



IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

Last in, first out? Estimating the effect of seniority rules in Sweden

David von Below
Peter Skogman Thoursie

WORKING PAPER 2008:27

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market policies, studies of the functioning of the labour market, the labour market effects of educational policies and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala

Visiting address: Kyrkogårdsgatan 6, Uppsala

Phone: +46 18 471 70 70

Fax: +46 18 471 70 71

ifau@ifau.uu.se

www.ifau.se

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

ISSN 1651-1166

Last in, first out? Estimating the effect of seniority rules in Sweden*

by

David von Below* and Peter Skogman Thoursie[♠]

First version: September 7, 2007

This version: November 12, 2008

Abstract

In this paper we investigate whether a relaxation in seniority rules (the ‘last-in-first-out’ principle) had any effect on firms’ employment behaviour. Seniority rules exist in several countries and, like Sweden, most European countries have a more lenient employment protection for firms below a certain size. Despite the fact that small firms represent a large share of all firms and stand for a substantial share of total employment, there is limited knowledge of how such exemption rules affect firms’ employment behaviour — the consequences of seniority rules on firms’ employment behaviour have not been examined at all. Using data including the population of firms matched with the population of workers for the period 1999–2002, we do not find any general effects on worker flows or on hires and separations. The only exception is a tendency of an increase in the share of separations for older workers and workers with longer seniority. The result points to the importance of considering in detail how legislation is formulated and how it works in practice.

Keywords: Employment protection, employment change, hires, separations, Regression discontinuity

JEL-codes: E24; J63; J65

* We thank Louise Kennerberg, Helge Benmarker and the Institute for Labour Market Policy Evaluation (IFAU) for providing us with the data. We also thank Marcus Eliasson, Eva Mörk, Per Pettersson-Lidbom, Per Skedinger and seminar participants at the Department of Economic, Umeå University, IFAU and the Institute for International Economic Studies (IIES). We especially thank Lena Nekby and Anders Åkerman for their participation in the initial phase of this project.

♣ Institute for International Economic Studies, Stockholm University, SE-106 91 Stockholm, Sweden. E-mail: david.vonbelow@iies.su.se.

♠ Department of Economics, Stockholm University, SE-106 91 Stockholm, Sweden. E-mail: Peter.thoursie@ne.su.se.

Table of contents

1	Introduction	3
2	Theory and empirical literature on worker flows	6
2.1	Theoretical prediction on worker flows	6
2.2	Empirical literature on worker flows	7
3	Employment protection and LIFO in Sweden	10
4	Data and empirical strategy	11
5	Graphical empirical analysis	16
5.1	Threshold effects of the exemption rule	16
5.2	Effects on worker flows	20
6	Econometric analysis of worker flows	25
6.1	Regression discontinuity analysis	25
6.2	Estimations for firms with at least three separations	30
6.3	Difference in differences analysis	31
6.4	Heterogeneous effects with respect to firm size	34
7	Discussion and conclusion	36
	References	39
	Appendix	43

1 Introduction

This paper empirically investigates how employment protection affects firms' employment behaviour. How employment protection affects the labour market functioning is a controversial subject (see e.g., OECD, 2004). The main motivation for employment protection is to protect workers from unfair dismissals. Employment protection is also a welfare arrangement that provides economic security to workers, ultimately increasing the value of work. Opponents against strong employment protection argue that more lenient protection would improve the efficiency of the labour market. How employment protection affects firms' employment behaviour is vital to the design of policy and to the understanding of labour market behaviour.

Previous empirical research often relies on cross-country variation by correlating some strictness index measure of employment protection with the employment level. One major challenge with cross-country studies is to obtain comparable measures of strictness, since legislation varies widely across countries (see e.g., discussion in Howell *et al.*, 2007; Kugler 2007). We therefore argue that it is important to evaluate certain aspects of the employment protection legislation. One such aspect is seniority rules, which protect workers with long seniority. Seniority rules exist in several countries (OECD, 2003). In France, Italy, Mexico and the Netherlands, the law stipulates some kind of seniority-based rules regarding dismissals. In Finland and the US, seniority rules are often laid down in collective agreements. In Norway and the UK, seniority rules are not stipulated by law or laid down by collective agreement but often used as an accepted custom. In the Netherlands and in Sweden the seniority rule is formulated as a 'last-in-first-out' (LIFO) principle. In Sweden this LIFO principle is one main cornerstone in the Swedish Employment Protection Act (EPA, *Lagen om anställningsskydd*, SFS 1982:80, 22 §). The principle states that the worker who was employed last has to go first when a firm downsizes. The LIFO principle was reformed in January 2001 so that employers with ten or fewer employees were allowed to exempt two workers from the seniority rule.

Like Sweden, most European countries have a more lenient employment protection for firms below a certain size. For example, workers in France at firms with fewer than ten employees are less protected against layoffs. In Italy, workers in firms with fewer than 16 employees are less protected. In Spain, redundancy payments are lower for firms with fewer than 25 employees. Despite the fact that small firms represent a substantial share of all firms and stand for a large share of total employment, there is limited knowledge how such exemption rules affect firms' employment behaviour.¹ Moreover, the consequences of seniority rules on firms' employment behaviour have not been examined at all.

The purpose of this paper is to estimate whether the exemption rule, which implied that firms with ten or fewer employees could exempt two workers from the last-in-first-out principle, had any effects on firm's employment behaviour. Theory suggests that less protection – the exemption rule in this case – increases worker flows through increased hires and separations. The exemption rule may also induce firms to engage in strategic behaviour to avoid these institutional constraints. Therefore, we will first investigate whether firms engaged in such strategic behaviour in order to be allowed to use the exemption rule. Next, we analyse the effects of the exemption rules on workers flows.

We use data including the population of firms matched with the population of workers for the period 1999–2002. Worker flows are measured at a monthly basis. The break in the policy for firms of size ten provides a natural setting for analysing the impact of the exemption rule using a regression discontinuity (RD) approach. The key advantage of this approach is that it provides estimates that are 'as credible as those from a randomized experiment' (Lee 2008) under relatively weak conditions.² The reason is that we do not study a reform per se, but a discontinuity present in a static policy. In this paper, this means that we compare employment behaviour between firms with ten and eleven employees. As such, we do not have to rely on any assumptions about

¹ The overall evidence from the few existing studies suggest very small effects (Bauer *et al.* 2007; Borgarello *et al.* 2004; Schivardi and Torrini 2004; Verick 2004. See also the empirical overview in Section 2.

² The regression discontinuity approach has recently become increasingly popular in economics because of the rather weak identifying assumptions. For an overview of the regression discontinuity design and recent studies, see Imbens and Lemieux (2008).

the comparability of the outcome development for the treated and the control group, which is the case when using the differences-in-difference (DD) approach.³ However, we will also apply the DD estimator as a complementary analysis, and in addition use this approach in order to examine whether there are any differential effects by firm size.

The paper contributes to the literature on employment protection and firms' employment behaviour by focusing on the effect of a seniority rule on firms' strategic behaviour and on workers flows.⁴ Moreover, knowledge of how exemption rules in general affect firms' employment behaviour is rare. Furthermore, previous studies often rely on small and un-representative data sets (see Schivardi and Torrini, 2004). This makes it difficult to generalise from results and it is not always clear whether non-robust results are due to insignificant effects or due to a small number of observations. This is not the case in our paper since we have access to data on the whole population of firms and workers.

Results show that the exemption rule had no effect at all on the size distribution of firms and we do not find any general effects on worker flows. The only exception is a tendency of an increase in the share of separations for older workers and workers with longer seniority, but these effects are not robust to how standard errors are calculated, and the effects are small in economic terms. The result is interesting not only because it is the first paper evaluating the consequences of the LIFO principle on firms' employment behaviour, but it also points to the importance of considering in detail how legislation is formulated and how it works in practice.

The remainder of the paper is organized as follows. In Section 2 we present an overview of the theory and empirical literature on worker flows. Section 3 describes the employment protections legislation and the LIFO principle in Sweden. Data and the empirical strategy are outlined in Section 4. Results based on a graphical analysis are presented in Section 5, and econometric results are presented in Section 6. Discussion and conclusions are given in Section 7.

³ The commonly used difference-in-differences estimator has been found to perform poorly with inappropriately chosen control groups (see e.g., Lemieux and Milligan 2008).

⁴ Two previous studies using the LIFO reform in January 2001 found that individual sick reporting increased at firms which could exempt two workers from the seniority rule (see Lindbeck *et al.*, 2006; Olsson, 2008).

2 Theory and empirical literature on worker flows

2.1 Theoretical prediction on worker flows

Theory predicts effects of employment protection on a wide range of outcomes such as employment, employment volatility, productivity, wages, GDP growth, innovations, etc. The brief overview below will focus solely on the theoretical predictions of dismissal costs on workers flows. Legislation often states that a layoff must be based on objective grounds and the criteria for objective grounds are formulated in the legislation. Dismissal legislation also includes other more direct costs for layoffs such as redundancy pay, notice periods and seniority rules, which restrict the employers' influence over whom to lay off.

Lay-off costs are often modelled as a tax on layoffs. Lazear (1990) argues that such taxes can be avoided by an efficient labour contract where an *ex ante* transfer – such as a wage reduction – is made from the worker to the employer. In practice, however, with imperfect labour markets and rigid wages (including minimum wages and wages set by collective agreements), dismissal legislation will impose adjustment costs for the employer. As such, theory predicts that lay-off protection decreases hires as well as separations. Since fewer workers are hired, this reduces the exit rate from unemployment. At the same time, fewer workers enter unemployment through reduced separations. Taken together, the net effect on the employment level (or unemployment) is therefore *a priori* ambiguous. The predicted effect on worker flows, on the other hand, is unambiguously negative.

Theory also predicts that dismissal protections have differential effects on different groups of workers (Bertola *et al.*, 2008). Redundancy pay and notice periods often increase with seniority, which means higher lay off costs for workers with longer seniority such as older workers. For workers with lower attachment to the labour market such as young, immigrant, female and disabled workers it may be difficult to verify their productivity. For example, it might be more difficult to assess productivity of younger workers with little labour market experience; employers might find it difficult to evaluate degrees taken in other countries; employers might be more uncertain about

women's presence at work since they, on average, have longer periods with parental leave and higher sickness absence on average than men. Workers with more uncertain productivity have smaller chances of being hired if risk averse employers see them as a more risky investment. Taken together, the exemption rule might improve employment opportunities for younger, female and foreign born workers. For older workers, the exemption rule might instead increase separations.

Adjustment costs such as seniority rules also induce firms to engage in strategic behaviour to avoid these institutional constraints. If exemption is possible on the basis of a threshold scale, firms could split into formally independent units. Smaller firms could stop growing (Schivardi and Torrini, 2004). Employment protection legislation also affects the entry and exit probabilities of firms (i.e., the *extensive margin*). If fewer firms enter and more firms close due to employment protections, the employment level will decrease.

Theory also predicts positive effects of employment protections. Besides protecting workers from unfair dismissals, it is often argued that the main purpose of dismissal protection is to protect workers from income fluctuations generated by job losses. This is the insurance argument for employment protection, where insurance improves worker welfare. The reason why legislation is required to provide insurance is analogous to the argument in favour of for example social insurance – insurance markets affected by moral hazard and adverse selection do not provide sufficient coverage (Pissarides, 2001). Furthermore, employment protection might also improve the employer–employee relationship (motivation, willingness to co-operate etc.), which in turn could enhance productivity. Dismissal protection might also raise worker productivity due to improved training incentives, both from the employer as well as on the work side, which increase human capital (Mortensen and Pissarides, 1999).

2.2 Empirical literature on worker flows

Empirical evidence on the impact of employment protection on the employment level and worker flows does not clearly support the theoretical predictions (see surveys by Addison and Teixeira, 2005, and Kugler, 2007). A vast majority of the empirical literature follows Lazear (1990) in exploiting cross-country variation in index values for the strictness of employment protection legislation. The only robust empirical finding in

this literature is that more strict employment protection reduces flows into and out of unemployment. The results concerning aggregate employment are not robust – estimated effects point in different directions. A number of studies, however, suggest that dismissal protection reduces employment and increases unemployment for younger workers and in some cases also female workers (see e.g., Bassani and Duval 2006; Bertola *et al.*, 2008; Botero *et al.*, 2004; Heckman and Pagés-Serra, 2000; Skedinger, 1995).

In addition to the problem of finding comparable measures of strictness across countries, cross-country studies suffer from other identification problems. It is difficult to handle other important differences than differences in legislations between countries which may help to explain the estimated relationship between employment protection and worker flows. Some of the aggregated studies therefore exploit time variation within countries. However, employment protection usually shows very little variation over time. It is also the case that fluctuations in employment and the unemployment rate might cause changes in the legislation, making employment protection endogenous due to reversed causality.

