Employment Protection Reform, Enforcement in Collective Agreements and Worker Flows[#]

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Abstract

In this paper, we analyse a reform of notice periods for employer-initiated separations in Sweden. The reform was aimed at encouraging the hiring of older workers by reducing the notice periods for newly hired older workers. The reform implied minor or no changes in the notices for younger workers and was initiated at different times for various industries. These circumstances provide ample opportunity for the identification of its effects. Using detailed matched employer-employee data, we apply a difference-in-difference approach to examine how hirings and separations were affected by the change in the employment protection legislation. Our findings on worker flows indicate heterogeneous effects across different industrial sectors. A salient feature of the results is that the estimated effects increase with the treatment dose (i.e., the size of the reduction, measured in months, of the notice across different age groups). Our analysis also provides new insights into the implementation of employment protection legislation in collective agreements, which has received little attention in the literature previously, possibly due to the lack of data.

Keywords: Employment protection; Hirings; Separations; Collective agreements; Matched employer-employee data
JEL Classification: J23; J63; J14; J52; K31

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1. Introduction

Partial employment protection reforms have been introduced in many countries to improve the labour market prospects for older workers. In Sweden, legislative changes in the Employment Protection Act (EPA) in 1997 reduced the firing costs for employees aged 45 and over with the explicit purpose of increasing the job-finding probabilities and lowering unemployment among older workers. The pre-reform rules regarding the terms of notice for employer-initiated separations were based on the *age* of the employee, whereas the new rules are based on *tenure*. For newly hired, older workers, the reform implied a shorter notice period. The change was substantial for the oldest age group: a reduction in notice from six months to one month, equivalent to 42 percent (5/12) of an annual salary. For younger employees, smaller or no reductions were introduced. Partial reforms in other countries with similar aims have typically used layoff taxes as the policy instrument, with higher taxes imposed on firms dismissing older workers.

An important feature of employment protection legislation in Sweden is that it is optional, implying that parts of the legislation can be undone in collective agreements between employers and trade unions. Compared to other countries, legislation in Sweden seems to be particularly far-reaching in this respect. The possibility to deviate from the legislation through collective agreements applies to rules regarding notice, which means that it cannot be taken for granted that legislative changes in the EPA gain legal force in all sectors of the labour market.

The EPA reform in 1997 has several attractive properties that make it suitable for evaluating the effects of employment protection. First, the reform was targeted towards older workers, so younger workers can be used as controls. Our prediction, in line with conventional theory, is that shorter notices of termination increase the probability of hiring among older workers. Second, because notice periods were reduced by varying degrees among workers aged 25 – 44, ranging from 1 to 4 months, and not reduced at all among the youngest (aged 18 – 24), a stronger test of the hypothesis is possible. We expect the probability of hiring to increase monotonically with the treatment dose. Third, the possibility for employers and unions to opt out of the legislation provides another source of identification on which we have collected extensive data. In some collective agreements, the changes in the EPA were implemented with a delay of several years, so some of the older workers were not subject to treatment in 1997 and thus form an additional suitable control group. Our hypothesis is that stricter enforcement of the legislation entails stronger effects on hirings.

Conventional theory not only predicts that hirings will increase with shorter notices, but also that separations should increase. We therefore tested for the effect on separations, using the same methodology as described above. The theoretical prediction regarding employment is ambiguous. To the extent that hirings increase more than firings, employment increases. However, the EPA reform was designed in such a way that only workers recruited after 1997 were subject to the new rules regarding notice periods. The old rules continue to apply to workers hired before 1997 that have remained with the firm. Because the remaining workers were not subject to the reductions in notice, we expect the reform to have a larger effect on hirings than on separations, at least in the short run.

This paper contributes to the literature in two ways. First, the effects of employment protection reforms targeted towards older workers are better understood. Second, new insights are provided about the "black box" of employment protection, i.e., to what extent different degrees of enforcement influence the effects as measured by implementation in collective agreements. To the best of our knowledge, this paper is the first study to use extensive and detailed information on enforcement across collective agreements to examine the effects of employment protection legislation.

In contrast to the EPA reform, the idea behind many partial employment protection reforms in other countries has been to make it more difficult for firms to dismiss workers in the protected group. For France, Behaghel, Crépon and Sedillot (2008) find perverse effects of layoff taxes. The hiring probability for older workers was reduced, and no benefits in the form of fewer dismissals were achieved. Similar results have been demonstrated by Acemoglu and Angrist (2001) concerning legislation in the United States aimed at protecting disabled workers. In their analysis of layoff taxes for the elderly in Austria, Schnalzenberger and Winter-Ebmer (2009) obtained more encouraging results: the policy reduced layoffs among the targeted group, but these effects were thwarted by the substitution of younger workers for older ones at large firms.

Differential enforcement of employment protection legislation comes in two different guises. The first type results from the explicit design of the legislation, while the second is not defined by the letter of the law. The former typically stipulates differential treatment across groups of workers or types of firms. For example, in several countries, employment protection legislation is less stringent in small firms. This particular feature of the legislation provides suitable control groups (e.g., firms below or above the size threshold), which have been exploited in recent within-country studies of partial reforms (see the extensive surveys of employment protection research by Skedinger, 2010a and 2010b). These studies add to the growing evidence that more stringent legislation reduces job and worker flow and examine a variety of other outcomes.[1]

The other guise of differential enforcement is more elusive in its character but not necessarily of lesser importance for the effects of the legislation. Few studies have examined the effects of this type of differential enforcement of employment protection legislation, presumably due to the lack of data. Similar in spirit to our study, Fraisse, Kramarz and Prost (2009) and Okudaira (2008) exploited the variations in judicial discretion across regions. Fraisse et al. (2009) found that increases in the judge density in France act as a threat to employers by encouraging their compliance to labour regulation, and Okudaira (2008) considered the assignment of judges to Japanese prefectures. Their conclusion is that enforcement matters. When legislation is applied in a more stringent way (i.e., the exogenous allocation of more judges), its effects on labour market outcomes, such as employment and job flows, are stronger. Our study incorporates aspects of both types of differential enforcement as we combine the evaluation of a partial reform targeted towards a specific age group with information on the more difficult-to-observe enforcement of the same reform in collective agreements.

Our data on hirings and separations originate from a large register-based matched employer-employee data set from Statistics Sweden covering the period 1990 – 2005. For each individual in the data set, we observe notices (measured in months) based on information from the relevant collective agreements and the EPA (if applicable). The notice is based on the age and/or tenure, depending on the period and the collective

[1] For studies on job and worker flows, see, for example, von Below and Skogman Thoursie (2010), Kugler and Pica (2008), Martins (2009) and Schivardi and Torrini (2008).

agreement, which differs for manual and non-manual workers in the same industry. We also have ample information on other individual and firm variables likely to influence worker turnover.

2. Labour Market Reforms in 1997

In this section, we discuss notice reform in the EPA and its implementation in collective agreements and calculate the consequences for the expected discounted firing costs. We also describe other labour market reforms in 1997 that could impinge on our results.

2.1 The Notice Reform and Its Implementation in Collective Agreements

Comprehensive legislation regarding employment protection in Sweden was introduced in the Employment Protection Act (EPA) of 1974. Some legislation in this field had been in place before 1974, specifically targeting older workers. In 1971, special rules were introduced regarding the notices for employees aged 45 or older (SOU 1973:7). Before the new legislations in 1971 and 1974, the legal system of employment protection was based almost exclusively on collective agreements between employers and trade unions and the application of case law. The rules regarding notices in the EPA were based on the age of the employee and not on tenure (as was the case in most collective agreements at the time). The legislators argued that older workers needed special protection. Rules based on tenure were seen as detrimental to labour mobility in this group because seniority capital would be lost for workers changing jobs (Regeringens proposition, 1973:129). According to the EPA, layoffs should be based on the last-in-first-out principle, and the seniority capital accumulated by older workers cannot be transferred to a new employer.

By 1996, the views among legislators regarding the notices for older workers had changed completely. Age-based notices were considered counter-productive for older workers. Shorter notices, it was argued, would increase the propensity for employers to hire older workers (Regeringens proposition, 1996/97:16). One reason behind the change of focus from labour mobility to new hirings was the rise in unemployment since 1974.

Figure 1 shows the diverging patterns of unemployment for older and younger persons in the years preceding the reform. In the wake of the economic crisis of the early 1990s, unemployment rates started to decline for the young in 1993, whereas rates continued to increase for older persons up to 1997.

- Figure 1 about here -

The new bill was presented to the Swedish Parliament (*Riksdag*) on October 24, 1996. The reform of the notice periods gained legal force on January 1, 1997. The full details of the reform are presented in Table 1. The pre-reform rules were based on the age of the employee, with a scale starting from a one-month notice for employees younger than 25 and ending with a maximum of a six-month notice for employees aged 45 or older. Rules in the new legislation were based on tenure. Employees with tenure shorter than two years were entitled to a notice of one month, while the rules stipulated up to six months of notice for employees with a tenure of at least 10 years. No changes in the notice period for employee-initiated separations, which is one month, were enacted in the reform.

- Table 1 about here -

A long period of transition occurred before the law became effective for all workers. The old rules continued to apply to workers employed with the same firm after January 1, 1997, to the extent that these rules were more favourable for the worker. Because the EPA was optional, collective agreements between employers and trade unions could deviate from the EPA. Thus, only newly hired workers were initially affected by the legal changes and only in certain areas of the labour market because the specific implementation could vary, depending on the agreement. Hence we expect to find larger effects on hirings than on separations following the reform. The EPA reform could potentially induce substitution across age groups (as was indeed found for the Austrian layoff tax in the study by Schnalzenberger and Winter-Ebmer, 2009). However, the Swedish reform implied only small differences in notice across close age groups – one month – so the employers' gain from substitution should be limited.

Table 2 provides an overview of the implementation of the new legislation regarding notices in the collective agreements (more details can be found in the Appendix, Table A.1.).[2] We consider agreements for both manual and non-manual workers in engineering, construction and retail. These agreements are among the largest agreements in the labour market.

- Table 2 about here -

Before the reform of the legislation, two agreements followed the rules according to the law rather closely, namely those for manual workers in engineering and retail. The other agreements set up their own rules, based on the age of the employee (manual construction workers) or on a combination of age and tenure (non-manuals in engineering and construction). The changes in the EPA in 1997 were implemented in all of the agreements but at different points in time. In the Engineering agreement for manual workers the rules were implemented on January 1, 1997, while they were adopted with a delay of up to four years in other agreements. For non-manual workers in engineering, the rules were implemented on February 1, 2001. Manual workers in construction are covered by three agreements, with new rules introduced during 2000 and 2001. Workers in retail are covered by a substantial number of different agreements, depending on the type of products or services sold; however, for manual workers in this sector, almost all of the agreements specify the same rules regarding notices. The reform date for these workers was July 1, 2001. The rules for non-manual workers are much more heterogeneous in this respect and have not been included in the table.

