008	0.002	0.024	0.004	–0.004	0.018
	(0.37)	(0.14)	(0.90)	(0.53)	(0.36)	(2.29)
	[9,722]	[10,022]	[3,190]	[6,464]	[3,130]	[6,415]
2004–10	–0.009	0.002	0.007	–0.012	0.001	0.020
	(0.47)	(0.11)	(0.24)	(0.88)	(0.09)	(2.73)
	[11,548]	[11,800]	[3,767]	[7,701]	[3,705]	[7,643]
2004–11	–0.004	–0.006	–0.002	–0.007	0.001	0.024
	(0.20)	(0.38)	(0.09)	(0.53)	(0.06)	(3.63)
	[13,656]	[13,661]	[4,538]	[9,025]	[4,449]	[8,959]

Table 5. Treated age group: 22–25. Control age group: 27–29

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.010	–0.004	0.010	–0.019	0.002	0.001
	(1.10)	(0.44)	(0.03)	(1.29)	(0.43)	(0.35)
	[75,461]	[73,126]	[25,116]	[49,456]	[22,943]	[45,408]
2004–08	0.008	0.004	–0.012	–0.023	0.002	0.001
	(1.00)	(0.54)	(0.42)	(1.63)	(0.83)	(0.36)
	[97,704]	[94,683]	[32,320]	[64,264]	[29,645]	[59,208]
2004–09	0.004	0.007	–0.024	–0.032	0.005	0.003
	(0.62)	(1.16)	(0.89)	(2.20)	(1.80)	(1.53)
	[120,797]	[117,279]	[38,722]	[80,709]	[35,574]	[74,592]
2004-10	0.006	0.009	–0.021	–0.034	0.005	0.004
	(0.83)	(1.42)	(0.83)	(2.38)	(2.00)	(1.77)
	[144,947]	[140,788]	[45,525]	[97,813]	[41,754]	[90,519]
2004-11	0.009	0.010	–0.022	–0.036	0.004	0.003
	(1.29)	(1.68)	(0.98)	(2.58)	(1.81)	(1.62)
	[170,707]	[165,621]	[53,264]	[115,527]	[48,944]	[107,091]
White-collars						
2004–07	0.015	0.018	0.019	0.019	0.004	0.010
	(0.77)	(1.05)	(0.70)	(1.86)	(0.37)	(1.21)
	[18,564]	[19,071]	[6,039]	[12,388]	[5,889]	[12,282]
2004–08	0.010	0.013	0.016	0.017	0.001	0.009
	(0.63)	(1.00)	(0.58)	(2.41)	(0.09)	(1.14)
	[24,236]	[24,448]	[7,909]	[16,156]	[7,727]	[16,019]
2004–09	–0.006	0.013	–0.015	–0.009	0.001	0.015
	(0.38)	(1.07)	(0.46)	(0.53)	(0.08)	(1.70)
	[30,124]	[30,794]	[9,424]	[20,482]	[9,221]	[20,315]
2004-10	–0.002	0.008	–0.011	–0.019	0.008	0.017
	(0.17)	(0.72)	(0.29)	(0.81)	(0.78)	(1.84)
	[35,967]	[36,530]	[11,276]	[24,419]	[11,050]	[24,231]
2004-11	0.002	0.005	–0.044	–0.019	0.010	0.020
	(0.12)	(0.45)	(0.94)	(0.82)	(0.99)	(2.14)
	[42,467]	[42,484]	[13,680]	[28,477]	[13,323]	[28,265]

Table 6. Treated age group: 19–25. Control age group: 27–29. Sub-groups

a) Workers bound by minimum wage (5 % interval)

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.068	0.016	–0.083	–0.037	0.004	–0.001
	(5.13)	(1.23)	(1.69)	(1.48)	(1.10)	(0.45)
	[62,596]	[56,439]	[28,480]	[35,793]	[24,540]	[30,579]
2004–08	0.064	0.012	–0.055	–0.036	0.004	–0.001
	(6.57)	(1.21)	[1.18)	(1.50)	(1.22)	(0.47)
	[83,634]	[75,421]	[37,757]	[47,258]	[32,848]	[40,661]
2004–09	0.082	0.013	–0.072	–0.045	0.000	–0.002
	(8.86)	(1.57)	(1.69)	(1.83)	(0.12)	(0.66)
	[99,627]	[96,405]	[46,320]	[62,675]	[40,522]	[54,657]
2004-10	0.072	0.015	–0.073	–0.045	0.003	–0.001
	(8.53)	(1.75)	(1.77)	(1.79)	(0.94)	(0.37)
	[122,579]	[112,839]	[55,603]	[76,062]	[48,612]	[66,502]
2004-11	0.073	0.012	–0.079	–0.050	0.003	–0.001
	(8.86)	(1.44)	(2.00)	(2.08)	(1.16)	(0.32)
	[146,973]	[136,392]	[66,176]	[89,523]	[58,203]	[78,457]

b) Workers in small firms (up to 50 employees)