In recent years there have been an increasing number of studies examining the impact of employment protection at the firm or worker level. These studies either exploit reforms that change the dismissal legislation for firms under a certain number of workers, or changes in legislation that occur at different points in time across regions. Kugler and Pica (2008) exploit a reform in Italy where firms below size fifteen were exempted from legislation prior to 1990 but were subject to the legislation from that year. According to their results, worker flows decreased for smaller firms relative to large firms when they were subject to the legislation. Kugler (2004), exploiting a reform in Colombia, found that the flows in and out of unemployment increased when employment protection was more liberal. Results from Autor *et al.* (2007) support this finding. Based on US data they show that worker flows decreased in states that implemented a stricter employment protection than the so-called employment-at-will principle. There are exceptions to these results, however. Martins (2007) found no effects on worker flows when studying a reform in Portugal in 1989, where firms with at most twenty workers get more lenient protection legislation. Germany changed their

dismissal legalisation at two times during the 1990s. Until 1996, firms with five or fewer workers had more lenient protection legislation. In 1996, this exemption was extended to firms with at most ten workers. In 1999, the threshold of five workers was re-introduced. German studies exploiting these reforms find no or very small effects on worker flow (see Bauer *et al.*, 2007).

Taken together, results exploiting reforms using micro-data in different countries in order to estimate effects on worker flows are mixed. It should be emphasized, however, that it is not always straightforward to compare results from different countries as it is in general difficult to calculate how the reform really changed the costs for firms. If changes in firms' adjustment costs are small it is reasonable to find small reform effects.

As regards studies focusing on effects on the employment level results are mixed. Some studies show that stricter employment protection reduces the employment level (see e.g., studies by Autor *et al.* (2004, 2006) on US data; Kugler *et al.* (2003) who use data from Spain; Martins (2007) who uses data from Portugal; and Schivardi and Torrini (2008) using data from Italy). Miles (2000), on the other hand, show no employment effects using data for the US. Autor *et al.* (2007) find positive effects for the US and a German study by Verick (2004) also shows positive effects.

Some studies focus more directly on the estimation of threshold effects when firms of different sizes have different degrees of employment protection. This question is closely related to our paper since we will examine the consequences of the relaxation of the LIFO principle, which was introduced in 2001 for firms below the size on eleven. Along this dimension, Borgarello *et al.* (2004) and Schivardi and Torrini (2004), using different empirical datasets and estimation strategies, analyse threshold effects resulting from the exemption threshold in Italian employment protection legislation. Overall, their evidence suggests that if there is an effect of the exemption threshold on the size distribution of firms, it is probably very small. In a similar study for Spain, Boeri and Jimeno (2005) observe that lay-off probabilities for workers in firms exempted through a threshold scale are higher than those of workers in firms covered by employment protection legislation. As for Germany, Verick (2004) finds a slightly lower probability of employment growth for firms just under the threshold, when the threshold was tightened through the change in the German dismissal protection legislation.

3 Employment protection and LIFO in Sweden

The 1982 Employment Protection Act (EPA) stipulates that the default type of employment contract is a permanent contract. Fixed-term contracts are allowed if warranted by the nature of the work to be done, and probation periods of up to six months are allowed in connection with permanent contracts. Workers may be fired for gross misconduct, or they may be laid off, either because of ‘shortage of work’ or due to reasons pertaining to the worker personally. The latter is rarely used in practice, and instead it is ‘shortage of work’ that is the principal justification for laying off workers in most cases. A newly employed worker has a minimum notice period of one month, and this increases by one additional month for every two years of tenure, up to a maximum of six months’ notice after ten years of tenure. When a firm decides to downsize, i.e., to lay-off employees due to ‘shortage of work’, it cannot pick and choose at will, but must follow the LIFO principle – in other words, if downsizing by n employees, it is the n workers who most recently joined the firm that must be laid off. In addition, an employee who has been laid off due to ‘shortage of work’ has priority in reemployment for nine months following layoff (subject to having worked at least 12 months with the firm).

Several provisions in the EPA make the LIFO principle less stringent than what a first impression suggests. First, if the firm is bound by collective agreements, the workforce should be divided into groups based on their trade union affiliation, and the LIFO principle then applies to each such group separately. Most important, however, is that collective agreements often stipulates that the employer and the union can negotiate about even stricter divisions of worker groups based on ‘similar work tasks’. It is also the case that the law explicitly states that an individual who is redundant with respect to his current work tasks, must be qualified enough for alternative tasks – whether this is the case is at the discretion of the employer. In particular, this last provision implies the following. Suppose a firm has ‘shortage of work’ with respect to task X, but not to task Y. Worker A is the one with shortest seniority performing task X, and worker B is the worker with shortest seniority performing task Y. In general, the firm should lay off worker B in this case, shifting worker A from task X to task Y – but the employer can

still lay off worker A if he is judged not to be qualified enough for task X (Wilhelmsson, 2001, p. 4).

Second, the share of temporary contracts has increased substantially during the last ten-year period. Thus, the employer has the possibility to screen new workers for a significant period of time, which makes hiring less risky.

A change in the EPA provision regarding the LIFO principle entered into force on 1 January 2001. After the reform, firms with 10 or fewer employees are allowed to exempt two workers from LIFO considerations, i.e., it can assign ‘key worker’ status to two individuals, who will then escape layoff even if they ought to have been the one to go if LIFO were followed strictly.

The chronology of the implementation of the reform is outlined in Lindbeck *et al.* (2006). Their Table 1 makes it clear that it is not likely that the reform was anticipated by actors in the labour market to any great extent – the reform was initiated and implemented against the will of the Social Democratic government, through a coalition between the liberal-conservative opposition and the Green Party, and it was unclear until late in 2000 exactly which firms would be eligible for this loosening of the LIFO requirement.

4 Data and empirical strategy

In this paper we use data at the firm-month level. Our data comes from the IFAU database, which consists of several data sets; one is the LOUISE from Statistics Sweden, which includes register information on the population aged 16–65 during the period 1985–2004. The IFAU database also includes the Employment register with yearly information on all Swedish firms, containing monthly start and end dates of all employments, along with identifiers for employers and employees.⁵ From these data sets we can construct measures on the employment stock as well as on worker flows

⁵ The phrase ‘monthly start and end dates of all employments’ should be understood as follows: within each yearly Employment register dataset, each employment is identified with its start and end months *within the year*. If the employment was continued from the preceding year, and continued into the following year, start and end months will be given as January and December respectively. Hence we can trace employments, and construct seniority measures, back to 1995 only, which is when the first Employment register data becomes available.

(i.e., hires and separations) for all firms on a monthly basis. By defining data on a monthly basis we measure the size of the firm with high precision, which is important in this paper since we want to distinguish between firms with ten and eleven employees.

When determining the size of a firm's work force in connection with a shortage-of-work situation, all employees are given the weight of 1 (e.g., part-time workers, temporary workers etc.).⁶ Employees who have a 'managerial position' should not be counted when defining the size of a firm's work force. We cannot identify which employees are managers, but we take this issue into account when constructing firm size figures in the following manner. Some 12 percent of firms in our sample have one or several 'entrepreneurs' associated with them. These 'entrepreneurs' are available in our data, and are never counted as employees. For firms with one or more 'entrepreneurs', we define firm size simply as the total number of employees in each month. For firms with no 'entrepreneurs', we argue that at least one of the employees must act as a manager; hence we reduce the baseline monthly firm size figure by one for all these firms.

Thanks to the individual register we also have information on the age, gender and country of birth for all worker flows. Moreover, since the Employment register is available to us back to 1995 we can trace whether workers have been employed in the same firm since that year (see also footnote 6). Thus, we can construct a measure of worker seniority which in 1999 is censored at 48 months. We restrict the population of firms used in the analysis to firms with 2–20 employees, whereof around 91 percent belong to the private sector. The analysis is further restricted to workers aged 18–64. The sample period used is 1999–2002. There is no information in the data on whether the employment contract is on a temporary or a permanent basis. This is not a problem when defining the firm size since the EPA stipulates that all types of contracts should be taken into account when defining firm size. Note, however, the seniority rule is only relevant for workers on permanent contracts. Furthermore, the seniority rule is only of importance when separations are involuntary, but in the data we cannot separate whether separations are voluntary, fires, or lay offs. It is therefore possible that

⁶ A couple of other narrowly defined groups are also excluded in the workforce count; see SFS 1982:80 for details.

voluntary separations to some extent disguise effects of the exemption rule. In a sensitivity analysis we will investigate this issue by only looking at small firms with at least three separations in a month. Since three separations in a month is a relatively unusual event – 3 percent of the sample – we argue that these separations consist of a larger share of involuntary separations. As an alternative we also look at observations where the difference between the number of separations and hires is at least two.

Theory predicts that a relaxation of employment protection will increase worker flows through increased hires and separations. Therefore, as a first outcome measure we use the absolute month-to-month employment change defined as

$$(1) \quad ABSE_{it} = \frac{|E_{it} - E_{it-1}|}{(E_{it} + E_{it-1})/2},$$

where E_{it} is the employment level for firm i in month t . Since firms are of different sizes it is important to normalize with respect to the employment level, here defined as the average employment level for the two months. We also analyse new hires and separations separately. We will use the share of new hires and the share of separations defined as the fraction of new hires (separations) to the firm size on a monthly basis. Table 1 shows descriptive statistics for firms across firm sizes.

Table 1. Mean characteristics by firm size 1999–2001.

<i>Firm size</i>	2	3	4	5	6	7	
No. of firms.	42,907	25,696	18,760	14,472	11,545	9,617	
Absolute empl. Change	0.108	0.087	0.078	0.071	0.065	0.061	
Share new hires	0.060	0.056	0.054	0.052	0.050	0.048	
Share separations	0.056	0.052	0.050	0.048	0.047	0.046	
<i>Firm size</i>	8	9	10	11	12	13	
No. of firms.	7,984	6,590	5,503	4,579	3,855	3,300	
Absolute empl. change	0.059	0.057	0.056	0.055	0.054	0.053	
Share new hires	0.048	0.048	0.048	0.048	0.048	0.047	
Share separations	0.045	0.045	0.045	0.045	0.045	0.045	
<i>Firm size</i>	14	15	16	17	18	19	20
No. of firms	2,784	2,456	2,190	1,877	1,696	1,562	1,348
Absolute empl. change	0.055	0.053	0.053	0.052	0.052	0.051	0.050
Share new hires	0.048	0.048	0.047	0.047	0.047	0.047	0.047
Share separations	0.045	0.045	0.044	0.044	0.044	0.044	0.045

The empirical strategy outlined is straightforward. We will start with a graphical analysis in Section 5 where we first show whether firms engaged in strategic behaviour in order to use the exemption rule. This would happen if firms with more than ten employees downsized in order to use this rule; or if firms with ten or fewer employees avoided expanding their workforce above the threshold of ten. Threshold effects also exist if newly created firms were systematically below the size of ten in order to use the exemption rule, i.e. threshold effects at the *extensive margin*. McCrary (2008) suggests a test of whether subjects under study manipulate the forcing variable determining treatment – i.e. firm size in our case.

The principle of this test is to estimate the distribution of the forcing variable and look for a discontinuous break around the cut-off (i.e., between firm sizes ten and eleven). Since firm size is a discrete variable, there may be discrete breaks in the distribution even if firm size were not manipulated. Our solution to this problem is to compare the firm size distribution when the exemption rule was in force (2001–2002) with the corresponding distribution before the exemption rule took place (1999–2000). If the two firm size distributions (before and after the reform) are equal, this indicates an absence of any threshold effects.

To further examine whether firms adjusted their size in order to use the exemption rule, we also analyse growth dynamics by firm size. If the exemption rule makes it less expensive to lay off workers, future lay-off costs would rise if a firm of size ten hires one additional worker compared to the case when a firm of size eleven hires one additional worker. For this reason, we would expect firms just below the threshold to be less likely to grow compared to other firms, i.e. firms at and above the threshold of eleven as well as firms smaller than ten. As regards the probability of downsizing, the theoretical predictions are less clear-cut. Firms just above the threshold have higher firing costs which might imply fewer incentives to downsize compared to a firm of size ten or smaller. At the same time, firms of sizes above ten would have incentives to reduce their work force in order to use the exemption rule. Knowledge of whether firms engaged in strategic behaviour is crucial because in absence of such type of behaviour it is straightforward to compare employment behaviour between firms around the threshold i.e., firms of sizes ten and eleven. Next, we continue with a graphical analysis

showing average worker flow measures by firm size and look for breaks between firms of sizes ten and eleven.

After the graphical analysis we estimate the quantitative effects of the exemption rule in Section 6 and establish to what extent the effects are statistically different from zero. We use the regression discontinuity (RD) approach, which basically means that we estimate whether there is a significant break in these worker flow measures between firms of sizes ten and eleven. The key assumption here, given that firms did not manipulate their size in order to benefit from the exemption rule, is that firms of sizes ten and eleven are statistically equal in all other respects, except for the fact that firms of size ten can exempt two workers from the LIFO principle but firms of size eleven can not. The advantage of this approach is that it provides estimates that are ‘as credible as those from a randomized experiment’ (Lee, 2008) under this assumption.

Using the RD approach means that we can only compare effects based on firms around the threshold implying that results can not be necessarily be generalised to all other firms. In fact, the exemption rule could have differential effects depending on the firm size. Choosing two ‘key workers’ provides more flexibility for a firm with three employees than for a firm with eight employees. If the firm with eight workers would like to lay off four workers, then the choice of the third and the fourth worker is as restricted as for a firm with more than 10 employees. Instead, the firm with three employees has no restrictions as it can choose the marginal worker it wants to fire assigning ‘key-worker’ status to the other two.