The interpretation of the effects of changes in the EPA in concurrent collective agreements is a potentially complex legal issue. These agreements either supplanted the legislation (while implementing it later on) or implemented it immediately as the reform

[2] Individual agreements are also allowed on the condition that the periods of notice are longer than those specified by law and that the agreement is not in conflict with a collective agreement that the employer and the employee are bound by.

gained legal force. Thus, the interpretation of the enforcement in the collective agreements seems straightforward.[3]

Table 2 shows that the changes in the EPA in 1997 have had an important influence on collective agreements. Presumably, the arguments put forward in favour of the new legislation have gained acceptance among employers and trade unions. This observation begs the question why collective agreements were so diverse with respect to notices before the reforms. We do not try to answer this question in the paper. Instead, we take the diversity for granted and use this source of variation to identify the effects of employment protection legislation. Another question relates to the differences in the timing of reform implementation across agreements. The age structure among employees in retail (with many young workers) could have made it less pressing to implement the reform in that sector, but the slow implementation for non-manuals in engineering (with considerably older workers on average) does not fit this explanation.[4]

For workers in firms not covered by the collective agreements, the EPA applies. In Sweden, approximately 90 percent of employees and virtually all larger firms are covered by such agreements.

In principle, workers and employers may anticipate employment protection reform and adjust their behaviour accordingly in various ways. Prior to this reform, older workers could seek employment in other firms before January 1, 1997 to take advantage of the provisionary rules requiring longer notices for hirings that occurred before that date. Employers could potentially gain by dismissing relatively young workers with long tenure earlier, before the reform. However, because most young workers have short tenure and long-tenured workers are protected by the seniority rules in the EPA, employers had limited benefits from anticipating the reform. The reform was likely not anticipated in a way that could affect our results because the bill was presented to the Riksdag on October 24, 1996, and it was uncertain until October whether the major trade union for manual workers, LO, would be able to successfully block the reform, according to newspaper accounts at the time (see Sedvallson, 1996). In the unlikely event that the

[3] Legal interpretation of changes in EPA seems to be uncertain in concurrent agreements stating that the rules are "complements to the EPA" (see the discussion in the annual report of the National Mediation Office, 2009). There are no such formulations in the agreements considered by us.

[4] Table A.3 in the Appendix presents descriptive statistics on age and other characteristics for workers in different collective agreements.

1997 reform was anticipated, our estimated effects of the reform on hirings and separations would be biased downwards. We regard the risk of anticipation to be larger for the subsequent implementations of the reform for non-manual workers in engineering and in construction and retail.

2.2 Consequences of the Notice Reform for Expected Discounted Firing Costs

To estimate the expected discounted costs due to the reform, we constructed an index based on the formula proposed by Heckman and Pagés-Serra (2000). This index measures the expected dismissal costs at the time a worker is hired:

$$Index_{j,s,t} = \sum_{i=1}^{T} \beta^i \, \delta_j^{i-1}(1-\delta_j)\left(b_{j,s,t+i} + aS_{s,t+1}^{jc} + (1-a)S_{s,t+1}^{uc}\right)$$

where j denotes the collective agreement that the worker is bound by, s is the age group of the worker, t is the time period (years), T is the maximum tenure of a worker, β is the discount factor, δ is the probability of not being laid off, b is the advance notice (in months), a is (in our interpretation[5]) the probability that a dismissal is judged to be with just cause should the case be opened in court, S^{jc} is the severance pay associated with dismissals with just cause, and S^{uc} is the severance pay in case a court rules that the dismissal is without just cause.

Like Heckman and Pagés-Serra (2000), we assume a discount rate of 8 percent and a common layoff rate across agreements of 12 percent, which implies a value of 0.88 for δ. Later, we relax the assumption of a common layoff rate. Using the industry-specific rates for the United States, we assign δ values of 0.84 for engineering, 0.80 for retail and

[5] This differs from the interpretation of Heckman and Pagés-Serra (2000) in which a is the probability that the economic difficulties of the firm are considered as a justified cause for dismissal. Because this situation is always the case in Sweden and the associated severance pay (S^{jc}) is zero, implying that this term drops out of the equation, we have chosen an interpretation more relevant to the specific Swedish context.

0.56 for construction.[6] These figures reflect the turnover in the absence of (stringent) employment protection legislation. The minimum tenure with a firm is assumed to be 1 year, and the maximum 20 years. The severance pay associated with dismissals with just cause is zero in Sweden; therefore, the second term within the second parenthesis drops out. The severance pay for dismissals without just cause (denoted as damages awarded to the employee in the EPA) is defined in monthly salaries by the EPA and is dependent on tenure and age (see Lunning and Toijer, 2006).[7] The probability of a dismissal being judged as unjust is initially assumed to be zero and then arbitrarily set to 1 percent.

In Table 3, the expected discounted costs of a dismissal are defined before and after the reform as well as for two types of worker: one hired at age 20 and another at age 45. Notice is defined according to the respective agreements and the EPA (see Appendix and Table 1). The simple differences between the pre- and post-reform costs are calculated for each worker type and can be interpreted as the individual reform effect for the two types. The difference-in-difference (d-i-d) in the table refers to the above difference for a 45-year-old worker versus the difference for a 20-year-old worker and can be viewed as the relative reform effect across age groups.

- Table 3 about here -

Table 3 reveals that the firing costs for both older workers and for younger workers were affected by the reform. However, the reform effect is modest for the 20-year-olds, amounting to a cost increase of about 0.3 – 0.7 of a monthly salary, depending

[6] The figures, based on data from the Bureau of Labor Statistics, are averages for both manual and non-manual workers over the period 2001-05. Data were not available for engineering, so the figures for the manufacturing of durable goods have been used for this industry.

[7] For workers below the age of 60, the so-called "special damages" for the case of a dismissal without just cause is 16 months of salary for workers with less than 5 years tenure, 24 months of salary for workers with at least 5 but less than 10 years tenure and 32 months of salary for workers with at least 10 years tenure. In addition, two age-dependent rules apply for special damages. First, for workers above the age of 60, the corresponding damages are equivalent to 24, 36 and 48 months of salaries, respectively. Secondly, workers at least 45 years of age are entitled to count each month of employment past that age as 2 months, up to a maximum of 60 such extra months. This rule also applies to workers above the age of 60 who are entitled to higher damages. Damages, in monthly salaries, cannot exceed the number of months employed unless the worker has been employed for less than six months in which case the damages amount to 6 months of salary. The court can also award "general damages" to the employee as compensation for psychological costs and non-payment of salary. General damages are not included in our computation of severance pay without just cause.

on the assumed parameter values. For the 45-year-olds, a substantial decrease in costs, ranging between 1.9 and 3.9 monthly salaries, was found. The difference-in-difference between the two types of worker thus varies between –2.6 and –4.2. The difference-in-difference is larger (in absolute value) in the construction sector; otherwise, the variation across agreements and industries is small. The values indicate that the reform had a significant impact on the expected discounted firing costs for older workers in relation to young workers. The cost-decreasing effect amounts to 22 – 35 percent of an annual salary, depending on the agreement.

The effect of a higher layoff rate on the total expected costs is ambiguous. In industries with a relatively high layoff rate (e.g., construction), the expected firing costs are higher in the short term. However, because the probability that a worker remains employed also decreases faster, the short-term firing costs are countered by lower expected costs in the longer run. To the extent that firing costs increase with age and tenure, the long-term cost-reducing effect is strengthened. Table 3 shows that the introduction of severance pay for dismissals without just cause in the index does not affect any of the reform effects because the rules regarding severance pay have remained unchanged in the pre- and post-reform periods.

2.3 Other Labour Market Reforms and Collectively Agreed Employment Protection Schemes

Two partial reforms of particular importance to labour market outcomes for older workers were introduced on January 1, 1997. One reform concerned stricter rules for the eligibility of disability insurance for 60 - 64-year-olds. Labour market reasons (in combination with medical reasons) for granting pensions were no longer allowed for this group. As pointed out by Saint-Paul (2009), an increase in the retirement age could increase hirings among some older workers because hiring costs at a given age are spread over a longer employment period. The other reform was part of the legislative changes in the EPA and made it easier for firms to deviate from seniority rules (last-in-first-out) when dismissing older workers due to a lack of work. The new law stipulated that firm-level agreements between employers and trade unions that exempt workers older than

57.5 years from the seniority rules no longer needed approval by a union at the central level. To check the sensitivity of our results to these reforms that could affect worker flows for workers between the ages of 57 and 64, we estimated separate regressions for the age group 45 - 54, the members of which were the primary target group of the 1997 EPA reform but were not subject to the other reforms.

Special employment protection schemes (*omställningsavtal*) in case of layoffs have also been collectively negotiated on a broader scale for manual and non-manual workers in the private and public sectors (Andersson, Fölster and Skedinger, 2002, Martinson, 2005). In relation to the EPA and the industry-specific collective agreements, these schemes imply longer notices and the possibilities to deviate from seniority rules. Another important difference regards financing. The schemes are financed through insurance fees, payable by firms with membership in the relevant employer organisation. Unlike the costs associated with notices, risk pooling exists across firms. In 2005, seven so-called job security councils (*Trygghetsråd*) administered the schemes. Two of the councils cover workers in the collective agreements considered in our study, *Trygghetsfonden TSL*, founded in 2004 for manual workers in the private sector, and *TRR Trygghetsrådet* for non-manuals in the same sector, established in 1974. Because these firing costs are borne by the councils and not by the individual employer, the age profiles of hirings and separations should not be affected by the special employment protection schemes.[8]

3. Data

Our study is based on a matched employer-employee database from Statistics Sweden that contains detailed data on Swedish firms and establishments linked with a large sample of individuals covering the period 1990 - 2005. Individual, plant and firm-level-

[8] Benefits in both schemes are available for workers dismissed due to the lack of work and are dependent on age and tenure. Non-manual workers receiving notices are entitled to counselling and coaching during the notice period. When the notice period has ended, the councils provide monetary compensation in the form of severance pay (manual workers) or supplements to unemployment insurance benefits (non-manual workers).

based data are linked together with unique tracking numbers. Information on the implementation of notice periods at the industry level is added from the collective agreements for manual and non-manual workers. In the empirical part of the paper, we focus on hirings and separations in two large industries: engineering and retail.[9] In each of these sectors, we distinguish between different collective agreements covering the different types of workers within each industry.

The database consists of the following parts. First, the individual data contain individual wage statistics based on Statistics Sweden's annual salary surveys, supplemented by information from a series of data registers. The dataset encompasses information on more than two million individuals for the period 1990 - 2005 (accounting for roughly 50 percent of the labour force) and contains information on workers' wages, education, work hours, occupation codes, sector codes, demographic data, etc. Second, the financial statistics (FS) include detailed firm-level data. The included variables are value added, capital stock (book value), number of employees, wage bill, ownership status, profits, sales and industry affiliation.

Finally, the plant-level data contain detailed information at the plant level, such as employee demographics, salaries, education and codes for company mergers, closures, formations and operational changes.

For each individual in the data set, we observe the notice period (in months), based on information in the relevant collective agreement and in the EPA (if applicable). The notice period is based on age and/or tenure, depending on the period and the collective agreement, which differs for manual and non-manual workers in the same industry.

Our main focus is hirings and separations. The construction of the indicator variable for new hirings is based on workers that are newly employed in a firm that is present in the data the year before. This condition is imposed to reduce the risk of classifying a worker as being newly employed due to the firm being new in the data set, although not being a start-up. Similarly, we define a worker separation as observing an individual at time t who is not present in the same firm at time $t+1$, although the firm is continuing at $t+1$.

[9] Construction is not included due to the lack of data of sufficient quality in that sector.