	P(Entry)	P(Exit)	ln H^{new}	ln H^{remain}	ln W^{new}	ln W^{remain}
Blue-collars						
2004–07	0.013	0.013	0.050	0.015	0.001	0.004
	(0.74)	(0.78)	(0.88)	(0.46)	(0.19)	(1.03)
	[26,934]	[114,627]	[10,105]	[16,424]	[9,901]	[16,107]
2004–08	0.015	–0.001	0.025	0.002	0.005	0.005
	(1.08)	(0.10)	(0.55)	(0.08)	(0.87)	(1.31)
	[33,681]	[34,405]	[12,730]	[20,467]	[12,460]	[20,089]
2004–09	0.010	0.000	–0.029	–0.016	0.008	0.006
	(0.85)	(0.04)	(0.73)	(0.66)	(1.67)	(1.94)
	[40,899]	[41,640]	[15,177]	[25,162]	[14,836]	[24,686]
2004-10	0.009	–0.002	–0.039	–0.025	0.009	0.007
	(0.77)	(0.14)	(1.06)	(1.04)	(2.01)	(2.13)
	[48,346]	[48,901]	[17,738]	[29,942]	[17,284]	[29,308]
2004-11	0.009	–0.007	–0.039	–0.036	0.010	0.007
	(0.82)	(0.65)	(1.14)	(1.52)	(2.19)	(2.24)
	[56,175]	[56,787]	[20,854]	[34,816]	[20,040]	[34,056]
White-collars						
2004–07	0.022	0.069	0.015	–0.000	–0.021	0.024
	(0.58)	(2.08)	(0.58)	(0.02)	(1.00)	(1.67)
	[4,429]	[4,852]	[1,605]	[2,772]	[1,558]	[2,743]
2004–08	0.015	0.003	0.019	0.013	–0.019	0.024
	(0.52)	(0.11)	(0.93)	(0.65)	(1.06)	(1.86)
	[5,544]	[5,940]	[2,057]	[3,434]	(2,007)	[3,399]
2004–09	–0.006	0.042	0.007	0.017	–0.027	0.025
	(0.23)	(2.57)	(0.35)	(0.74)	(1.73)	(2.04)
	[6,700]	[7,250]	[2,412]	[4,225]	[2,358]	[4,186]
2004-10	–0.002	0.008	0.002	0.007	–0.024	0.015
	(0.11)	(0.39)	(0.10)	(0.30)	(1.65)	(1.25)
	[7,709]	[8,406]	[2,776]	[4,856]	[2,720]	[4,815]
2004-11	–0.003	–0.002	0.004	0.005	–0.031	0.007
	(0.15)	(0.12)	(0.20)	(0.21)	(2.13)	(0.62)
	[8,653]	[9,393]	[3,141]	[5,430]	[3,081]	[5,388]

Table 7. Treated age group: 19–25. Control age group: 27–29. Placebo tests

	Placebo reform t=2003		Placebo reform t=2004		Placebo reform t=2005	
	P(Entry)	P(Exit)	P(Entry)	P(Exit)	P(Entry)	P(Exit)
Blue-collars						
t–3 – t	–0.041	–0.038	–0.042	–0.032	–0.020	–0.044
	(4.27)	(3.97)	(4.02)	(2.85)	(2.01)	(5.25)
	[98,164]	[93,258]	[99,741]	[97,327]	[104,810]	[100,541]
t–3 – t +1	–0.052	–0.043	–0.038	–0.045	–0.009	–0.048
	(6.11)	(5.01)	(4.40)	(5.83)	(1.24)	(6.31)
	[123,084]	[118,317]	[128,019]	[124,548]	[136,353]	[130,072]
t–3 – t +2	–0.053	–0.053	–0.029	–0.052		
	(6.79)	(7.33)	(4.22)	(7.40)		
	[151,362]	[145,538]	[159,562]	[154,079]		
t–3 – t +3	–0.047	–0.059				
	(6.91)	(8.69)				
	[182,905]	[175,069]				
White-collars						
t–3 – t	–0.008	0.031	0.000	–0.018	0.007	–0.031
	(0.45)	(2.05)	(0.00)	(1.21)	(0.39)	(1.88)
	[19,774]	[19,791]	[19,331]	[20,114]	[19,377]	[20,327]
t–3 – t +1	–0.011	0.012	0.003	–0.026	0.028	–0.032
	(0.72)	(0.99)	(0.19)	(2.11)	(1.98)	(2.17)
	[24,389]	[24,926]	[24,281]	[25,384]	[24,729]	[25,432]
t–3 – t +2	–0.010	–0.000	0.018	–0.029		
	(0.72)	(0.01)	(1.37)	(2.50)		
	[29,339]	[30,196]	[29,633]	[30,489]		
t–3 – t +3	0.000	–0.007				
	(0.02)	(0.63)				
	[34,691]	[35,301]				