To examine differential effects by firm size we also provide an analysis using a difference-in-differences (DD) analysis. Here we will use firm data at the quarterly level for the period 1999–2002, where a firm is defined as treated if average quarterly firm size is ten or below. The control group is firms with an average quarterly firm size above ten. We then estimate whether there is a significant change for small firms relative to large firms in the employment measures when the reform takes place in 2001. The key assumption is that small and large firms would have had parallel trends in the employment measures in absence of the reform. This will be carefully investigated by inspecting the development of the employment measures for small and large firms before the reform. This approach also allows us to estimate analogous effects but now

separately for each firm size. We will here use firms with sizes 16–20 as the control group. Each estimation approach will be more explicitly stated in the subsequent analysis.

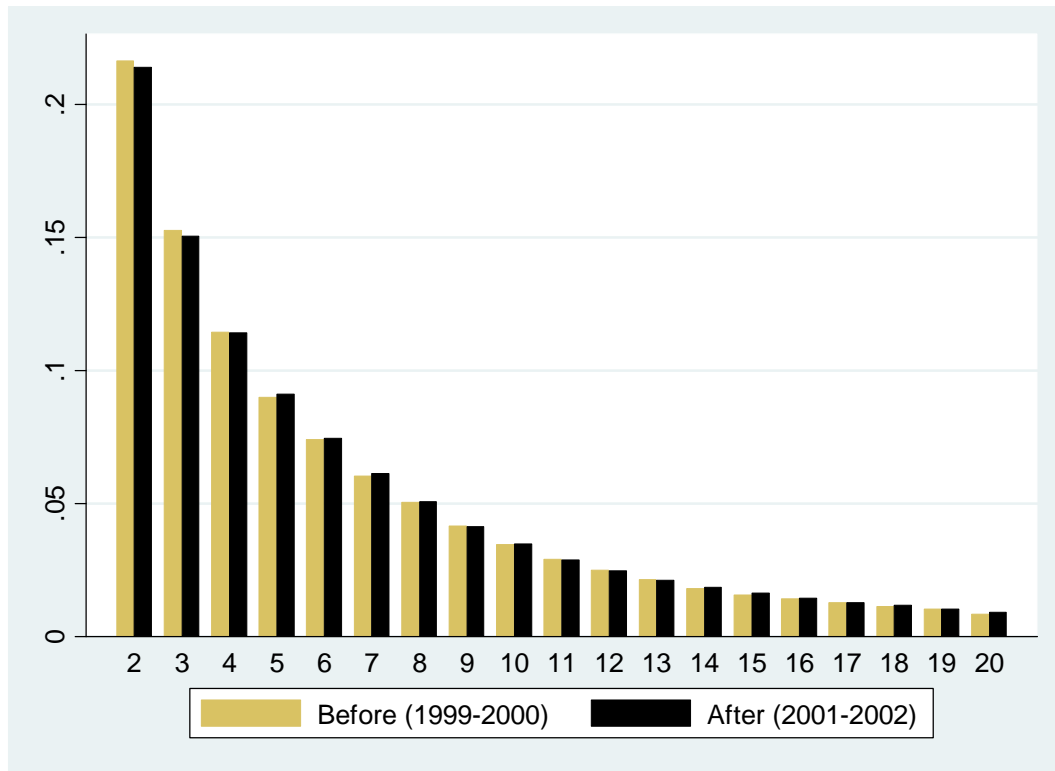
5 Graphical empirical analysis

5.1 Threshold effects of the exemption rule

Figure 1 shows firm shares by size before and after the reform. Small firms constitute the majority of the sample population. More than one-half of the firms have fewer than five workers; firms with ten or eleven workers constitute around six percent. If firms manipulated their size or if new ‘small’ firms were created in order to use the exemption rule, we would expect to find a discrete break in the shares of firms between ten or eleven workers. However, the density function seems to be a smooth negative function of firm size without any unusual discrete breaks. Moreover, the firm size distribution for the pre-reform period is remarkably equal to the distribution when the exemption rule was in force. This indicates that firms did not manipulate their size; nor were entering firms systematically of size ten or smaller.⁷

⁷ Note that this conclusion also holds if we exclude the smallest firms of size two and three from the size distribution.

Figure 1. Firm size distribution before and after the exemption rule



Next, we analyse whether firms just below the threshold were less likely to grow compared to all other firms, and whether firms above the size of ten around the threshold had different probabilities of reducing their workforce. *Figure 2* shows the probabilities of firm growth by initial firm size, before and after the LIFO reform. The probability of firm growth is defined as the fraction of firms that increased the size of their workforce (by any amount) from one month to another. In general, the probability of firm growth seems to increase with initial firm size. However, no clear downward jump in the probability of growing is found at the threshold value of ten. The pattern of growth probability is also very similar to the corresponding pattern before the reform.

Figure 2. Probability of firm growth by initial firm size, before and after the exemption rule

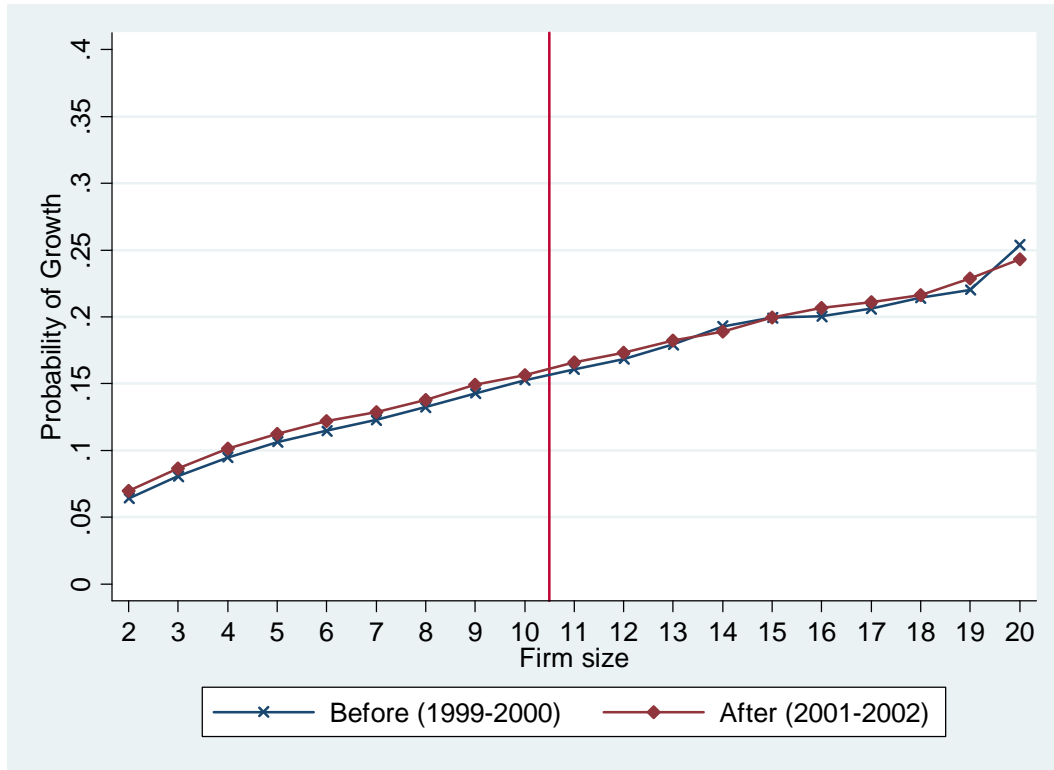
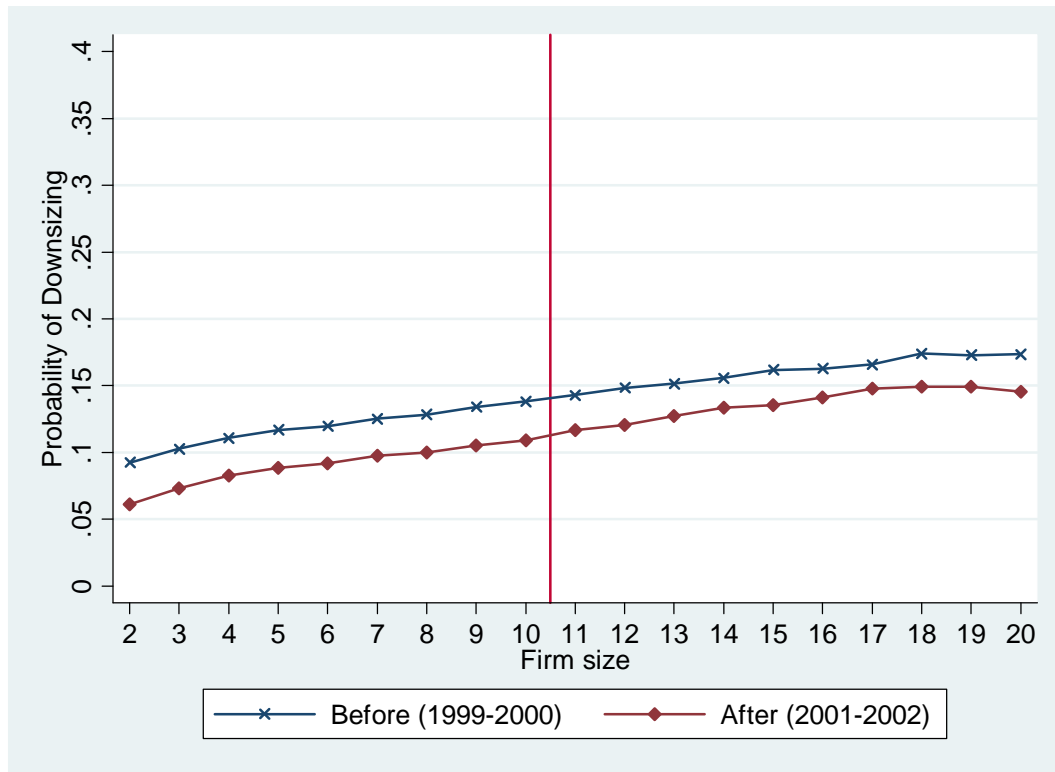


Figure 3 shows the probability of downsizing by (initial) firm size. The probability of downsizing is defined analogously to the probability of firm growth, i.e. the fraction of firms reducing their workforce (by any amount) between two months. The probability of downsizing is positively related to the initial firm size, but we find no systematic distortion of a smooth relationship around the threshold.

Figure 3. Probability of downsizing by initial firm size, before and after the exemption rule



Following Bauer *et al.* (2007), we have also tested whether there are any significant thresholds effects using the following regression model:

$$(2) \quad y_i = \alpha + f(\text{Firm size}_i) + \sum_k \lambda_k D(\text{Firm size}_i = k) + \varepsilon_i$$

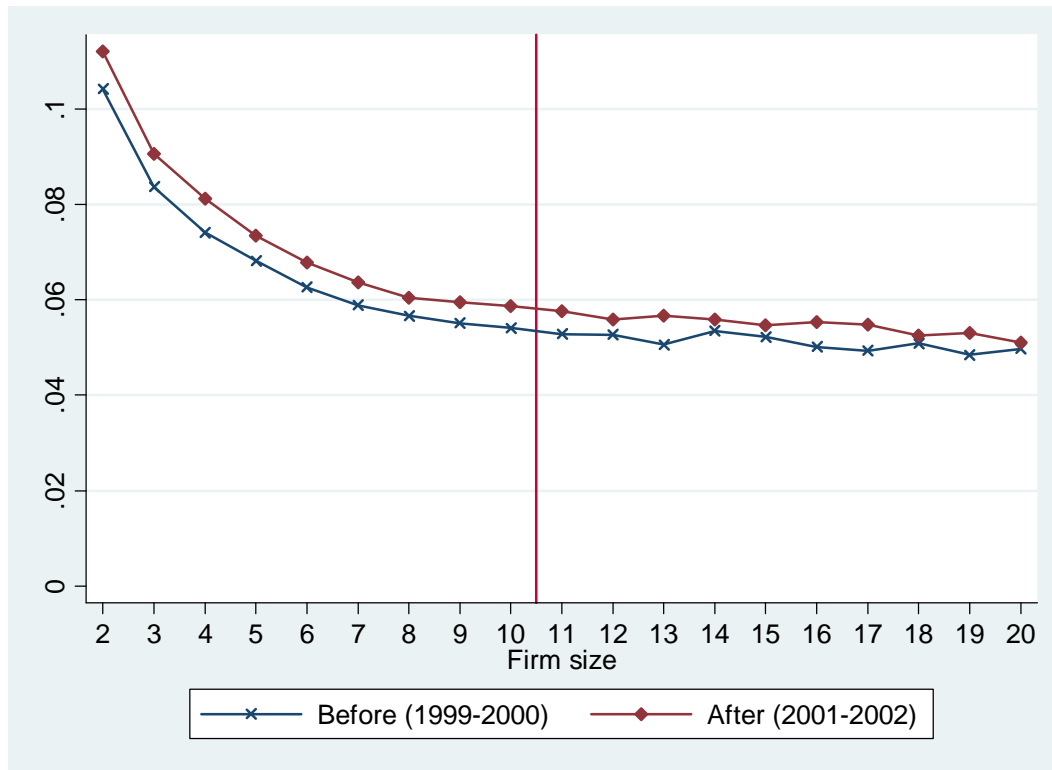
where y_i is an indicator variable for whether the firm i has increased or reduced the work force, $f(\text{Firm size}_i)$ is some polynomial function of firm size and $D(\text{Firm size}_i = k)$ is a set of indicator variables for sizes around the threshold. λ_k is supposed to capture any threshold effects provided that the function $f(\text{Firm size}_i)$ fits the smooth relationship between the outcome measure and firm size. Results from estimations do not show any significant threshold effects (results are available from the authors). Taken together, the analyses above indicate that the relaxation of the LIFO principle had no impact on firms' propensity to change their work force.