Another restriction is that we do not include firms with very large changes in the observed number of employees between two consecutive time periods.[10] We exclude these large-scale hirings and separations from the empirical analysis because we do not want extraordinary events, such as massive layoffs or company mergers to interfere with the results. Our restrictions imply that the analysis will be based on continuing firms and will not take into account the impact of firm start-ups and firm closures.

The variable definitions and descriptive statistics are presented in Tables A.2 and A.3 in the Appendix. In Table A.3, the means and standard deviations are shown for the three different samples of individuals: manuals in engineering, non-manuals in engineering and manuals in retail. Workers aged 45 - 64 represent between 33 and 42 percent of the workforce in engineering but only one-fifth of the workforce in retail. Turnover, measured across all age groups, is also markedly higher in retail than in engineering.

Figures 2 and 3 depict hirings and separations by age groups in engineering and retail. Worker flows are highly cyclical, especially for hirings and among young workers and manuals. The year preceding the reform is indicated by a vertical line. Retail firms implemented the reform for manual workers rather late in the year (July); therefore, the year 2001 is likely to reflect the effects of both regimes. For this reason, we eliminated the year 2001 from the data in the regressions for these workers and use 2002 as the start of the post-reform period.

- Figures 2 and 3 about here -

The figures highlight the tradeoff that we confront when analysing the effects of the reform. On the one hand, the differential in the treatment dose is largest when comparing outcomes for the 45 – 64-year-olds to those of the youngest age group, 18 – 24. This observation supports using the youngest group as the control. On the other hand, because cyclicality in worker flows is more pronounced among the young, the older age

[10] More specifically, in our analysis on hirings, we omit firms with a hiring rate above 30 percent in combination with those having more than 100 new employees. For separations, we omit firms with (i) a separation rate above 30 percent in combination with observing 100 fewer employees or (ii) a separation rate above 50 percent. The results, available on request, are not qualitatively affected by these restrictions.

groups are more similar to the 45 – 64-year-olds in this respect. Also, the pre-reform trends are more similar, which is an argument for using 40 – 44-year-olds as the control. We have chosen to focus our analysis on using 18 – 24-year-olds as the control, but we also test the sensitivity of our estimates to using several other age groups.

4. Econometric framework

Our empirical strategy is to use a difference-in-difference approach to compare changes in worker turnover before and after the change in EPA. Ideally we would like to compare the outcomes before and after the policy change for a group affected by the change (the treatment group) to a group not affected (the control group). These two groups are represented by the oldest age group (45 - 64) and the youngest (18 - 24). Our main analysis is based on comparing worker turnover for these two age groups. In between these two age groups, other age groups are to a varying degree affected by the reform, as discussed in Section 2.1. These age groups, characterised by different treatment doses, are also studied.[11]

Based on our panel of individuals and firms, we estimate the following regression:

$$y_{it} = \alpha_0 + \alpha_1 Treated\_Age\_Group_{it} + \alpha_2 Post_t + \alpha_3 (Treated\_Age\_Group * Post)_{it} \\ + x'_{it}\beta + z'_{it}\delta + \varepsilon_{it}$$

Our dependent variable in the analysis on hirings is an indicator variable equal to one if a worker is newly hired at time *t* and zero otherwise. In regressions on separation behaviour, the dependent variable equals one if an individual is separating from a firm at time *t* 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

[11] Because the computation of expected discounted firing costs (in Section 2.2) showed that the effect was negligible for 18 - 24-year-olds and that reform effects also differed little across agreements, we do not operationalise the treatments with these costs.

*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 the EPA relative to the (basically) unaffected youngest age group. 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 the individual's education, the log of the number of employees, the capital-labour ratio, the value added per employee, the share of females and the share of employees with post-secondary education. The additional explanatory variables control for the observable differences between the two groups, which means that they account for the possibility that characteristics are systematically different before and after the policy change (compositional bias). All of the estimations include fixed time, sub-industry and regional effects in which, for instance, the time fixed-effects control for common shocks to the business cycle.

A crucial assumption behind the d-i-d estimator is the parallel trend assumption. One method to check for parallel trends 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 policy changes. If the placebo estimators are statistically significant, then there is a risk that the estimated d-i-d coefficients are biased. As a check for robustness, we estimate a large number of different placebo regressions. The nature of the reforms during the notice periods makes it possible to apply placebo regressions in two different ways. First, we estimate placebo regressions based on prior non-reform years. Second, the different timing of the reforms in different industries allows us to estimate placebo regressions on combinations of industries and collective agreements that have not undertaken any reforms.

The manner in which the standard errors should be clustered to account for autocorrelation and within-firm or within-individual correlations is not obvious (see Bertrand et al., 2004). Several alternatives are possible with our data on individuals and firms. These options include clustering at the worker, age group or firm levels. We have chosen to use the most conservative alternative, which is clustering at the firm level.[12]

Another issue related to the d-i-d approach is that the composition of the groups of treated and untreated individuals should be stable over time. The notice reform can influence worker turnover by changing the behaviour of the existing workforce (incentive effect) and/or by changing the composition of workers (compositional effect). The risk of a composition bias is mitigated in our case because we use individual data in which we can follow workers before and after the reforms.

One potential concern with the estimation of the regression equation relates to omitted variables bias. To account for the impact of other individual and firm characteristics that might influence results, we estimate specifications with a large number of alternative control variables. These variables include controls for the share of 45 - 64-year-olds at the firm level and the share of manual workers. Both of these variables are used to account for the possibility that the impact of the change in notice period is related to the composition of the firms' work force.

Year dummies are included in the regressions as a control for business cycle effects that are common to all employees. This measure may be too crude if macroeconomic conditions have differential effects across age groups. We therefore estimate alternative regressions in which we add age-specific unemployment and an interaction term between age groups and gross domestic product (GDP) growth to the specifications.

We also estimate more flexible empirical specifications, allowing the treated age group to be on a different trend than other age groups.[13] When allowing for different trends, we include an interaction term between the treated age group and a time trend.

Finally, reverse causality is a potential problem in studies on the effects of employment protection. One obvious possibility is that both legislative changes in the EPA and their implementation in collective agreements are triggered by deteriorating labour market conditions for older workers. Regarding rules for notice with the exception

[12] Clustering at the age group or at the individual levels implies *t*-values that are between 200 and 400 percent higher, respectively. Hence, the reform effects in many cases become significant.

[13] This robustness check is similar to that undertaken by Bertrand and Mullainathan (1999). See also Angrist and Pischke (2009).

of the Engineering agreement for manuals, our collective agreements are constructed in such a way that legislative changes have no effect during the agreement period, which is typically 2 – 3 years. Moreover, because deviations from notice legislation are negotiated mainly at the industry level and only rarely at the firm level, decisions by firms regarding hirings and separations can be assumed to be exogenous. Another advantage with our data is that treatment is based on age, an individual characteristic that is exogenous.[14]

5. Results

The results for hirings and separations in engineering are presented in Tables 4 (manuals) and 5 (non-manuals), and the corresponding results for retail are shown in Table 6 (manuals). Each table contains three panels with regressions for different lengths of the post-reform periods to examine whether the effects of the reforms differ in the short and the long term. All of the regressions are based on the same length of the pre-reform period, namely three years. Panel a) considers a post-reform period of one year, panel b) two years and panel c) three years. In each panel, more controls are successively added to the regressions – individual- and firm-specific controls and firm fixed-effects. The first four columns in each table display the results for hirings, and columns 5–8 relate to separations.

Columns 1 and 5 report the regressions for the simplest specification, with dummies for the variables for age 45 – 64, the post-reform period and the reform effect (the interaction between treatment group and post-reform period) plus (non-reported) dummies for region and year. In columns 2 and 6, individual and firm-specific controls, commonly included in similar analyses and that could impinge on turnover, are added. Columns 3 and 7 restrict the sample to a balanced firm panel, and the final specifications (4 and 8) introduce firm fixed-effects. The latter specifications control for unobserved, firm-specific and time-invariant factors that contribute to turnover, such as working environment. Of main interest is the coefficient for the reform effect and its sensitivity to

[14] This differs from studies of partial reforms based on firm size (discussed in Section 1) in which the exogeneity assumption may be questioned.

different specifications. The reform effect for 45 – 64-year-olds is based on 18 – 24-year-olds as the control in Tables 4 – 6, but later we will examine the sensitivity of the results to the use of other age groups as controls.

- Table 4 about here -

Table 4 reveals that the coefficient for the reform variable is 0.117 in the first column of panel a) (a one-year post-reform period). Taken at face value, this estimate means that the 1997 reform implied a short-run increase in the hiring rate of 11.7 percentage points among workers aged 45 – 64 in relation to the rate among 18 – 24-year-olds. As more controls are added, the estimate increases slightly to 0.128 (column 2). Columns 3 and 4 show estimates on a balanced firm panel. Based on this smaller sub-sample of individuals and firms, the reform effect is approximately 0.14.

In the longer term, the effect on hirings wanes out and becomes insignificant, as seen in panels b) and c) in which the post-reform periods are two and three years, respectively.

On the whole, not much seems to happen to separations for manual workers in engineering. As more controls are added, all of the estimates are statistically insignificant and of small magnitude. As expected, the dummy for age 45 – 64 is negative in all of the regressions, reflecting less turnover among older workers. Table 4 indicates that there was an initial but short-lived response in the form of more hirings without any increase in separations.

What about non-manual workers in engineering? Here, the regressions in panel a) of Table 5 indicate an initial effect of the 2001 reform on hirings of about 0.09. Again, the effect is reduced in size as the post-reform period is extended and is eventually insignificant in panel c) in which the estimations are based on a balanced panel of firms. As for separations, the initial estimate is 0.032 (in column 8), and this increase is sustained as the evaluation period increases. Thus, the net gain in employment for older workers was initially smaller for non-manuals than for manuals.

- Table 5 about here -

The pattern of hirings is different in retail (Table 6). After an initial response of approximately 0.06, the effect of the reform in 2001 is strengthened over time, up to about 0.09 in panel c). However, the contemporaneous effect on separations is somewhat larger throughout, which means that there is a marked increase in the turnover but hardly any net increase in the employment among manuals in retail.

- Table 6 about here -

To check the robustness of the estimates in Tables 4 – 6, several additional regressions were run. One concern with the estimates is that 18 – 24-year-olds are a group of workers that are potentially different from 45 – 64-year-olds, in terms of unobserved worker and job characteristics, despite being the group least affected by the reforms and thus not suitable as a control. Another concern is that those in the age group 60 – 64 were affected by the stricter rules for eligibility of disability insurance in 1997 and that a reform in the same year made it easier for firms to deviate from seniority rules when dismissing workers older than 57.5 years due to lack of work (discussed in Section 2.3). In addition, using a treatment group that is close to retirement age may be problematic. For these reasons, we experimented with using various age groups as control groups in response to the first concern and restricting the treatment group to the age interval 45 – 59 as a way to handle the second and third concerns.

The robustness checks for manuals in engineering are displayed in Table 7. We contrast the results to a benchmark in the form of the specifications in column 2 of Table 4 for hirings and column 6 for separations. These benchmarks are the preferred estimates because the firm panels entail a substantial loss of observations between 25 and 38 percent, depending on the industry and worker category, in the specifications with the longest post-reform period.