5.2 Effects on worker flows

Although we find no evidence that firms tried to ‘manipulate’ the size of their workforce, in order to fall or remain under the threshold level of ten employees, it is possible that worker flows were affected. Since firms demonstrably did not manipulate their workforce, it is straightforward to compare differences in employment behaviour between firms with ten and eleven employees during the period after the exemption rule entered into force (i.e., during 2001–2002). We will subsequently compare employment behaviour after the reform with the corresponding behaviour prior to the reform (i.e., in 1999–2000), following the same logic as in the analysis of threshold effects above.

Figure 4 shows the absolute month-to-month employment change, as defined by equation (1), separately before and after the reform and by firm size. In general, the absolute month-to-month employment change after the reform shows a negative relationship with firm size. However, no clear upward jump is found at the threshold value of ten. As such, employment volatility seems to be insensitive to whether firms can exempt two workers or not. This argument is also confirmed by inspection of the absolute employment change by firm size before the reform, which shows a remarkably equal pattern to the one after the reform.

Figure 4. Relative absolute employment change by firm size, before and after the exemption rule



If the exemption rule yielded an increase in new hires and separations of the same magnitude, this would not show up in the absolute employment change measure. For this reason we also analyse the changes in the shares of new hires and separations, separately, in the same way that we analysed the absolute employment change. *Figure 5 and 6* show the share of new hires and the share of separations by firm size, respectively. *Figure 5* shows that there are no differences in the shares of hires between firms with ten or eleven employees. In fact, the shares of new hires and separations are fairly flat across all firm sizes (except for firms with two employees). The reason that the share of new hires is larger before the reform is due to an improvement in the business cycle at that period. As regards the share of separations, *Figure 6* shows very small differences in the share of separations for firms of adjacent sizes. If anything, the share of separations is slightly higher for firms of size ten compared to firms of size eleven, which is consistent what we would have expected according to economic theory. However, this slight increase does not remain for firms below size ten.

Figure 5. Share new hires by firm size, before and after the exemption rule

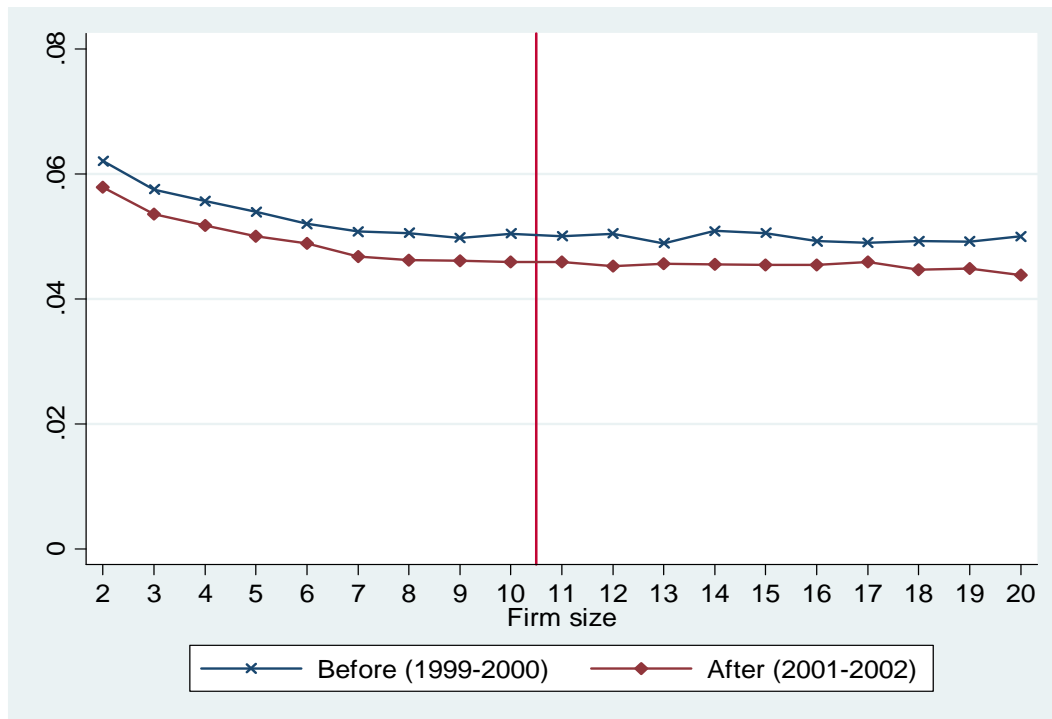
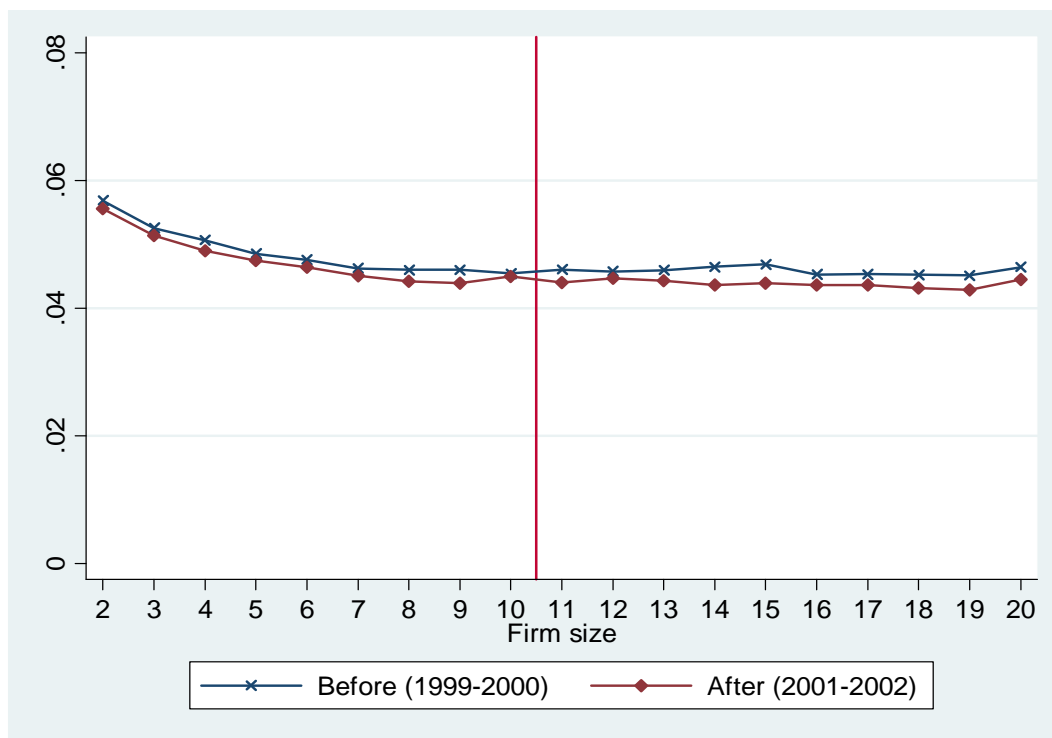


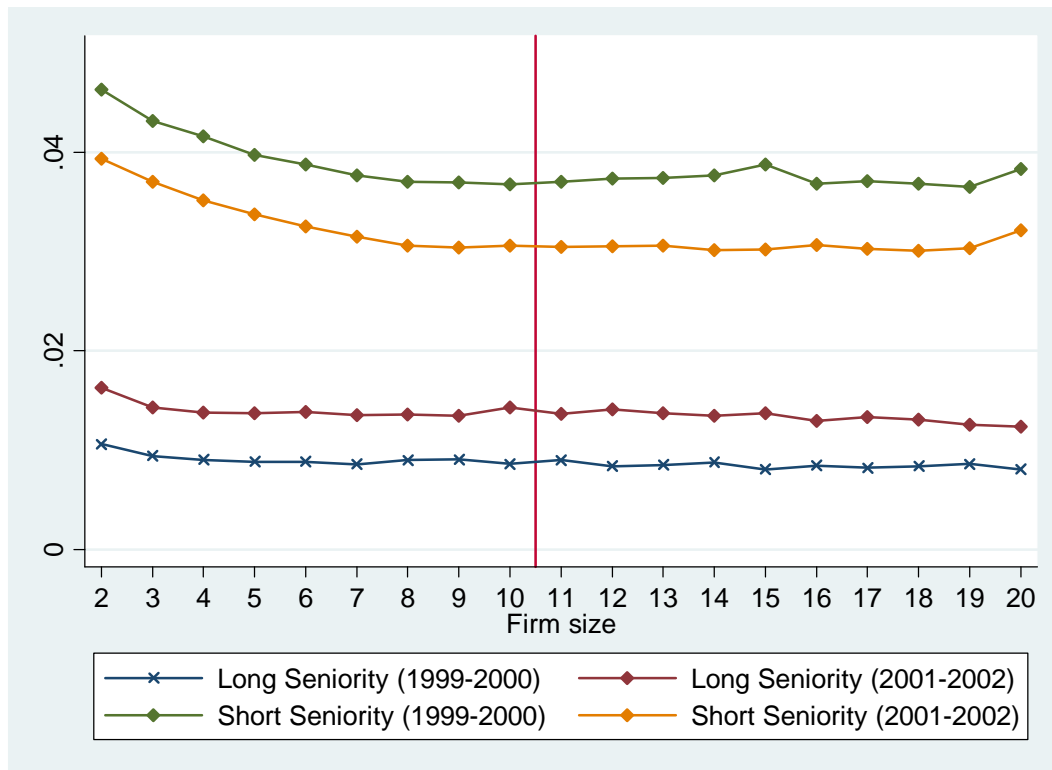
Figure 6. Share separations by firm size, before and after the exemption rule



As discussed in Section 2, different degrees of employment protection can have consequences for the composition of workers. One hypothesis is that the LIFO principle has consequences for *which* workers get laid off. For example, Buhai *et al.* (2008) addresses the question of whether the LIFO increases the bargaining power of the most senior workers. If increased bargaining power reduces the risk of being laid off we would expect separations among high seniority workers to be less likely in firms above the threshold of ten workers, which can not exempt two workers from the LIFO principle. *Figure 7* shows the share of separations by firm size, separately for workers with more than 48 months of seniority and with less than 48 months of seniority, respectively.⁸ According to *Figure 7* there is no pronounced break in the share separations at the cut-off, neither for workers with long seniority nor for workers with short seniority. If anything, there seems to be a slightly larger share of separations for workers with long seniority at firms of size ten.

⁸ The seniority threshold of 48 months was used because we do not have data further back than to 1995. Looking at an individual in January 1999 who as been employed at the same employer since 1995, we know that she has been employed for at least 48 months but we do not know if it is longer than that.

Figure 7. Share separations by firm size, before and after the exemption rule, for workers with more and less than 48 months of seniority, respectively.



Moreover, according to the theoretical discussion in Section 2, the exemption rule may improve employment prospects for young, older, immigrant and female workers. Inspecting separate figures by young (aged 18–25) and old workers (aged 55–64) and for female and immigrant⁹ workers, results reveal that the exemption rule did not change the composition of workers with respect to these characteristics, except that there seems to be a small discontinuous break between firm sizes eleven and ten as regards separations for older workers (figures for the period when the exemption rule was in place are shown in the appendix). The effect of older workers is also likely to be related to the finding of an effect on separations for workers with longer seniority since age and seniority are positively correlated.

⁹ Immigrants are defined as individuals born outside of the Nordic countries.

Taken together, the above graphical analysis indicates that the relaxation of the exemption rule for ‘small’ firms did not affect worker flows in any important way. The absence of any thresholds effects supports the idea of comparing employment behaviour around the threshold. Therefore, we next estimate the statistical effects of the exemption on worker flows using the RD approach.

6 Econometric analysis of worker flows

6.1 Regression discontinuity analysis

To estimate the statistical effects of the exemption rule on worker flows we use monthly data for the period 2001–02, and base our estimations on the following specification:

$$(3) \quad y_{imt} = \alpha + f(\text{Firm size}_{imt}) + \delta I[\text{Firm size}_{imt} \leq 10] + \lambda_m + \gamma_t + \varepsilon_{imt}$$

where y_{imt} is one of the worker flow measures (absolute employment change, share new hires and share separations) for establishment i in month m and year t , $f(\text{Firm size})$ is some polynomial function of firm size and $I[\text{Firm size} \leq 10]$ is an indicator variable taking the value 1 if a firm has ten or fewer workers (0, otherwise), and δ is the causal effect of the relaxation of the LIFO principle provided that the function $f(\text{Firm size})$ adequately fits the smooth relationship between the outcome measure and firm size. All month and year effects are represented by the parameters λ_m and γ_t , respectively.