- Table 7 about here -

The estimated reform effects decrease in size as successively older control groups are used, which is unsurprising because the treatment dose gets closer to the one administered to the 45 – 64-year-olds. In most cases, the coefficients remain significant. When the treatment group is restricted to 45 – 59-year-olds, the results are basically unchanged. Another concern derives from the fact that the EPA reform in 1997 affected all of the firms not bound by a collective agreement, regardless of industry. Hence, the effects of the collectively negotiated reforms in 2001 may be underestimated because some firms in the industry had already been treated in 1997. This condition is of no concern for manuals in engineering for which the EPA reform was implemented immediately, but the results for the other groups under consideration could be affected. We have no direct information as to whether a firm is covered by collective agreement, but non-coverage is more prevalent among small firms and virtually absent in the largest firms. If this aspect is important, we expect to find smaller effects in the smallest firms following the 2001 reforms. The results could differ depending on firm size for other reasons as well, but in the opposite direction. To the extent that it is more costly for small firms to adapt to employment protection regulation due to, for example, its fixed costs being spread over fewer employees, the estimated effects of the reforms may be larger in these firms. Thus, we checked for robustness in this respect by running regressions for various subgroups of firm sizes. For hirings, the results suggest a U-shaped relationship between firm size and the reform effects, although the coefficients in the smallest firms are estimated with low precision. For separations, firm size seems to be of little relevance.

Robustness with regard to gender was examined by running separate regressions for males and females in Table 7. For hirings, the estimates are consistently larger for males but are insignificant beyond the initial evaluation period. The estimates for separations are insignificant without exception.

Another concern with the estimates in Tables 4 – 6 relate to omitted variables bias. Year dummies are included in these regressions as a control for business cycle effects that are common to all employees. This measure may be too crude if macroeconomic conditions have differential effects across age groups. We have therefore experimented with adding age-specific unemployment to the specifications. Adding age-specific unemployment to the specification yields larger and significant long-term effects on hirings, with little impact on separations. The short-term effect is unchanged with a reform effect equal to 0.130. Adding an interaction term between age groups and GDP growth did not alter the conclusions. Thus, for longer-run effects on hirings at least, the results are sensitive to the measurement of the business cycle. Our last exercise in Table 8 adds a trend interacted with a dummy for the treatment group, with little effect on the estimates.[15]

- Table 8 about here -

Table 8 repeats the format of the robustness checks in Table 7 for non-manuals in engineering. The pattern is similar: decreasing reform effects when older age groups are used as controls, a U-shaped relation with firm size and no changes when tenure is added. However, the gender gap is smaller than for manuals, and the results are more robust to the inclusion of unemployment and GDP growth (with the exception of the impact of the latter on hirings in which the estimated coefficients are considerably larger). One difference is the impact of age-specific unemployment on separations. The reform effect becomes insignificant when allowing for differential unemployment effects across age groups. One possible explanation for this finding is the information technology (IT) crash in Sweden, which started in March 2000 and resulted in large flows into unemployment among non-manuals in engineering.

Regarding the results for manuals in retail that are displayed in Table 9, the most notable difference in relation to Table 8 is the absence of a U-shaped association with firm size for hirings. Here, the reform effect seems to be monotonically increasing with firm size.

- Table 9 about here -

[15] We have also experimented with a number of additional control variables: individual tenure (in the separations regressions), the share of manual workers at the firm and the share of 45 – 64-year-olds at the firm. The two firm variables are intended to pick up influences on turnover from the structure of the workforce (i.e., if firms with relatively many older workers are more inclined to hire such workers). These exercises produced small changes in the results, which are available upon request.

For a more profound check of the robustness of our findings, we performed placebo tests on the reforms. We applied the respective reforms to the "wrong" industries or worker categories and to years when no reforms were undertaken. If we find reform effects during the placebo tests, then these effects could be spurious and consequently be regarded as evidence against the interpretation of the effects in Tables 4 – 6 as being true effects of the reforms. Tables 10 and 11 report the placebo tests, based on our preferred specifications.

- Table 10 about here -

In Table 10, the "wrong" industries and worker categories are subjected to the reforms we have examined. Using the reform in 1997 as the placebo, the results for retail show negative estimates in most cases, while the results for non-manuals in engineering display a similar pattern to the results for manuals. In the latter case, a significantly positive short-run effect (0.088) is estimated for hirings. While this estimate is smaller than the corresponding one for manuals (0.128), this finding raises some doubts about the findings in Tables 4 - 6. One interpretation is that an unobserved industry shock, affecting manuals and non-manuals in a similar way, may be behind the pattern of estimates we observe.

However, another interpretation of this result is that there are spillover effects from the reform to non-manuals in the engineering industry. Such spillovers could exist if the two worker categories are interdependent in the production process or if equity concerns make it difficult for employers to treat the worker categories at the same workplace differently. For production technology to affect our estimates for non-manuals, the interdependency needs to be a specific kind, namely age-specific. For example, a decrease in the hiring costs for 45 – 64-year-old manuals should increase the demand for non-manuals in the same age interval but not the demand for non-manuals in other age groups. We regard this kind of interdependency as less plausible but are not able to distinguish between the two potential explanations with the data at hand.[16]

[16] Bergström and Panas (1992) find that manuals and non-manuals are substitutes in engineering, which supports our interpretation of the results for non-manuals.

Table 10 shows the results for a placebo reform in 2001, implemented on manuals in engineering. Again, the results are not very dissimilar to the estimated effects for non-manuals in Table 5. In this case, spillovers may play a role, as discussed above. A potentially important difference is that manuals already had implemented the reform (in 1997), so the analogy between the two placebo tests is incomplete.

Table 11 presents another set of placebo regressions. Here, estimations apply to "wrong" years, i.e., years preceding the actual reform. The overall impression is that mostly negative reform effects are estimated in almost two-thirds of the cases. Only 10 percent of the estimates yield significant and positive effects, while the rest are insignificant.

- Table 11 about here -

Studying the different collective agreements in more detail, we first note that the short-term reform effect on hirings is insignificant the year before the reform for manuals in engineering. For other placebo years and post-reform periods, the estimated reform coefficient is either insignificant or negative and significant. This finding implies that a positive and significant reform effect is not obtained in any other year prior to 1997, when the actual reform was implemented. For separations, all of the estimated coefficients for the placebo years are statistically insignificant.

For non-manuals in engineering, both short- and long-run reform effects were obtained in Tables 5 and 8. Table 11 instead shows lack of significant effects for the two- and three-year post periods when using the year 2000, i.e. the year before the actual reform, as placebo reform year. The corresponding short-run effect is negative and significant at the 10 percent level. All of the other years preceding 2001 are either significantly negative or statistically insignificant with the exception of the year 1999 in which we observe statistically significant reform effects that are in line with the ones obtained for the actual reform year. The corresponding placebo regressions on separations for non-manual employees in engineering show insignificant reform effects for the two years preceding the actual reform. In the placebo regressions for the periods three to five years prior to the reform, the coefficients are either negative and significant or insignificant. In sum, no positive and statistically significant reform effects for separations can be found for the placebo years, in contrast to the findings on the actual reform year presented in Tables 5 and 8.

Finally, the results for manual workers in retail, presented in Tables 6 and 9, showed a positive and significant reform effect on both hirings and separations. What about the corresponding placebo regressions? The lower panel of Table 11 shows that the reform effect becomes insignificant when studying the one- and two-year post periods for the year preceding the actual reform. Studying the three-year post period, a positive and significant reform effect is found. For all of the other years, the placebo reform effects are either negative and significant or insignificant with one exception. For 1996, a positive and significant reform effect is obtained for separations.

The placebo exercises show that there are no systematic results to suggest that our findings in Tables 4–6 can be dismissed as entirely spurious. We are inclined to place more emphasis on the placebo tests up to 1997 because the results for later years may be confounded by notice reforms not observed by us in other industries in the economy. In any case, the overall picture conveyed by the placebo tests does not change. Thus, we remain cautious as to the interpretation of our results.

6. Conclusions

In this paper, we have examined a reform of notice periods for employer-initiated separations in Sweden. The reform was aimed at encouraging the hiring of older workers by reducing the periods of notice for newly hired older workers from six months to one month. The new legislation implied minor or no changes in the notices for younger workers and was initiated at different time in various industries through the implementation in collective agreements. These conditions provide ample opportunity for the identification of its effects. The analysis also provides insights into the implementation of employment protection legislation in collective agreements, which has received little attention in the literature previously, possibly due to the lack of data.

Our findings indicate heterogeneous effects across sectors. A positive, albeit transient, effect is demonstrated for the hirings of older manuals in engineering, while separations were slightly affected. For older non-manual workers in the same industry, we estimate a positive effect on hirings that wanes out over time and a smaller net gain in employment was estimated because of an increase in separations. For older manuals in retail, a more sustained increase in turnover occurs because both hirings and separations increase. A salient feature of the results is that the estimated effects increase with the treatment dose, i.e., the size of the reduction in months of the notices across different age groups.

We performed numerous robustness checks of our results. Most of these checks produced minor changes in the estimated coefficients. However, placebo tests with reforms in "wrong" years and in "wrong" industries reveal significant effects in many cases, although rarely with a positive sign and mainly pertaining to hirings. Hence, we remain somewhat cautious as to the interpretation of our results. It should also be emphasised that we do not capture general equilibrium effects in our analysis.

We feel more confident in concluding that the reforms did not produce the perverse results established in the studies by Acemoglu and Angrist (2001) and Behaghel et al. (2008) discussed in the Introduction. Our analysis suggests that the Swedish reforms did not cause separations to increase with little effect on hirings, unlike some of the reforms intended to protect the employment of vulnerable groups in other countries through the implementation of firing taxes. The Swedish results may be explained by the design of the reforms, which granted extensive protection to workers remaining with the firm. As the number of older workers under the new rules accumulates over time, this particular feature of the reform is likely to diminish in importance. Our results may also reflect that voluntary separations among older workers were discouraged after the reform because job mobility would entail less employment protection for these workers.