Since we have the population of Swedish firms, implying a huge number of observations, we can simply estimate whether there are any significant difference in the outcome measure between firms of size ten and eleven. We can also increase the firm size bandwidth and include firms of size nine and twelve and also eight and thirteen. In order to estimate a potential discontinuous break between firms of size ten and eleven when we increase the bandwidth we have to control for the general relationship between firm sizes and the outcome measure of interest. In the results reported we include a linear and a squared term of firm size but results are insensitive to higher order of firm size polynomials. Since firms are observed repeatedly over the year, standard errors are clustered at the firm level.

Results from estimations of the exemption rule on the worker flow measures, using different firm size bandwidths, are reported in *Table 2*. As regards the absolute employment change and the share of new hires, results do not show any significant effects of the exemption rule. For the share of separations, the only significant effect – at the 10 percent significance level – is obtained when firms of sizes 9–12 are used in the estimation (see column 2). This is consistent with the pattern found in *Figure 6* and in accordance with what theory would predict about a less restrictive employment protection. However, the estimated coefficient of 0.0016 implies an estimated increase in the share of separations of 0.16 percentage points. Considering that the average share of separations for firms of size ten is 4.5 percent this amounts to an estimated increase of 3.5 percent. One might argue that this is a non-significant effect in economic terms, and that we have estimated a very precise effect due to a very larger number of observations. This brings us to the question of how calculate the correct standard errors due to potentially correlated errors within certain groups.

Table 2. Regression discontinuity estimates with different firm size windows

	Firms sizes 10 and 11	Firms sizes 9-12	Firm size 8-13
Absolute Employment change	0.0009 (0.0008)	-0.0005 (0.0013)	0.0004 (0.0010)
Share new hires	-0.0002 (0.0005)	-0.0007 (0.0008)	-0.0003 (0.0006)
Share separations	0.0007 (0.0005)	0.0016* (0.0008)	0.0006 (0.0006)
<i>Observations</i>	204,602	416,679	647,019

Estimations include month dummies and a year dummy. Standard errors are clustered at the firm level. Three asterisks indicate statistical significance at the 1% level; two asterisks for the 5% level, and one asterisk for the 10% level. When the employment change measure is used, the number of observations is slightly lower since the measure is based on a time difference (for column 1 it is: 204,144; column 2: 415,708; column 3: 645,488).

The consequence of not taking into account correlated errors within certain groups is that standard errors are likely to be downward biased. Since all estimates, except for the share of separations in column 2, are insignificant this issue is perhaps irrelevant. Anyway, we will we check what happens with the effect on share separations if we account for correlated errors within firm size. This is equivalent to relying on aggregated data on

size levels as presented in Figure 6. We therefore perform OLS on the following equation:

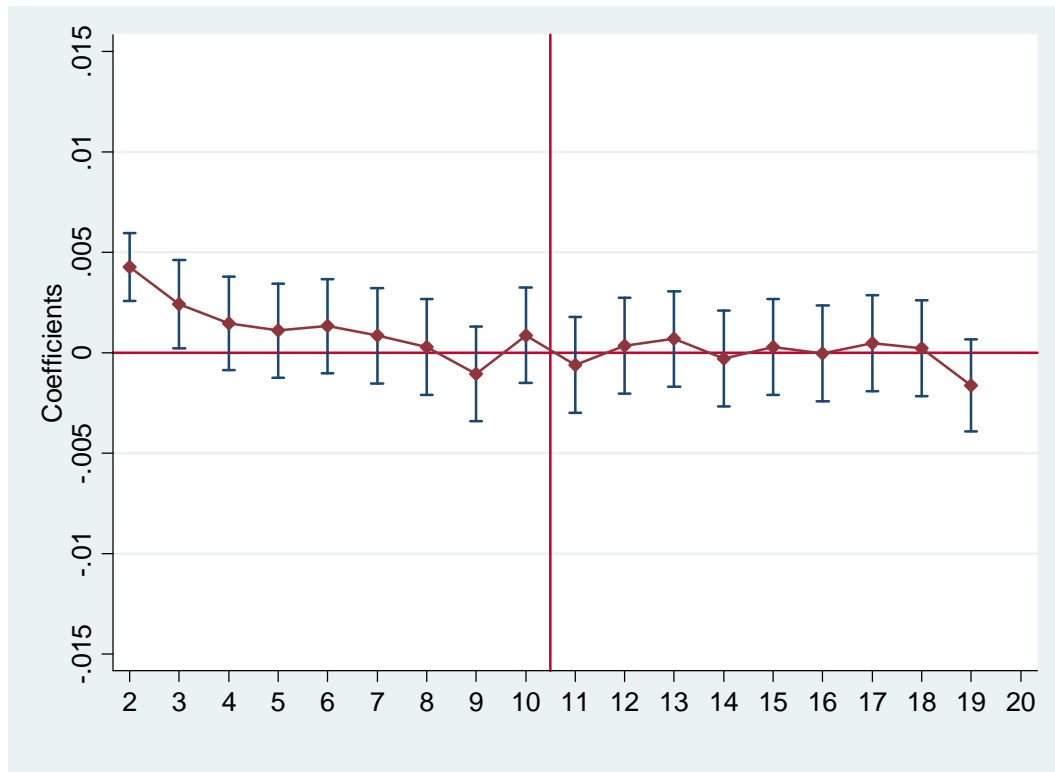
$$(4) \quad \bar{y}_s - \bar{y}_{s+1} = \delta_s I[size = s] + \varepsilon_s,$$

where \bar{y}_s is the share of separations for firms of size s , $I[size=s]$ is an indicator variable taking the value 1 if firm size is s (0 otherwise), and δ_s captures the immediate jump in employment change between firms of sizes s and $s+1$. This setup means that when we examine whether there is a discontinuous break between firms of size ten and eleven, s is equal to ten. Since we can estimate eighteen such changes of y , one for each difference between adjacent firm sizes, we can check whether the estimated effect between firms with ten and eleven employees is likely to be drawn from a different distribution in contrast to all other estimated differences between adjacent firm sizes. All estimations are robust to any form of correlated errors with each firm size (such as correlations between industries and regions within the same size level). Note that we only use post-reform data (the years 2001 and 2002) in these estimations.

Figure 8 shows the estimated differences between adjacent firm sizes, with corresponding 90 percent confidence intervals, for the share of separations (we use 90 percent confidence intervals here since the corresponding effect reported in Table 2 was only significant the 10 percent significance level). The estimated difference between sizes ten and eleven is positive and somewhat more positive than other estimated differences between adjacent firm sizes around the neighbourhood of firm sizes (say 8–12). More importantly, however, the estimated 90 percent confidence interval contains the zero, implying that the effect is not significantly different from zero.¹⁰

¹⁰ We have also performed an analogous analysis for the absolute employment change and the share of new hires and all effects are found to be insignificant.

Figure 8. Estimated RD effects of the exemption rule on share separations between adjacent firm sizes with corresponding 90 Percent confidence Interval.



Since the graphical analysis shows a tendency of an effect for older workers and workers with longer seniority, we turn to estimations of heterogeneous effects. Results are presented in *Table 3*. The only effects significant at the 1 percent level are effects on the share of separations for older workers and workers with longer seniority. For older workers this turns out to be the case when firm sizes ten and eleven are included. The increase in the share separations for older workers is 0.04 percentage points. Since the share separations for older workers is about 0.75 percent this amounts to a 5 percent increase in separations due to the exemption rule.

As regards seniority of at least 48 months, this holds when firm sizes 9–12 are used. Standard errors reported in *Table 3* are robust to errors correlated within firms. If we take into account correlated errors within firms size by estimating equation (4), the estimated difference in separations between firm size ten and eleven is not significant anymore, neither when considering older workers nor for workers with longer seniority

– the estimated 95 percent confidence intervals contain the zeros (results are available from the authors). For both groups of workers the estimated differences in the share of separations between firm size ten and eleven are, however, larger than all other estimated differences between adjacent firms sizes (except for the differences between firm sizes two and three and between three and four, which are so far from the cut-off of value it is difficult to make any inferences from such differences). For this reason, we conclude that it can not be completely ruled out that the exemption rule slightly increased separations for older workers and workers with longer seniority, even if estimates based on equation (4) are not significant.

Table 3. Regression discontinuity estimates with different firm size windows, separately by groups

	Firm sizes 10 and 11	Firm sizes 9-12	Firm size 8-13
<i>Young</i>			
Share hires	-0.0004* (0.0002)	-0.0005 (0.0004)	-0.0002 (0.0003)
Share separations	-0.0002 (0.0002)	-0.00005 (0.0003)	-0.0001 (0.0003)
<i>Old</i>			
Share hires	0.00009 (0.0002)	-0.0002 (0.0003)	0.00003 (0.0002)
Share separations	0.0004*** (0.0002)	0.0004 (0.0003)	0.0004* (0.0002)
<i>Immigrants</i>			
Share hires	-0.0001 (0.0001)	-0.0002 (0.0002)	-0.0001 (0.0002)
Share separations	-0.00001 (0.0001)	0.00008 (0.0002)	0.000004 (0.0001)
<i>Female</i>			
Share hires	-0.0002 (0.0003)	-0.0003 (0.0005)	0.0004 (0.0003)
Share separations	0.0001 (0.0003)	0.0005 (0.0005)	0.000006 (0.0003)
<i>Seniority ≥ 48 Months</i>			
Share separations	0.0006** (0.0003)	0.0013*** (0.0005)	0.0005 (0.0003)
<i>Seniority < 48 Months</i>			
Share separations	0.0001 (0.0005)	0.0003 (0.0007)	0.0001 (0.0006)
<i>Observations</i>	204,602	416,679	647,019

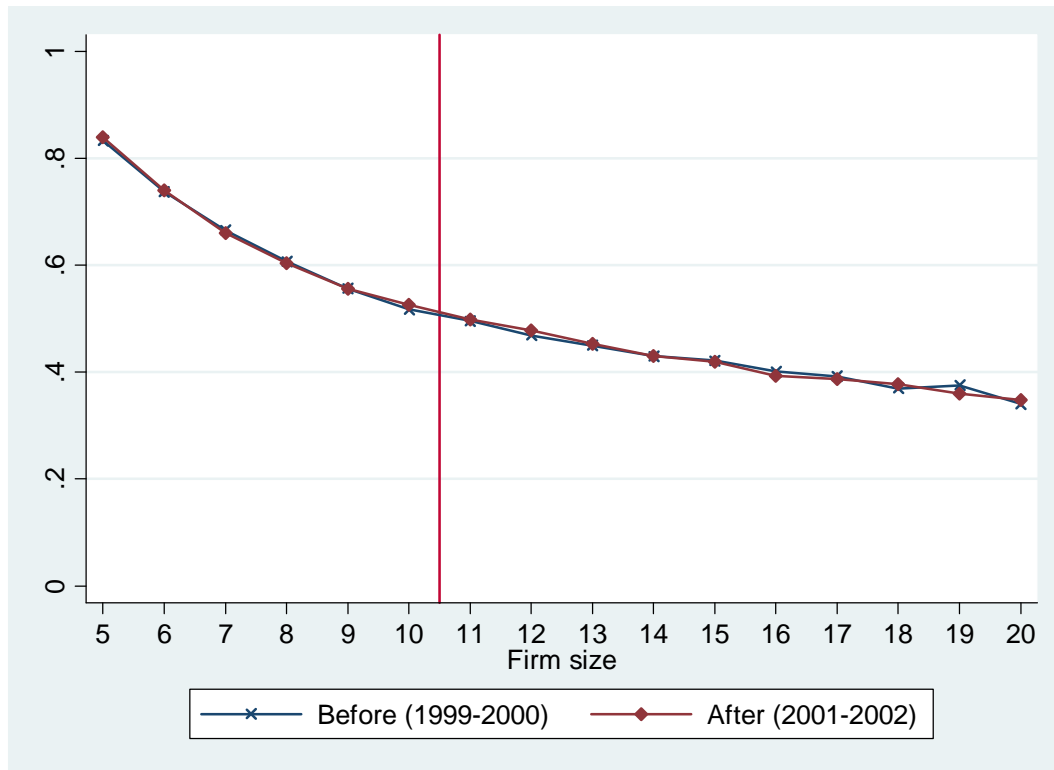
See Table 2.

6.2 Estimations for firms with at least three separations

The effect on the share of separations has been this far been based on all separations, i.e., voluntary as well as involuntary. As discussed in Section 3, we can not separate between voluntary, fires and lay offs. Since the exemption rule is only of importance when separations are involuntary, it might be the case that voluntary separations disguise any effects of the exemption rule on lay offs due to ‘shortage of work’. For this reason we have estimated the effect of the exemption rule on the share of separations for the subsample of firm-months with at least three separations (around 3 percent of the total sample). For these firms it is likely that a substantial amount of the separations are involuntary. *Figure 9* shows the average share of separations by firm size for firms with at least three separations. We also estimated the effects of the relaxation of the LIFO principle using this subpopulation sample relying on equations (3) and (4). According to our results, no effects are significant.¹¹

¹¹ We also used the same subpopulation sample and estimated effects for different worker compositions i.e., on older workers aged 55–64, workers aged 18–25, female workers, for immigrant workers and for workers with different seniority levels. No effects are significant at any conventional significance levels. We have also performed an analogous analysis for the share of hires but no effects appear significantly different from zero. Results are available from the authors.

Figure 9. Share separations by firm size for firm-months with at least three separations, before and after the exemption rule



It might be the case that three separations per firm and month do not to a larger extent capture separations due to shortage of work. It could for example just reflect bad working conditions. As an alternative analysis we have therefore used observations where the difference in the number of separations and hires is at least two. Neither in this case do we find a significant effect of the exemption rule.¹²

6.3 Difference in differences analysis

Using firm-quarterly data as repeated cross-sections for the period 1999–2002, we also use the DD approach where a firm is defined as treated if average quarterly firm size is ten or below. The estimations are based on the following regression model:

$$(5) \quad y_{iq} = \alpha + \gamma D_{iq} + \rho After_q + \delta D_{iq} \times After_q + \lambda_q + \varepsilon_{iq}$$

¹² We have also performed an analysis of heterogeneous effect with respect to 59 industries (based on a two-digit industry classification). There are no clear-cut results from such an analysis. Only in about 5 percent of the cases do we find any significant effects, and sometimes negative effects. It could also be argued that we might have found significant effects in 5 percent by chance.

where y_{iq} is the outcome measure for firm i in quarter q , D_{iq} is a dummy variable taking the value 1 if the firm has ten or less workers in quarter q , $After_q$ is a dummy variable taking the value 1 for the period when the exemption rule was in place (1st quarter in 2001 and onwards), and 0 otherwise. Time (quarter) effects are represented by λ_q . Under the assumption that ‘small’ and ‘large’ firms would have parallel outcome trends in absence of the exemption rule, δ is the causal effect of the exemption rule.

The informal way to examine the parallel outcome trend assumption is to inspect the development of the outcome measure for the treated and control groups, respectively. Figure 10-12 show the quarterly development of absolute employment change, share of hires and separations, respectively, for small and large firms. As shown by these figures, the two groups seem to have parallel trends in these employment measures before the introduction of the exemption rule. The figures also reveal that there is no relative increase for small firms in the absolute employment change, new hires nor separations. This is also confirmed by the DD estimates from equation (5), reported in Table 4, where all estimates are insignificant at the 5 percent significance level. The effect on the share of new hires is significant at the 10 percent level but negative, which is opposite to what we would expect according to the theoretical prediction. In fact, a significant effect at the 10 percent level can hardly be considered as significant since we have more than 2 million observations in the estimation. Neither is the effect significant in economic terms.¹³ DD estimates, separate by groups (young and old, female and immigrants), not presented here, are all insignificant.

¹³ One major concern with the Difference-in-differences approach is the issue of understatement of standard errors as recently discussed by Donald and Lang (2007), Bertrand *et al.* (2004) and Conley and Taber (2007). Difference-in-differences estimations above ignore potential correlation between firms within the treatment and control groups. One way to solve this problem is to rely on the aggregated data as presented in Figures 10–12. Results from fixed-effect estimations based on these aggregated data show no significant effects. Results are available from the authors upon request.

Figure 10. Development of absolute employment change by quarters during 1999–2002.

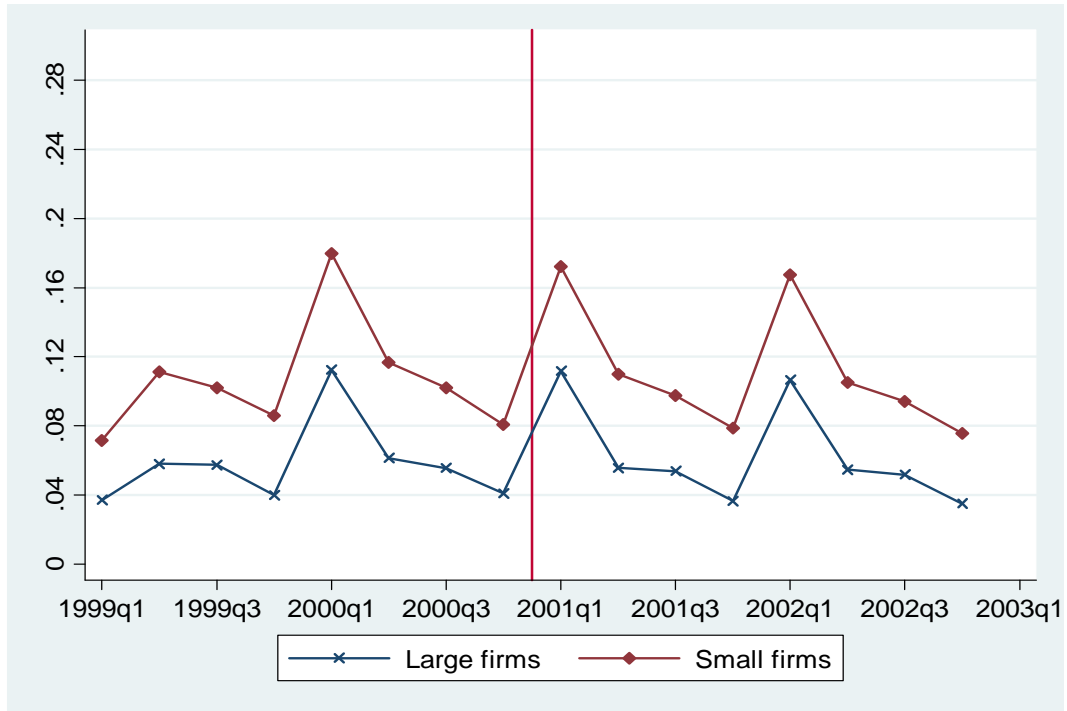


Figure 11. Development of share new hires by quarters during 1999–2002.

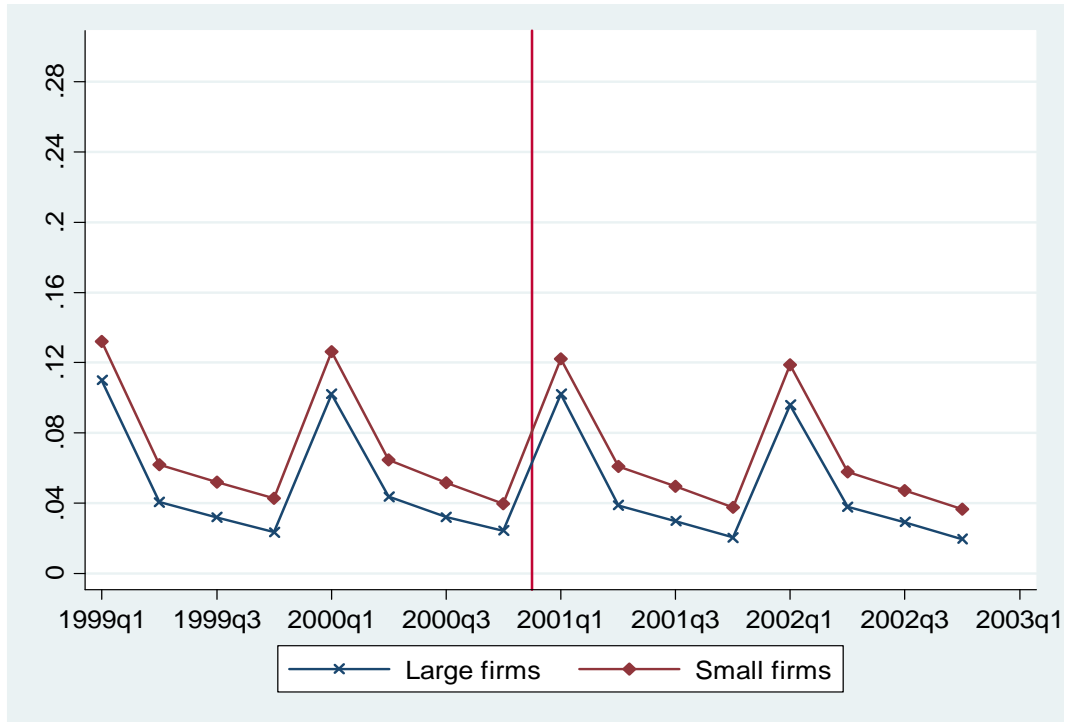


Figure 12. Development of share separations by quarters during 1999–2002.

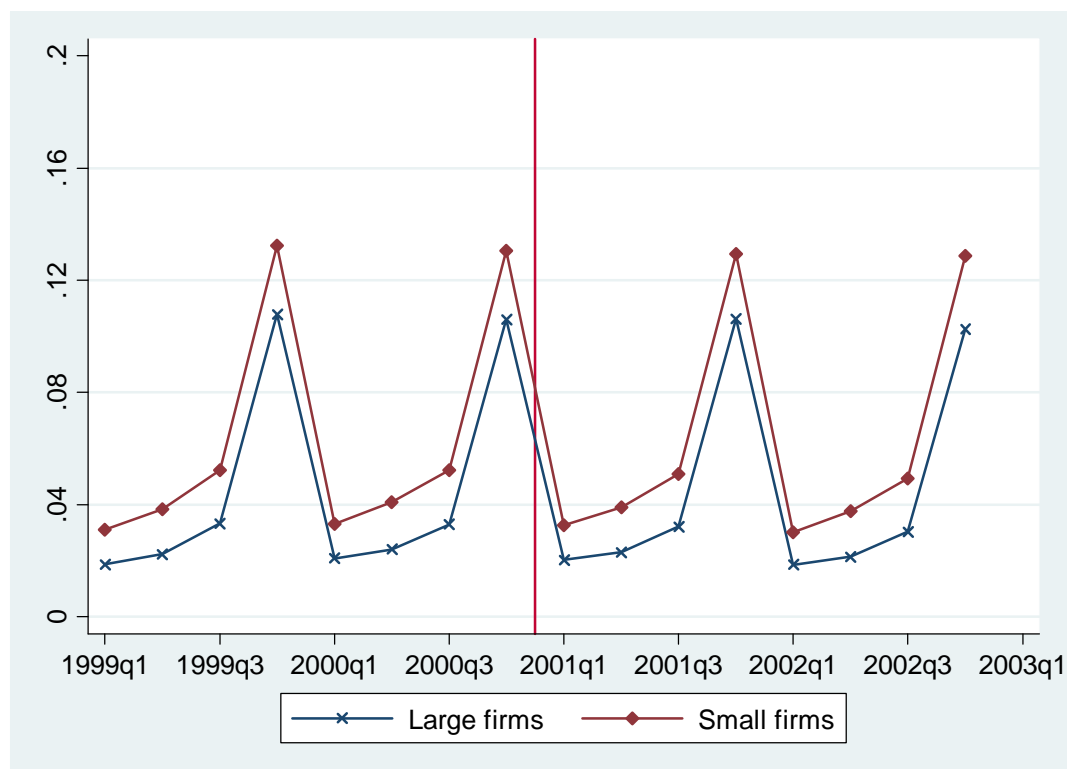


Table 4. Difference-in-differences estimates of the 2001 reform in the LIFO principle.

	Employment change	Share hires	Share separations
DD-effect of the exemption rule	0.0010 (0.0007)	-0.0007* (0.0004)	-0.0002 (0.0003)
Observations	2,210,490	2,212,247	2,212,247

Estimations include quarter dummies. Standard error clustered on firm level.

6.4 Heterogeneous effects with respect to firm size

Using the DD approach we can also perform a more careful analysis on how the possibility to nominate two employees as ‘key workers’ affects lay-off decisions. As discussed in Section 4, the effect of the relaxation of the LIFO principle could potentially have different effects depending on the firm size. To examine this we allow for separate effects for each firm size by estimating the following equation:

$$(6) \quad y_{iq} = \alpha + \lambda_q + \sum_{s=2}^{15} \gamma_s D_{iqs} + \rho After_q + \sum_{s=2}^{15} \delta_s D_{iqs} \times After_q + \varepsilon_{iq}$$

where D_{iqs} is a dummy variable taking the value 1 if the firm is of size s (0 otherwise). The parameter δ_s measures how the outcome has changed for a firm of size s relative to the reference group of firms (sizes 16–20) after the relaxation of the LIFO principle. Provided that all firm sizes have parallel trends in the outcome, δ_s measures the causal effect of the exemption rule.

We present results using the share of new hires and share of separations as the dependent variables. Standard errors are clustered on the firm level.

Figure 13 shows the estimated coefficients from equation (6) with corresponding 95 percent confidence intervals when the share of new hires is used as dependent variable. *Figure 14* shows the corresponding coefficients with the share of separations as the dependent variable. As shown in these two figures, there are no significant effects at all for any of the firm sizes in the relevant neighbourhood of the cut-off.

Figure 13. DD estimates of the exemption rule on the share new hires by firm size with corresponding 95 percent confidence intervals. Reference Group: Firms of sizes 16–20.