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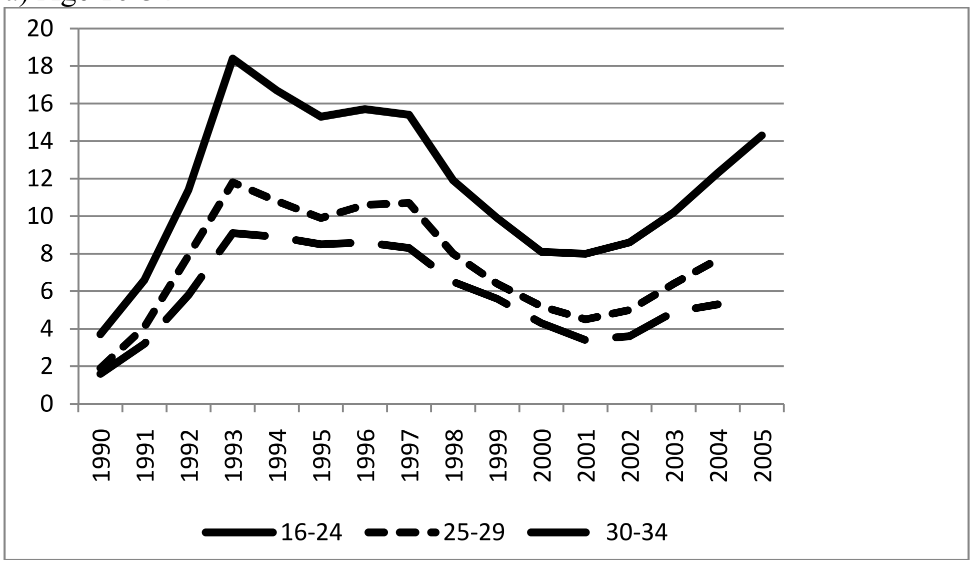
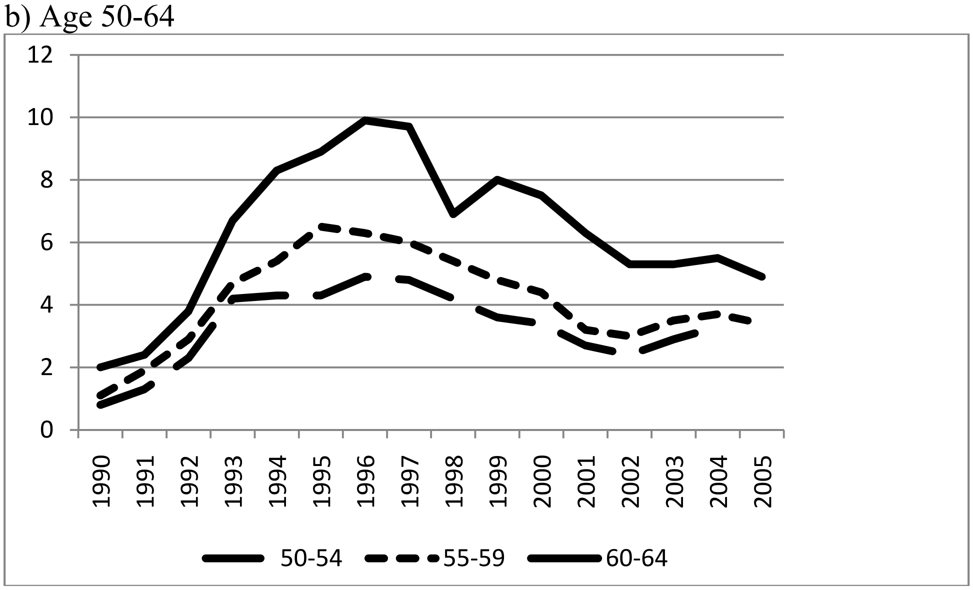
Figure 1. Unemployment rates, by age, 1990–2005

a) Age 16-34



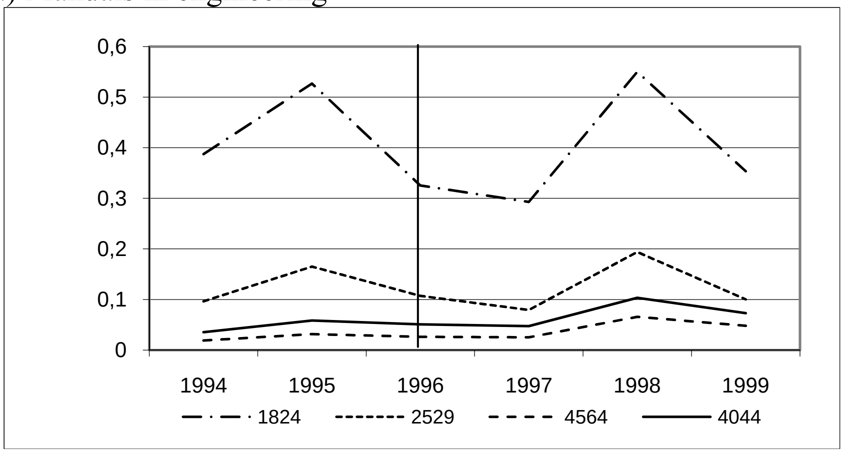
b) Age 50-64



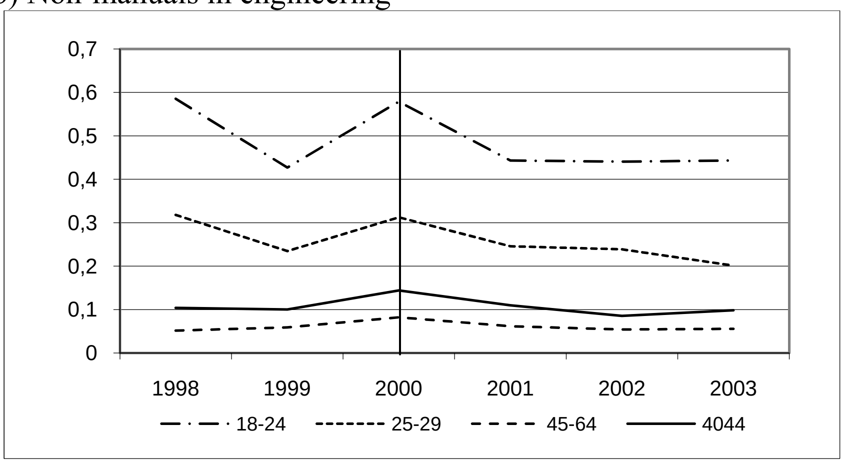
Source: Statistics Sweden.

Figure 2. Hiring rates, by age

a) Manuals in engineering



b) Non-manuals in engineering



c) Manuals in retail

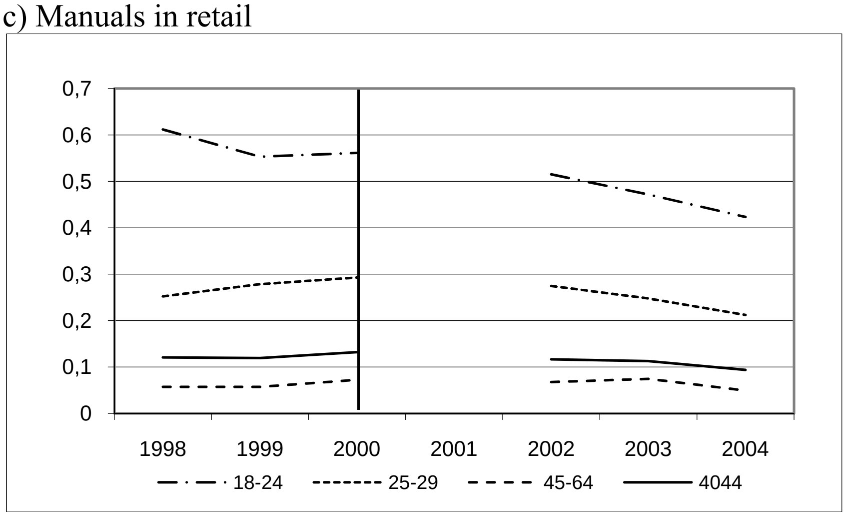
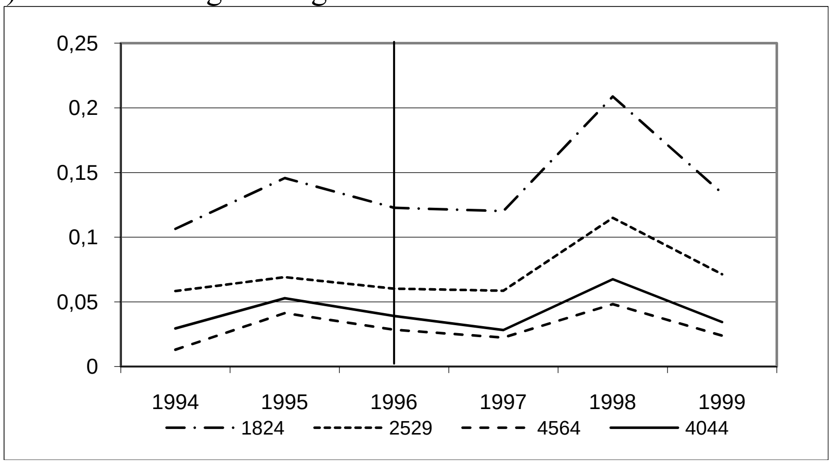
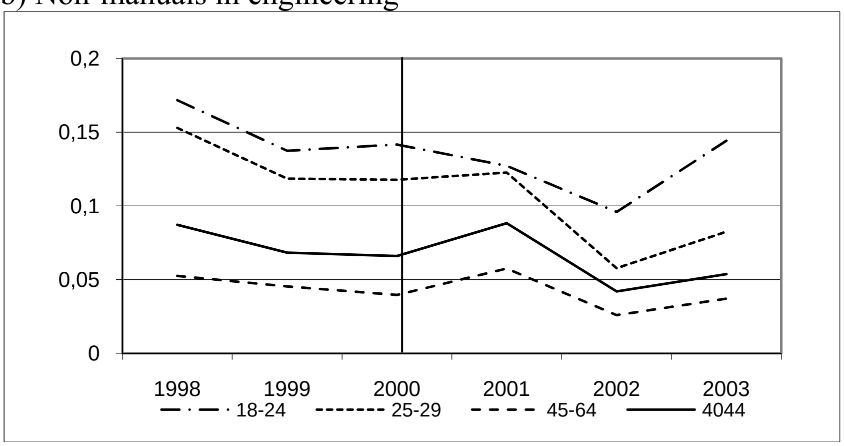


Figure 3. Separation rates, by age

a) Manuals in engineering



b) Non-manuals in engineering



c) Manuals in retail

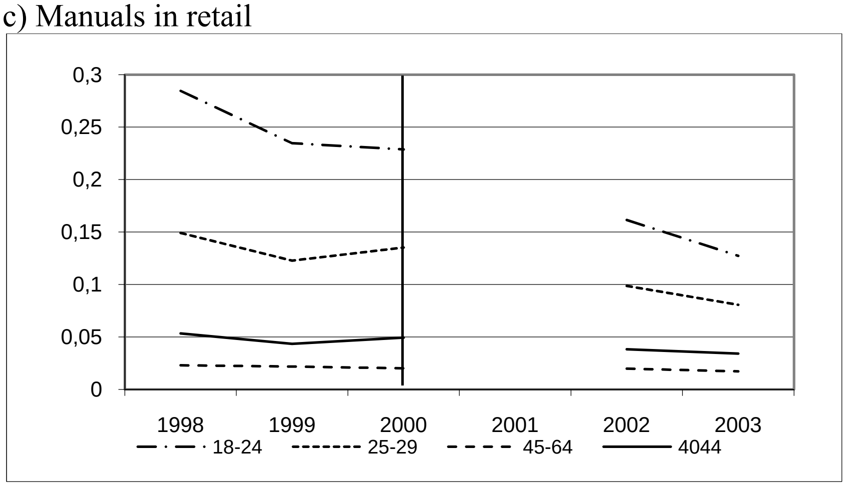


Table 1. Reform of terms of notice for employer-initiated separations in the Employment Protection Act, 1 January 1997

I. Rules *before* the reform, based on age of the employee:

1 month if age is 24 or younger
2 months if age is 25 to 29*
3 months if age is 30 to 34*
4 months if age is 35 to 39*
5 months if age is 40 to 44*
6 months if age is 45 or older*

* Applies to employees with a permanent contract who at the time of notice have been employed by the same firm for (i) the latest 6 consecutive months; or (ii) at least 12 months in total during the latest 2 years.