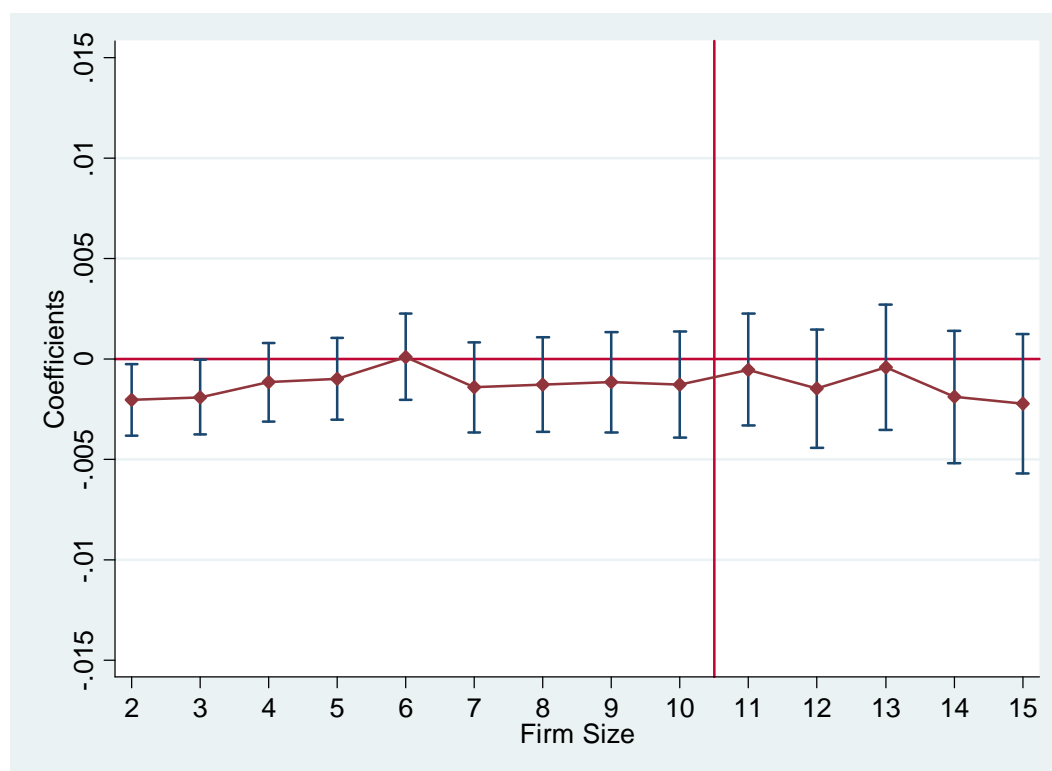
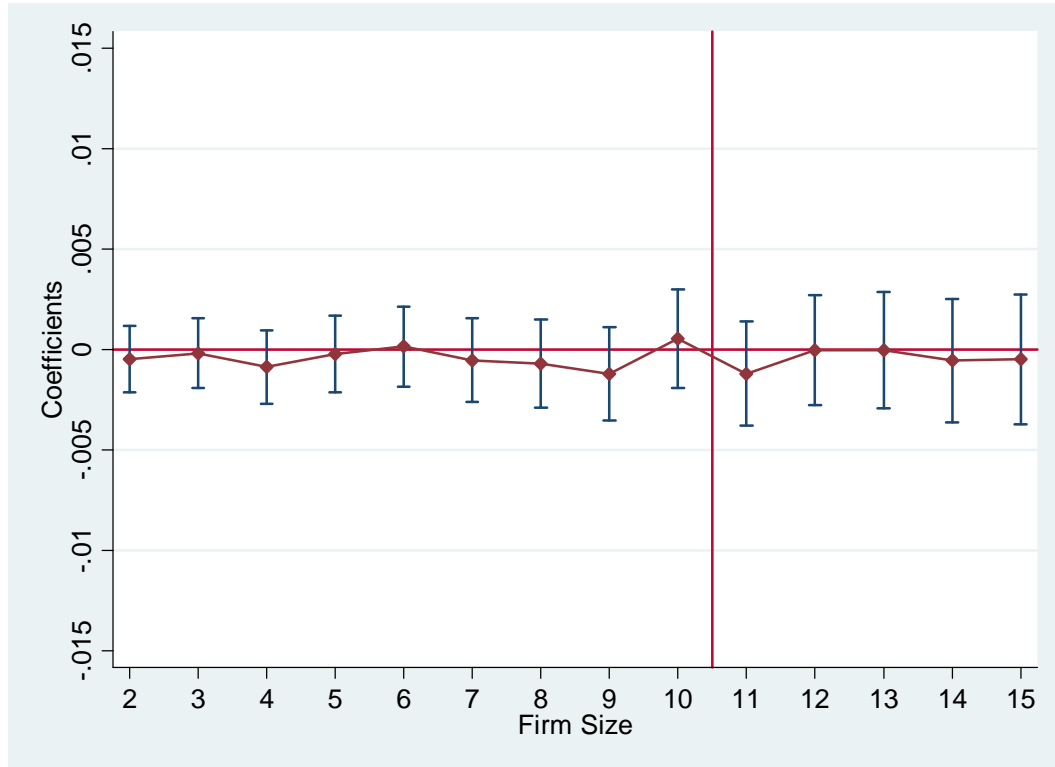


Figure 14. DD estimates of the exemption rule on the share of separations by firm size with corresponding 95 percent confidence intervals. Reference Group: Firms of sizes 16–20.



7 Discussion and conclusion

How employment protection affects firms' employment behaviour is vital to the design of policy and to the understanding of labour market behaviour. This paper empirically investigates how employment protection affects firms' employment behaviour by focusing on a certain aspect to the legislation, namely a seniority rule. Previous empirical research often relies on cross-country variation by correlating some strictness index measure of employment protection with the employment level. One major challenge with cross-country studies is to obtain comparable measures of strictness, since legislation varies widely across countries. We therefore argue that it is also important to evaluate certain aspects of the employment protection legislation such as the seniority rule. Seniority rules exist in several countries such as Finland, France, Italy, Mexico, the Netherlands, Norway, the UK and the US.

In Sweden the seniority rule states that the worker who was employed last has to go first when a firm downsizes, i.e., the so-called LIFO principle. This principle has received much attention, both from a political and an academic point of view – where opponents argue, e.g., that unemployment will result because employers become more reluctant to hire new workers. For this reason, the LIFO principle was reformed in January 2001 so that employers with ten or fewer employees were allowed to exempt two workers from the seniority rule. Using unique data including the population of firms matched with the population of workers for the period 1999–2002, the break in the policy for firms of size ten provides a natural setting for analysing the impact of the exemption rule using a regression discontinuity (RD) approach.

The paper contributes to the literature on employment protection and employment behaviour by focusing on the effect of a seniority rule on firms' strategic behaviour and on workers flows. There is also limited knowledge of how exemption rules for smaller firms in the employment protection legislation in general affect employment behaviour. Moreover, previous studies often rely on small and unrepresentative data sets, which makes it difficult to generalise from results and it is not always clear whether non-robust results are due to insignificant effects or due to a small number of observations. Our results show that the exemption had no effect at all on the size distribution of firms and we do not find any general effects on employment fluctuations or on hires and separations. The only exceptions is a tendency of an increase in the share of separations for older workers aged 55-64 and for workers with longer seniority, but these effects are not robust to how standard errors are calculated and the effects are small in economic terms.

The result in this paper is interesting not only because it is the first paper evaluating the consequences of the LIFO principle on firms' employment behaviour, but it also points to the importance of considering in detail how legislation is formulated and how it works in practice. According to Calleman (2000) and Wilhelmsson (2001), employers can to a large extent circumvent the LIFO principle and keep a valuable worker through negotiations with the union. When there is shortage of work with respect to certain work tasks, the LIFO principle is supposed to hold within a certain clientele of workers working with these, or comparable, tasks. Since the legislation is negotiable it is always

possible for the firm and the union to define such a clientele of workers in a very narrow way. As such, a worker who is considered as valuable for the firm can be defined as belonging to a clientele of workers not affected by the LIFO principle. It is not hard to imagine a situation where it is in the union's interest that a recently hired person stays with the firm. Consider a small firm with, say, five workers, exposed to hard competition and with work shortage with respect to certain work tasks. If the alternative to down-sizing is bankruptcy it would be in both the union's and the firm's interest to let the newly hired person stay if his or her contribution to the firm is extremely valuable. In addition, even within a group of workers affected by the principle, an employer may keep a worker with shorter seniority, by arguing that this worker is qualified for certain work tasks whereas another worker with longer seniority is not. It is then this latter worker who will be laid off.¹⁴ Moreover, the share of temporary contracts has increased substantially during the last ten-year period. Thus, the employer has the possibility to screen new workers for a significant period of time, which makes a hiring less risky.

Taken together, one interpretation of the findings in this paper is that the seniority rule does not matter much since there are ways for the employer to circumvent it.

¹⁴ When the employer has not signed any collective agreement and when none of the employees is union member, the union has no influence on the employer's employment decision. In such a case it is possible that the exemption rule has no effect at all if the employer makes all employment decisions. If the exemptions were important we would yet have found effects since small firms do sign collective agreements even if to a smaller extent than larger firms. This issue can not be further investigated in the paper since we do not have information on whether collective agreements are signed or not.

References

- Addison, J. and P. Teixeira (2003), “The economics of employment protection”, *Journal of Labour Research*, 24, 85–129.
- Autor, D. H., J. Donohue and S. J. Schwab (2004), “The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?”, *American Economic Review, Papers and Proceedings*, 93 (2), May, 440–446.
- Autor, D. H., J. J. Donohue and S. J. Schwab (2006), “The Costs of Wrongful-Discharge Laws,” *Review of Economics and Statistics*, 88(2), May, 211–231.
- Autor, D., W., Kerr and A. Kugler (2007), “Do Employment Protections Reduce Productivity? Evidence from U.S. States,” *Economic Journal*, 117, F189–F217.
- Bassani, A. and R. Duval (2006), “Employment Patterns in OECD Countries: Reassessing the Role of Policies and Institutions”, Working paper No. 35, Directorate for Employment, Labour and Social Affairs, OECD, Paris.
- Bauer, T. K., S. Bender and H. Bonin (2007), “Dismissal Protection and Worker Flows in Small Establishments”, *Economica*, 74, 804–821.
- Bertola, G. (1990) “Job Security, Employment, and Wages,” *European Economic Review*, 54(4): 851–879.
- Bertola, G., Blau, F., and L. Kahn (2008), “Labor Market Institutions and Demographic Employment Patterns”, forthcoming in *Journal of Population Economics*.
- Bertrand, M., Duflo, E., and S. Mullainathan (2004), “How Much Should We Trust Difference-in-Differences Estimates,” *Quarterly Journal of Economics*, 119, 249–275.
- Boeri, T. and J. Jimeno (2005), “The effects of employment protection: learning from variable enforcement”, *European Economic Review*, 49, 2057–77.
- Borgarello, A., P. Garibaldi and L. Pacelli (2004), “Employment protection and the size of firms”, *Giornale degli economisti e annali di economia*, 63, 33–68.

- Botero, J., S. Djankov, R. La Porta, F. Lopez-de-Silanes, and A. Shliefer (2004), "The Regulation of Labor", *Quarterly Journal of Economics*, 119, 1339-1382.
- Buhai, S., Portela, M., Teulings, C., and A. van Vuuren (2008). "Returns to Tenure or Seniority?", IZA Discussion Paper No. 3302.
- Calleman, C. (2000), *Turordning vid uppsägning*, Nordstedts Juridik AB. Stockholm.
- Conley, T. and C. Taber (2007), "Inference with "difference in differences" with a small number of policy changes". Manuscript.
- Donald, S., and K. Lang (2007), "Inference with Difference in Differences and Other Panel Data," *Review of Economics and Statistics*, 89, 221-233.
- Heckman, J., and C. Pagés-Serra (2000), "The Cost of Job Security Regulation: Evidence from Latin America Labor Markets", *Economica*, 1, 109-144.
- Howell, D. R., Baker, D., A. Glyn and J. Schmitt (2007), "Are Protective Labor Market Institutions at the Root of Unemployment? A Critical Review of the Evidence", *Capitalism and Society*, 2, 1-71.
- Imbens, G. and T. Lemieux (2008), "Regression Discontinuity Designs: A Guide to Practice," *Journal of Econometrics*, 142 (2), 615-635.
- Kugler, A. (2004), "The Effect of Job Security Regulations on Labor Market Flexibility: Evidence from the Colombian Labor Market Reform", Working Paper No 10215, NBER, Cambridge. MA.
- Kugler, A. (2007), "The Effects of Employment Protection in Europe and the USA," *Centre de Recerca en Economia Internacional*, 18, 1-48.
- Kugler, A. and G. Pica (2008), "Effects of Employment Protection on Job and Workers Flows: Evidence from the 1990 Italian Reform," *Labour Economics*, 15 (1), 78-95.
- Kugler, A., J. F. Jimeno and V. Hernanz (2003), "Employment Consequences of Restrictive Permanent Contracts: Evidence from Spanish Labor Market Reforms. CEPR Discussion Paper No. 3724.