II. Rules *after* the reform**, based on tenure*** of the employee:

1 month if tenure is shorter than 2 years
2 months if tenure is at least 2 years but shorter than 4 years
3 months if tenure is at least 4 years but shorter than 6 years
4 months if tenure is at least 6 years but shorter than 8 years
5 months if tenure is at least 8 years but shorter than 10 years
6 months if tenure is at least 10 years

** Applies to employees with a permanent contract. The old rules continued to apply to employees employed by the same firm as before the reform.

*** Based on total length of employment by (i) the same firm; or (ii) firms belonging to the same combine; or (iii) firms having changed ownership through acquisitions and mergers. All types of employment count, including part-time and fixed-term employment.

Source: Lunning and Toijer (2006).

Table 2. Reforms of terms of notice for employer-initiated separations in selected collective agreements, 1997–2001

Industry	Manual Workers			Non-manual workers		
	Pre-reform rules	Post-reform rules	Date of reform	Pre-reform rules	Post-reform rules	Date of reform
Engineering	Old EPA, age-based	New EPA, tenure-based	January 1,1997	CA-NM, age/tenure-based	New EPA, tenure-based	February 1, 2001
Construction	CA-C, age-based	New EPA, tenure-based	2000–01	CA-NM, age/tenure-based	New EPA, tenure-based	April 1, 1998
Retail	Old EPA, age-based	New EPA, tenure-based	July 1, 2001	Various	Various	Various

Notes: Old (New) EPA= rules in accordance with Employment Protection Act up to 1997 (after 1997); CA = rules specific to collective agreement for manual workers in construction (C) or for non-manual workers (NM) in general. Implementation for non-manual workers in retail varies depending on specific agreement. See Table A.1 in Appendix for further details.
Source: Collective agreements, except for manual workers in engineering, for which the source is circulars, entitled “Anställning och uppsägning”, distributed by the employer association *Teknikföretagen* to employers and kindly made available to us by Robert Tenselius at the association.

Table 3. Expected discounted firing costs, before and after reform of advance notice, for workers hired at different ages. Monthly salaries

Agreement		δ=0.88 a=1		δ= industry-specific a=1		δ=industry-specific a=0.99	
		Age 20	Age 45	Age 20	Age 45	Age 20	Age 45
Engineering: Manuals	Pre-reform	0.842	3.428	0.874	3.847	0.988	3.996
	Post-reform	1.499	1.499	1.514	1.514	1.629	1.663
	Difference	0.656	–1.930	0.641	–2.333	0.641	–2.333
	Difference-in-difference	–2.586		–2.974		–2.974	
Engineering: Non-manuals	Pre-reform	1.013	3.798	1.019	4.125	1.134	4.273
	Post-reform	1.499	1.499	1.514	1.514	1.629	1.663
	Difference	0.486	–2.300	0.495	–2.611	0.495	–2.611
	Difference-in-difference	–2.786		–3.106		–3.106	
Retail: Manuals	Pre-reform	0.842	3.428	0.881	4.176	0.999	4.330
	Post-reform	1.499	1.499	1.473	1.473	1.591	1.627
	Difference	0.656	–1.930	0.592	–2.704	0.592	–2.704
	Difference-in-difference	–2.586		–3.296		–3.296	
Construction: Manuals	Pre-reform	0.796	3.428	0.866	5.017	0.985	5.171
	Post-reform	1.499	1.499	1.132	1.132	1.252	1.286
	Difference	0.702	–1.930	0.267	–3.885	0.267	–3.885
	Difference-in-difference	–2.632		–4.152		–4.152	
Construction: Non-manuals	Pre-reform	1.013	3.798	0.882	5.016	1.002	5.170
	Post-reform	1.499	1.499	1.132	1.132	1.252	1,286
	Difference	0.486	–2.300	0.250	–3.884	0.250	–3.884
	Difference-in-difference	–2.786		–4.134		–4.134	

Notes: The calculations are based on the job security index constructed by Heckman and Pagés-Serra (2000, p. 138). The parameter *δ*, which is the probability that the worker is not laid off during a given year, is 0.84 in engineering, 0.80 in retail and 0.56 in construction. The parameter *a* denotes the probability that a dismissal is with just cause should the case be opened in court. Workers are assumed to have a minimum tenure of 1 year with the firm and maximum tenure of 20 years. For more details on the computations, see the index equation in Section 2.2 and related discussion in text.

Table 4. Regressions for hirings and separations, manual workers in engineering, aged 45–64 and 18–24. Reform year: 1997

a) 1994–97

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Hirings				Separations			
Age 45-64	–0.385	–0.365	–0.364	–0.362	–0.095	–0.088	–0.090	–0.096
	(22.96)	(20.67)	(19.97)	(19.46)	(10.86)	(11.91)	(11.54)	(13.04)
Post-reform period	–0.107	–0.120	–0.130	–0.119	–0.022	0.014	–0.022	–0.017
	(3.25)	(3.81)	(3.86)	(3.68)	(0.85)	(0.94)	(0.92)	(1.13)
Reform effect	0.117	0.128	0.139	0.142	–0.000	0.006	0.007	0.007
	(3.86)	(4.27)	(4.37)	(4.54)	(0.04)	(0.75)	(0.77)	(0.79)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	116,872	116,872	107,029	107,029	113,285	113,285	96,127	96,127
No. firms	363	363	134	134	436	436	124	124
R-squared (adj)	0.244	0.257	0.259	0.269	0.044	0.060	0.064	0.089

b) 1994–98

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Hirings				Separations			
Age 45-64	–0.384	–0.363	–0.366	–0.365	–0.094	–0.087	–0.087	–0.093
	(22.86)	(21.12)	(19.14)	(19.46)	(10.12)	(9.71)	(8.92)	(10.84)
Post-reform period	–0.026	0.003	0.050	–0.079	0.026	–0.008	0.075	–0.014
	(0.74)	(0.07)	(1.09)	(1.89)	(1.60)	(0.46)	(1.50)	(0.42)
Reform effect	0.007	0.017	0.024	0.028	–0.028	–0.021	–0.011	–0.008
	(0.20)	(0.50)	(0.62)	(0.75)	(2.02)	(1.48)	(0.76)	(0.58)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	155,851	155,851	127,020	127,020	152,438	152,438	117,089	117,089
No. firms	453	453	109	109	528	528	109	109
R-squared (adj)	0.251	0.261	0.279	0.287	0.058	0.084	0.095	0.135

c) 1994–99

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hirings				Separations			
Age 45-64	–0.384	–0.365	–0.366	–0.363	–0.094	–0.088	–0.089	–0.093
	(22.50)	(21.14)	(18.40)	(18.32)	(10.44)	(10.18)	(9.15)	(10.35)
Post-reform period	–0.061	–0.023	0.045	–0.074	-	–0.011	0.087	–0.008
	(1.79)	(0.72)	(1.05)	(2.05)		(0.64)	(1.70)	(0.18)
Reform effect	0.031	0.037	0.041	0.042	–0.023	–0.017	–0.010	–0.012
	(1.06)	(1.25)	(1.20)	(1.26)	(2.01)	(1.47)	(0.80)	(0.92)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	190,600	190,600	143,460	143,460	187,952	187,952	131,608	131,608
No. firms	546	546	92	92	620	620	85	85
R-squared (adj)	0.235	0.246	0.271	0.279	0.055	0.072	0.081	0.113

Notes: All regressions include regional and year dummies and exclude observations of hirings and separations defined as outliers (see text). Individual and firm-specific controls consist of dummies for the individual's education, the log of the number of employees, the capital-labour ratio, value added per employee, the share of females, the share of employees with post-secondary education and six dummies for sub-industry. Absolute t-values, adjusted for clustering at the firm level, within parentheses.

Table 5. Regressions for hirings and separations, non-manual workers in engineering, aged 45–64 and 18–24. Reform year: 2001

a) 1998–2001

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Hirings				Separations			
Age 45-64	–0.462	–0.453	–0.459	–0.459	–0.097	–0.093	–0.094	–0.097
	(16.27)	(16.24)	(14.56)	(14.27)	(8.49)	(8.41)	(8.25)	(8.93)
Post-reform period	–0.108	–0.082	–0.083	–0.102	–0.020	–0.024	–0.028	–0.007
	(4.31)	(3.20)	(2.83	(3.61)	(1.07)	(1.34)	(1.29)	(0.29)
Reform effect	0.085	0.085	0.087	0.085	0.033	0.036	0.043	0.032
	(3.34)	(3.20)	(2.99)	(2.91)	(2.17)	(2.48)	(2.68)	(2.12)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	116,841	116,841	91,186	91,186	114,539	114,539	87,084	87,084
No. firms	732	732	187	187	768	768	169	169
R-squared (adj)	0.133	0.144	0.146	0.147	0.020	0.025	0.029	0.047

b) 1998–2002

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Hirings				Separations			
Age 45-64	–0.463	–0.454	–0.459	–0.462	–0.097	–0.093	–0.094	–0.099
	(16.44)	(16.31)	(12.72)	(12.47)	(8.62)	(8.63)	(8.41)	(8.94)
Post-reform period	–0.077	–0.077	–0.033	–0.055	–0.014	–0.023	–0.019	0.000
	(3.59)	(3.21)	(1.14)	(1.78)	(0.88)	(1.47)	(0.88)	(0.00)
Reform effect	0.084	0.080	0.051	0.051	0.033	0.034	0.040	0.036
	(3.32)	(3.16)	(1.79)	(1.71)	(2.66)	(2.78)	(3.01)	(3.26)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	144,403	144,403	98,758	98,758	141,375	141,375	99,158	99,158
No. firms	871	871	137	137	907	907	123	123
R-squared (adj)	0.125	0.136	0.142	0.152	0.019	0.026	0.025	0.041

c) 1998–2003

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Hirings				Separations			
Age 45-64	–0.463	–0.454	–0.446	–0.448	–0.098	–0.094	–0.098	–0.100
	(16.28)	(16.30)	(12.95)	(12.69)	(8.60)	(8.59)	(8.13)	(8.37)
Post-reform period	–0.077	-0.077	–0.031	–0.016	–0.035	–0.032	–0.042	–0.031
	(3.23)	(3.40)	(1.25)	(0.68)	(2.55)	(2.14)	(1.96)	(1.34)
Reform effect	0.082	0.077	0.029	0.029	0.023	0.022	0.030	0.025
	(3.47)	(3.15)	(1.18)	(1.15)	(1.80)	(1.72)	(2.18)	(2.05)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	170,275	170,275	104,851	104,851	167,858	167,858	106,222	106,222
No. firms	994	994	93	93	1,034	1,034	86	86
R-squared (adj)	0.118	0.130	0.134	0.143	0.019	0.025	0.027	0.039

Note: See notes to Table 4.

Table 6. Regressions for hirings and separations, manual workers in retail, aged 45–64 and 18–24. Reform year: 2001

a) 1998–2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hirings				Separations			
Age 45-64	–0.511	–0.538	–0.555	–0.554	–0.227	–0.224	–0.242	–0.244
	(48.78)	(40.84)	(48.03)	(44.45)	(17.17)	(16.71)	(19.02)	(18.91)
Post-reform period	–0.044	–0.074	–0.051	–0.085	–0.081	–0.080	–0.105	–0.117
	(4.86)	(6.89)	(5.72)	(4.34)	(6.31)	(6.51)	(8.31)	(8.73)
Reform effect	0.063	0.057	0.058	0.059	0.086	0.085	0.098	0.106
	(7.19)	(6.62)	(5.90)	(5.67)	(7.47)	(7.52)	(8.11)	(9.68)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	113,039	113,039	87,620	87,620	97,658	97,658	73,086	73,086
No. firms	929	929	158	158	965	965	138	138
R-squared (adj)	0.281	0.295	0.316	0.320	0.107	0.109	0.125	0.130

b) 1998–2003

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hirings				Separations			
Age 45-64	–0.512	–0.536	–0.560	–0.556	–0.227	–0.228	–0.246	–0.247
	(49.32)	(40.96)	(51.54)	(45.41)	(17.34)	(17.54)	(20.35)	(19.55)
Post-reform period	–0.083	–0.075	–0.102	–0.065	–0.109	–0.121	–0.081	–0.116
	(6.80)	(3.86)	(3.43)	(2.16)	(9.01)	(11.63)	(10.11)	(6.46)
Reform effect	0.089	0.081	0.083	0.081	0.101	0.099	0.106	0.109
	(8.75)	(8.04)	(6.67)	(6.21)	(12.11)	(12.07)	(18.66)	(17.67)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	148,291	148,291	103,715	103,715	127,816	127,186	88,062	88,062
No. firms	1,103	1,103	81	81	1,119	1,119	84	84
R-squared (adj)	0.260	0.273	0.302	0.307	0.099	0.101	0.119	0.122

c) 1998–2004

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)S*
	Hirings				Separations			
Age 45-64	–0.512	–0.535	–0.562	–0.557	–0.228	–0.228	–0.251	–0.252
	(48.92)	(40.87)	(51.14)	(44.77)	(17.39)	(17.52)	(22.83)	(22.35)
Post-reform period	–0.090	–0.143	–0.123	–0.122	–0.115	–0.110	–0.120	–0.131
	(7.56)	(11.42)	(6.42)	(3.95)	(9.39)	(9.35)	(17.75)	(9.44)
Reform effect	0.105	0.096	0.097	0.093	0.114	0.112	0.119	0.120
	(11.47)	(10.80)	(8.44)	(7.61)	(13.74)	(13.82)	(17.52)	(17.42)
Individual & firm-specific controls	N	Y	Y	Y	N	Y	Y	Y
Firm panel	N	N	Y	Y	N	N	Y	Y
Firm fixed effects	N	N	N	Y	N	N	N	Y
No. observations	183,264	183,264	118,910	118,910	156,379	156,379	97,610	97,610
No. firms	1,276	1,276	71	71	1,281	1,281	69	69
R-squared (adj)	0.249	0.261	0.300	0.301	0.094	0.097	0.120	0.122

Notes: The estimates of the reform effect are based on 2002 as the reform year, and excludes 2001. See also notes to Table 4.

Table 7. Robustness tests for hirings and separations, manual workers in engineering. Reform effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Hirings			Separations	
	1994–97	1994–98	1994–99	1994–97	1994–98	1994–99
Benchmark (Table 4)	0.128	0.017	0.037	0.006	–0.021	–0.017
	(4.27)	(0.50)	(1.25)	(0.75)	(1.48)	(1.47)
Subgroups						
Age:						
25–29 / 45–64	0.043	0.003	0.013	–0.000	–0.013	–0.011
	(3.20)	(0.23)	(0.99)	(0.01)	(1.78)	(1.78)
30–34 / 45–64	0.029	0.009	0.015	–0.002	–0.006	–0.007
	(3.36)	(1.23)	(2.26)	(0.37)	(0.78)	(1.09)
35–39 / 45–64	0.014	–0.027	0.002	0.004	–0.002	–0.005
	(2.11)	(0.22)	(0.44)	(0.95)	(0.40)	(0.96)
40–44 / 45–64	0.001	–0.009	–0.008	0.007	0.000	0.001
	(0.17)	(2.11)	(2.04)	(2.57)	(0.02)	(0.29)
18–24 / 45–59	0.128	0.018	0.038	0.007	–0.021	–0.017
	(4.26)	(0.53)	(1.29)	(0.77)	(1.50)	(1.47)
Firm size:						
1–19	0.322	0.219	0.253	–0.099	–0.094	–0.029
	(1.70)	(1.19)	(1.36)	(1.11)	(1.24)	(0.48)
20–49	0.077	–0.037	–0.041	0.091	0.088	0.068
	(4.08)	(0.50)	(0.57)	(1.66)	(1.75)	(1.33)
50–249	0.029	–0.004	0.007	0.014	0.004	0.007
	(0.71)	(0.13)	(0.24)	(0.62)	(0.17)	(0.33)
≥ 250	0.137	0.018	0.040	0.005	–0.023	–0.019
	(4.29)	(0.49)	(1.26)	(0.59)	(1.52)	(1.52)
Gender:						
Male	0.137	0.024	0.047	0.009	–0.017	–0.014
	(4.64)	(0.75)	(1.73)	(1.04)	(1.38)	(1.33)
Female	0.082	–0.022	–0.012	–0.002	–0.043	–0.030
	(2.13)	(0.46)	(0.28)	(0.11)	(1.75)	(1.65)
Additional variables						
Unemployment	0.130	0.147	0.089	0.005	–0.021	–0.025
	(4.12)	(4.87)	(2.69)	(0.44)	(1.27)	(1.54)
GDP growth	0.118	0.069	0.092	0.006	–0.021	–0.016
	(3.93)	(2.03)	(2.60)	(0.75)	(1.46)	(1.46)
Trend x Age 45–64	0.124	–0.003	0.025	–0.019	–0.050	–0.029
	(4.24)	(0.07)	(0.78)	(1.08)	(1.48)	(1.14)

Table 8. Robustness tests for hirings and separations, non-manual workers in engineering. Reform effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Hirings			Separations	
	1998–01	1998–02	1998–03	1998–01	1998–02	1998–03
Benchmark (Table 5)	0.085	0.080	0.077	0.036	0.034	0.022
	(3.20)	(3.16)	(3.15)	(2.48)	(2.78)	(1.72)
Subgroups						
Age:						
25–29 / 45–64	0.045	0.043	0.054	0.033	0.025	0.037
	(3.06)	(3.01)	(5.07)	(1.61)	(2.87)	(5.27)
30–34 / 45–64	0.013	0.021	0.020	0.014	0.024	0.027
	(1.32)	(2.66)	(2.53)	(2.20)	(4.27)	(4.75)
35–39 / 45–64	0.004	0.010	0.012	0.001	0.012	0.015
	(0.60)	(1.56)	(2.02)	(0.17)	(2.22)	(3.07)
40–44 / 45–64	0.005	0.009	0.010	–0.006	0.005	0.009
	(0.73)	(1.57)	(2.20)	(0.12)	(1.25)	(2.55)
18–24 / 45–59	0.085	0.081	0.077	0.036	0.034	0.022
	(3.17)	(3.12)	(3.11)	(2.51)	(2.77)	(1.71)
Firm size:						
1–19	0.074	–0.007	–0.008	0.007	0.011	0.021
	(0.58)	(0.06)	(0.07)	(0.11)	(0.19)	(0.39)
20–49	–0.271	–0.228	–0.166	–0.138	–0.117	–0.070
	(1.89)	(1.88)	(1.49)	(1.30)	(1.47)	(1.06)
50–249	0.086	0.097	0.094	0.023	0.034	0.032
	(1.31)	(1.94)	(2.11)	(0.66)	(1.24)	(1.14)
≥ 250	0.085	0.076	0.069	0.042	0.039	0.024
	(2.99)	(2.74)	(2.61)	(2.64)	(2.88)	(1.68)
Gender:						
Male	0.091	0.081	0.082	0.030	0.030	0.020
	(3.27)	(3.03)	(3.23)	(2.04)	(2.33)	(1.51)
Female	0.082	0.085	0.072	0.045	0.042	0.026
	(2.29)	(2.53)	(1.92)	(2.03)	(2.38)	(1.44)
Additional variables						
Unemployment	0.074	0.075	0.077	0.021	0.021	0.005
	(3.07)	(3.08)	(3.09)	(0.80)	(1.02)	(0.24)
GDP growth	0.783	0.260	0.245	0.030	0.037	0.028
	(4.16)	(3.51)	(3.49)	(1.77)	(2.83)	(2.06)
Trend x Age 45–64	0.057	0.048	0.058	0.044	0.056	0.045
	(2.04)	(1.72)	(2.20)	(2.79)	(3.79)	(2.86)

Table 9. Robustness tests for hirings and separations, manual workers in retail. Reform effects

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
		Hirings			Separations	
	1998–02	1998–03	1998–04	1998–02	1998–03	1998–04
Benchmark (Table 6)	0.057	0.081	0.096	0.085	0.099	0.112
	(6.62)	(8.04)	(10.80)	(7.52)	(12.07)	(13.82)
Subgroups						
Age:						
25–29 / 45–64	0.018	0.034	0.044	0.047	0.054	0.057
	(1.78)	(3.01)	(4.29)	(8.17)	(10.12)	(11.36)
30–34 / 45–64	0.028	0.029	0.033	0.025	0.028	0.031
	(3.35)	(3.39)	(4.05)	(5.52)	(7.80)	(9.61)
35–39 / 45–64	0.012	0.023	0.028	0.018	0.018	0.022
	(1.92)	(3.30)	(5.08)	(4.65)	(5.07)	(7.23)
40–44 / 45–64	0.012	0.017	0.016	0.008	0.009	0.009
	(2.02)	(3.24)	(3.58)	(2.19)	(3.31)	(3.75)
18–24 / 45–59	0.057	0.080	0.094	0.084	0.099	0.111
	(6.23)	(7.81)	(10.46)	(7.30)	(11.74)	(13.47)
Firm size:						
1–19	–0.056	0.018	0.013	0.048	0.071	0.096
	(1.50)	(0.52)	(0.44)	(1.54)	(2.89)	(4.22)
20–49	0.031	0.019	0.062	0.036	0.049	0.059
	(0.63)	(0.47)	(1.72)	(0.99)	(1.52)	(2.05)
50–249	0.026	0.054	0.078	0.034	0.048	0.067
	(1.16)	(2.86)	(4.24)	(1.80)	(3.23)	(4.78)
≥ 250	0.066	0.089	0.103	0.094	0.109	0.120
	(6.47)	(7.92)	(10.67)	(7.88)	(13.53)	(15.46)
Gender:						
Male	0.044	0.062	0.077	0.074	0.088	0.100
	(2.95)	(4.44)	(6.69)	(4.69)	(7.29)	(8.56)
Female	0.060	0.086	0.043	0.087	0.102	0.114
	(6.32)	(7.76)	(4.40)	(9.26)	(14.17)	(16.21)
Additional variables						
Unemployment	0.063	0.090	0.089	0.056	0.079	0.105
	(6.98)	(8.49)	(8.17)	(4.84)	(9.30)	(14.93)
GDP growth	0.221	0.101	0.132	0.155	0.152	0.165
	(4.60)	(2.60)	(11.65)	(6.34)	(10.89)	(11.61)
Trend x Age 45–64	0.038	0.055	0.098	0.083	0.099	0.116
	(2.79)	(3.25)	(7.53)	(6.14)	(8.85)	(12.33)

Table 10. Placebo tests for hiring and separations. Reform effects in ”wrong” industries

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	Hirings			Separations		
Placebo reform 1997:	1994–97	1994–98	1994–99	1994–97	1994–98	1994–99
Retail, manuals	–0.042	–0.1231	–0.133	0.008	–0.040	–0.036
	(2.25)	(4.99)	(6.29)	(0.52)	(2.60)	(2.35)
Engineering, non-manuals	0.088	0.011	0.047	0.017	–0.017	–0.017
	(3.03)	(0.35)	(1.53)	(0.97)	(1.18)	(1.65)
Placebo reform 2001:	1998–01	1998–02	1998–03	1998–01	1998–02	1998–03
Engineering, manuals	0.085	0.052	0.072	0.012	0.022	0.028
	(3.74)	(4.29)	(7.18)	(1.09)	(2.26)	(2.79)
Placebo reform 2002:	1998–02	1998–03	1998–04	1998–02	1998–03	1998–04
Engineering, manuals	0.019	0.065	0.055	0.033	0.037	0.050
	(1.18)	(4.06)	(3.48)	(2.81)	(3.16)	(4.46)

Table 11. Placebo tests for hiring and separations. Reform effects in "wrong" years

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	Hirings			Separations		
Placebo reform year:	(t-3) - t	(t-3) - (t+1)	(t-3) - (t+2)	(t-3) - t	(t-3) - (t+1)	(t-3) - (t+2)
Engineering, manuals						
1995	–0.282	–0.177	–0.134	0.011	0.020	0.023
	(9.91)	(7.29)	(5.30)	(0.83)	(1.33)	(1.58)
1996	0.048	0.061	–0.010	0.023	0.024	0.010
	(1.24)	(1.66)	(0.23)	(1.18)	(1.50)	(0.57)
Engineering, non-manuals						
1995	–0.300	–0.308	–0.261	–0.060	–0.039	–0.020
	(7.37)	(10.09)	(7.04)	(3.84)	(3.21)	(1.74)
1996	–0.159	–0.093	–0.128	–0.011	0.000	–0.023
	(2.69)	(1.68)	(2.47)	(0.81)	(0.03)	(1.99)
1998	–0.073	0.008	–0.003	–0.049	–0.031	–0.030
	(2.05)	(0.25)	(0.11)	(3.18)	(2.60)	(2.29)
1999	0.118	0.056	0.072	–0.006	–0.011	–0.002
	(3.69)	(1.91)	(2.56)	(0.31)	(0.59)	(0.12)
2000	–0.051	0.004	0.014	–0.012	0.004	0.007
	(1.82)	(0.13)	(0.50)	(0.61)	(0.26)	(0.52)
Retail, manuals						
1995	–0.093	–0.102	–0103	–0.112	–0.051	–0.044
	(6.26)	(6.45)	(6.90)	(7.17)	(4.68)	(4.39)
1996	–0.084	–0.082	–0.138	0.062	0.041	0.001
	(4.17)	(4.47)	(5.57)	(2.32)	(1.92)	(0.07)
1998	–0.153	–0.125	–0.114	–0.066	–0.040	–0.031
	(8.46)	(9.25)	(8.09)	(3.53)	(2.48)	(2.17)
1999	–0.033	–0.029	–0.029	–0.018	–0.017	–0.017
	(1.76)	(2.08)	(2.08)	(1.39)	(1.54)	(1.54)
2000	0.003	0.003	0.021	0.012	0.012	0.050
	(0.21)	(0.21)	(1.89)	(1.13)	(1.13)	(4.71)

APPENDIX

Table A.1. Terms of notice for employer-initiated separations in selected collective agreements, 1995–2005

a) Engineering (ISIC codes: 28–35)

I. Manual workers

Agreements do not say anything specific about terms of notice, hence Employment Protection Act and changes therein apply throughout the period.

II. Non-manual workers

Agreements specify terms of notice supplanting Employment Protection Act throughout the period. Terms are both less stringent and more stringent than Act, depending on age and tenure of the employee.

1. – 31 March 1995*:

Tenure / Age (years)	< 25	25 – 29	30 – 34	35 – 39	40 – 44	≥45
< 6 months	1	1	1	1	1	1
6 months – 6 years	1	2	3	4	5	6
6 – 9 years	2	3	4	5	5	6
9 – 12 years	-	3	4	5	6	6
> 12 years	-	3	4	6	6	6

* Tenure is calculated as specified in the Employment Protection Act. If the dismissal is due to lack of work, the employer must follow the terms of notice specified in special employment protection schemes (*omställningsavtal*).

2. 1 April 1995 – 31 January 2001:
The rules as in 1) applies, with the addendum that notice should be extended by 6 months for employees who at the time of notice, which should be due to lack of work, have reached the age of 55 and have been employed for the 10 latest consecutive years.

3. 1 February 2001– :
For employment contracts signed before 1 February 2001, 1) applies, with addendum in 2). For contracts signed after this date, same rules as in new Employment Protection Act (see Table 1, part II, with addendum in 2).

b) Construction

I. Manual workers

Rules and dates of implementation in the three agreements in the construction sector are similar, but not identical. Rules have been less stringent than or identical to Employment Protection Act, depending on period and age of employee.

a) Building agreement (*Byggnadsavtalet*)
ISIC codes: 45110, 45120, 45211, 45212, 45229, 45250, 45320, 45340, 45410, 45420, 45450, 45500
ISIC codes unique to agreement: 45110, 45211, 45229, 45320, 45340, 45410, 45420, 45450

1. – 31 March 2000

1 month if age is 25 or younger
2 months if age is 25 to 35*
4 months if age is 35 to 44*
6 months if age is 45 or older*

* Applies to employees with a permanent contract who at the time of notice have been employed by the same firm for (i) at least 12 months in total during the latest 2 years, or in case of employees at least 45 years old (ii) the latest 6 consecutive months.

2. 1 April 2000 –
Same rules as in new Employment Protection Act for employment contracts signed 1 July 1997 or later (see Table 1, part II). For employment contracts signed before this date, rules in old agreement (see a.1) apply.

b) Construction (except buildings) agreement (*Anläggningsavtalet*)
ISIC codes: 45120, 45212, 45230, 45240, 45250, 45500
ISIC codes unique to agreement: 45240

1. – 31 March 2001
Same rules as in Construction agreement (see a.1)

2. 1 April 2001 –
Same rules as in Construction agreement (see a.2).

c) Road and rail construction agreement (*Avtalet för väg och ban*)
ISIC codes: 45120, 45230, 45500
ISIC codes unique to agreement: None

1. – 31 March 2000
Same rules as in Construction agreement (see a.1).

2. 1 April 2000 –

Same rules as in new Employment Protection Act for employment contracts signed 1 November 1997 or later (see Table 1, part II). For employment contracts signed before this date, rules in old agreement (see a.1) apply.
II. Non-manual workers 1. – 31 March 1998 Same rules as for non-manual workers in Engineering agreement (see Table 2c, part II, 1). 2. 1 April 1998 – Same rules as in new Employment Protection Act for employment contracts signed 1 April 1998 or later (see Table 1, part II). For contracts signed before this date, old rules apply as long as these imply longer notice (see 1). Same addendum as for non-manual workers in Engineering agreement applies to both new and old rules (see Table 2c, part II, 2).

c) Retail (ISIC codes: 50–52)

I. Manual workers There are many agreements in this sector: Retail trade agreement (*Detaljhandelsavtalet*), Wholesale trade agreement (*Partihandelsavtalet*) and other sector-specific agreements. Most agreements apply same rules regarding notice, namely those of Employment Protection Act (with delayed implementation).* * An exception is restaurant workers in the retail sector, who are covered by separate agreement with Hotel and Restaurant Workers' Union (*Hotell- och restaurangavtalet*). 1. – 30 June 2001 Same rules as in old Employment Protection Act (see Table 1, part I).* * Applies to employees with a permanent contract who at the time of notice have been employed by the same firm for (i) at least 12 months in total during the latest 2 years or (ii) the latest 6 consecutive months. 2. 1 July 2001 – Same rules as in new Employment Protection Act for employment contracts signed 1 July 2001 or later (see Table 1, part II). For contracts signed before this date, 1) applies.
II. Non-manual workers There are many agreements in this sector, with different rules regarding notice. Many workers are covered by agreement below, *Tjänstemannaavtalet HTF*. 1. – 31 December 1997 Same rules as for non-manual workers in Engineering agreement (see Table 2c, part II, 1 and addendum in 2). 2. 1 January 1998 – Same rules as in new Employment Protection Act for employment contracts signed 1 January 1998 or later (see Table 1, part II). For contracts signed before this date, 1) applies

Source: See Table 2.

Table A2. Variable definitions

Age groups	18-24 25-29 30-34 35-39 40-44 45-64
Share women	Share of females
L	Number of employees.
Capital/L	Capital intensity (Net property, plant and equipment)/employees (in million SEK).
Share high-skilled	Share of high-skilled employees.
Value added/L	Value added per employee (Sales-operational expenses excluding wages)/employees (in million SEK).
Education levels	Elementary school (<9 years) Compulsory school (9 years) Upper Secondary School <3 Upper Secondary School =3 Upper Secondary School =4 University undergraduate University graduate
Experience	Age minus number of years of schooling minus seven.
New hires	Indicator variable equal to one if an individual is newly hired at time *t*, zero otherwise.
Separations	Indicator variable equal to one if an individual is separating from a firm in time *t*, zero otherwise.

Table A3. Descriptive statistics, (t-3) – (t+3)

	Manuals in engineering		**Non-manuals in engineering**		**Manuals in retail**	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Age group 18-24	0.115	(0.319)	0.019	(0.138)	0.277	(0.448)
Age group 25-29	0.157	(0.363)	0.113	(0.316)	0.157	(0.364)
Age group 30-34	0.159	(0.365)	0.160	(0.366)	0.128	(0.335)
Age group 35-39	0.126	(0.332)	0.161	(0.368)	0.106	(0.308)
Age group 40-44	0.114	(0.318)	0.128	(0.334)	0.079	(0.270)
Age group 45-64	0.329	(0.470)	0.419	(0.493)	0.251	(0.434)
New hires	0.109	(0.312)	0.125	(0.331)	0.247	(0.431)
Separations	0.057	(0.232)	0.070	(0.255)	0.091	(0.287)
Elementary School <9	0.162	(0.369)	0.022	(0.147)	0.089	(0.285)
Compulsory School =9	0.182	(0.386)	0.044	(0.204)	0.175	(0.380)
Upper Secondary School <3	0.441	(0.497)	0.180	(0.384)	0.316	(0.465)
Upper Secondary School =3	0.196	(0.397)	0.382	(0.486)	0.366	(0.482)
Upper Secondary School =4	0.013	(0.113)	0.067	(0.249)	0.027	(0.162)
University undergraduate	0.006	(0.077)	0.289	(0.453)	0.027	(0.162)
University graduate	0.000	(0.013)	0.016	(0.126)	0.000	(0.011)
Log firm size	7.637	(1.487)	7.582	(1.556)	7.069	(1.681)
Share of high-skilled	0.216	(0.123)	0.355	(0.186)	0.127	(0.069)
Share women	0.228	(0.110)	0.241	(0.105)	0.574	(0.256)
Value added/L	0.516	(0.301)	0.574	(0.674)	0.440	(0.234)
Capital/L	0.269	(0.225)	0.252	(0.243)	0.195	(0.277)
Number of observations	428,757		338,440		407,352	

Notes: Figures are based on the same sample of workers and firms as in the regression analysis. See Section 3 for details.