- Lazear, E. (1990) "Job Security Provisions and Employment," *Quarterly Journal of Economics*, 105(3): 699–726.
- Lee, D. S. (2008), "Randomised Experiments From Non-random Selection in U.S. House Elections," *Journal of Econometrics*, 142 (2), 675–697.
- Lemieux, T. and K. Milligan (2008), "Incentive Effects of Social Assistance: A Regression Discontinuity Approach," *Journal of Econometrics*, 142 (2), 807-828.
- Lindbeck, A., Palme, M. and M. Persson (2006), "Job Security and Work Absence: Evidence from a Natural Experiment," Manuscript, International Institute for Economics Studies, Stockholm.
- Martins, P. (2007), "Dismissals for Cause: The Difference That Just A Eihgt Paragraohs an Make", Discussion Paper No 3112, IZA, Bonn.
- McCrary, J. (2008), "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 142 (2), 698–714.
- Miles, T. (2000), "Common Law Exceptions to Employment at Will and US Labor Markets", *Journal of Law, Economics, and Organisation*, 16, 74-101.
- Mortensen, D. and C. Pissarides (1999). New developments in models of search in the labour market. In R. O. Ashenfelter and D. Card (eds.), *Handbook of Labour Economics*, vol. 3B, Amsterdam: Elsevier Science.
- OECD. 2003. *Employment Outlook*. Paris: OECD
- OECD. 2004. *Employment Outlook*. Paris: OECD.
- Olsson, M. (2007), "Employment Protection and Sickness Absence," IFN Working Paper No. 717, Research Institute of Industrial Economics.
- Pissarides, C. (2001), "Employment protection", *Labour Economics*, 8, 131–59.
- Schivardi, F. and R. Torrini (2004), "Threshold Effects and Firm Size: The Case of Firing Costs. CEP Discussion Paper no. 633, London.
- SFS 1982:80, Lag om anställningsskydd, Statens författningssamling.

- Skedinger, P. (1995) "Employment Policies and Displacement in the Youth Labour Market," *Swedish Economic Policy Review*, 2, 135–171.
- Wilhelmsson, K. (2001), Kan turordningsreglerna anses fylla sin function som skydd mot godtyckliga uppsägningar?, Dissertation, Department of Law, Lund University.
- Verick, S. (2004), "Threshold Effects of Dismissal Protection Legislation in Germany", IZA Discussion Paper no 991, Bonn.

Appendix

Figure A1. Share new hires and separations by firm size for young and old during the period 2001-2002.

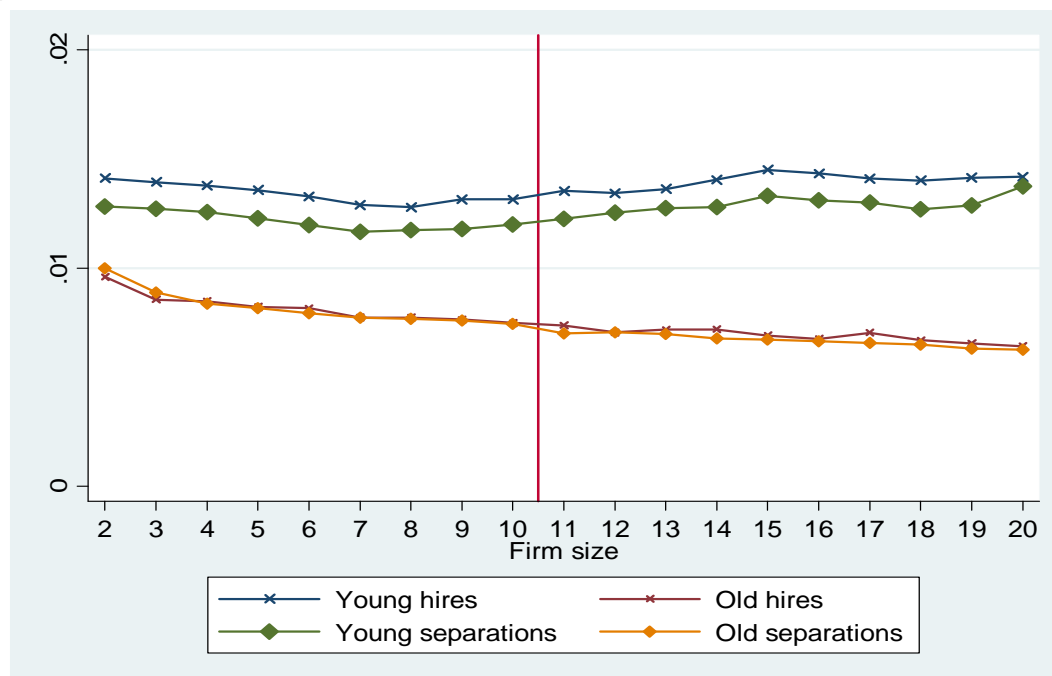
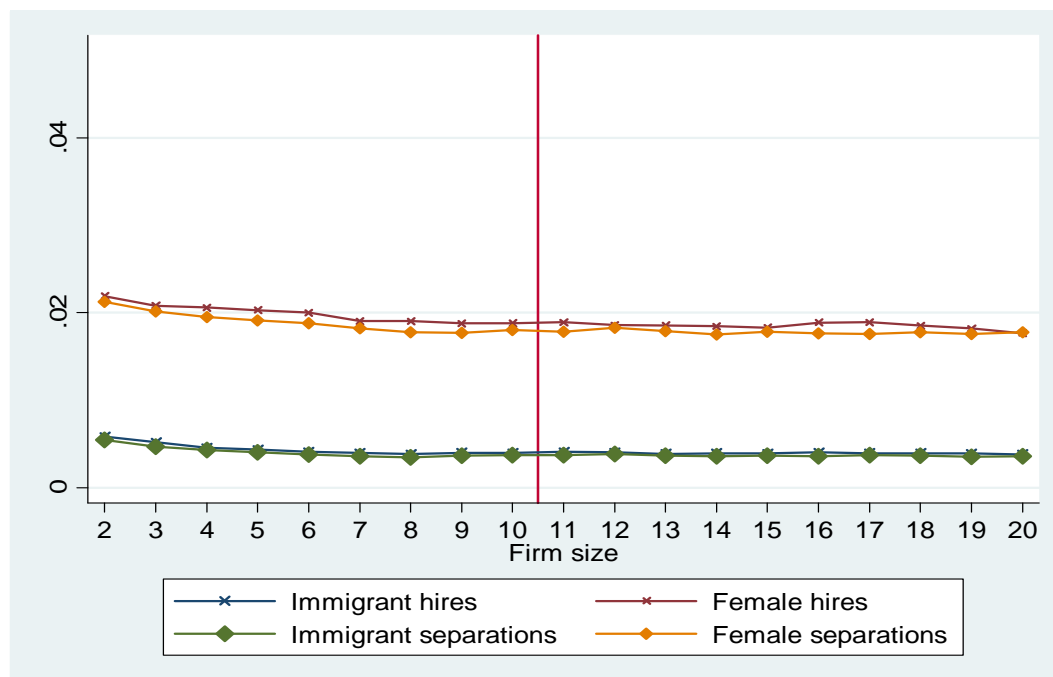


Figure A2. Share new hires and separations by firm size for immigrants and females during the period 2001-2002.



Publication series published by the Institute for Labour Market Policy Evaluation (IFAU) – latest issues

Rapporter/Reports

- 2008:1** de Luna Xavier, Anders Forslund and Linus Liljeberg "Effekter av yrkesinriktad arbetsmarknadsutbildning för deltagare under perioden 2002–04"
- 2008:2** Johansson Per and Sophie Langenskiöld "Ett alternativt program för äldre långtidsarbetslösa – utvärdering av Arbetstorget för erfarna"
- 2008:3** Hallberg Daniel "Hur påverkar konjunktursvängningar förtida tjänstepensionering?"
- 2008:4** Dahlberg Matz and Eva Mörk "Valår och den kommunala politiken"
- 2008:5** Engström Per, Patrik Hesselius, Bertil Holmlund and Patric Tirmén "Hur fungerar arbetsförmedlingens anvisningar av lediga platser?"
- 2008:6** Nilsson J Peter "De långsiktiga konsekvenserna av alkoholkonsumtion under graviditeten"
- 2008:7** Alexius Annika and Bertil Holmlund "Penningpolitiken och den svenska arbetslösheten"
- 2008:8** Anderzén Ingrid, Ingrid Demmelmaier, Ann-Sophie Hansson, Per Johansson, Erica Lindahl and Ulrika Winblad "Samverkan i Resursteam: effekter på organisation, hälsa och sjukskrivning"
- 2008:9** Lundin Daniela and Linus Liljeberg "Arbetsförmedlingens arbete med nystartsjobben"
- 2008:10** Hytti Helka and Laura Hartman "Integration vs kompensation – välfärdsstrategier kring arbetsoförmåga i Sverige och Finland"
- 2008:11** Hesselius Patrik, Per Johansson and Johan Vikström "Påverkas individen av omgivningens sjukfrånvaro?"
- 2008:12** Fredriksson Peter and Martin Söderström "Vilken effekt har arbetslöshetsersättningen på regional arbetslöshet?"
- 2008:13** Lundin Martin "Kommunerna och arbetsmarknadspolitiken"
- 2008:14** Dahlberg Matz, Heléne Lundqvist and Eva Mörk "Hur fördelas ökade generella statsbidrag mellan personal i olika kommunala sektorer?"
- 2008:15** Hall Caroline "Påverkades arbetslöshetstiden av sänkningen av de arbetslösas sjukpenning?"
- 2008:16** Bennmarker Helge, Erik Mellander and Björn Öckert "Är sänkta arbetsgivaravgifter ett effektivt sätt att öka sysselsättningen?"
- 2008:17** Forslund Anders "Den svenska jämviktsarbetslösheten – en översikt"
- 2008:18** Westregård Annamaria J. "Arbetsgivarens ökade ansvar för sjuklön och rehabilitering kontra arbetstagarnas integritet – Går det att förena?"
- 2008:19** Svensson Lars "Hemmens modernisering och svenska hushålls tidsanvändning 1920–90"

- 2008:20** Johansson Elly-Ann and Erica Lindahl "Åldersintegrerade klasser – bra eller dåligt för elevernas studieresultat?"
- 2008:21** Sibbmark Kristina "Arbetsmarknadspolitisk översikt 2007"
- 2008:22** Delmar Frédéric, Tim Folta and Karl Wennberg "Dynamiken bland företagare, anställda och kombinatörer"
- 2008:23** Angelov Nikolay, Per Johansson and Louise Kennerberg "Välja fritt och välja rätt – drivkrafter för rationella utbildningsval"
- 2008:24** Dahlberg Matz, Kajsa Johansson and Eva Mörk "Effekter av aktiveringskrav på socialbidragstagare i Stockholms stadsdelar"
- 2008:25** Grönqvist Erik and Jonas Vlachos "Hur lärares förmågor påverkar elevers studieresultat"
- 2008:26** von Below David and Peter Skogman Thoursie "Sist in först ut? En utvärdering av undantagsregeln"

Working papers

- 2008:1** Albrecht James, Gerard van den Berg and Susan Vroman "The aggregate labor market effects of the Swedish knowledge lift programme"
- 2008:2** Hallberg Daniel "Economic fluctuations and retirement of older employees"
- 2008:3** Dahlberg Matz and Eva Mörk "Is there an election cycle in public employment? Separating time effects from election year effects"
- 2008:4** Nilsson J Peter "Does a pint a day affect your child's pay? The effect of prenatal alcohol exposure on adult outcomes"
- 2008:5** Alexius Annika and Bertil Holmlund "Monetary policy and Swedish unemployment fluctuations"
- 2008:6** Costa Dias Monica, Hidehiko Ichimura and Gerard van den Berg "The matching method for treatment evaluation with selective participation and ineligibles"
- 2008:7** Richardson Katarina and Gerard J. van den Berg "Duration dependence versus unobserved heterogeneity in treatment effects: Swedish labor market training and the transition rate to employment"
- 2008:8** Hesselius Patrik, Per Johansson and Johan Vikström "Monitoring and norms in sickness insurance: empirical evidence from a natural experiment"
- 2008:9** Verho Jouko, "Scars of recession: the long-term costs of the Finnish economic crisis"
- 2008:10** Andersen Torben M. and Lars Haagen Pedersen "Distribution and labour market incentives in the welfare state – Danish experiences"
- 2008:11** Waldfogel Jane "Welfare reforms and child well-being in the US and UK"
- 2008:12** Brewer Mike "Welfare reform in the UK: 1997–2007"
- 2008:13** Moffitt Robert "Welfare reform: the US experience"
- 2008:14** Meyer Bruce D. "The US earned income tax credit, its effects, and possible reforms"
- 2008:15** Fredriksson Peter and Martin Söderström "Do unemployment benefits increase unemployment? New evidence on an old question"

- 2008:16** van den Berg Gerard J., Gabriele Doblhammer-Reiter and Kaare Christensen "Being born under adverse economic conditions leads to a higher cardiovascular mortality rate later in life – evidence based on individuals born at different stages of the business cycle"
- 2008:17** Dahlberg Matz, Heléne Lundqvist and Eva Mörk "Intergovernmental grants and bureaucratic power"
- 2008:18** Hall Caroline "Do interactions between unemployment insurance and sickness insurance affect transitions to employment?"
- 2008:19** Bennmarker Helge, Erik Mellander and Björn Öckert "Do regional payroll tax reductions boost employment?"
- 2008:20** Svensson Lars "Technology, institutions and allocation of time in Swedish households 1920–1990"
- 2008:21** Johansson Elly-Ann and Erica Lindahl "The effects of mixed-age classes in Sweden"
- 2008:22** Mellander Erik and Sofia Sandgren-Massih "Proxying ability by family background in returns to schooling estimations is generally a bad idea"
- 2008:23** Delmar Frédéric, Timothy Folta and Karl Wennberg "The dynamics of combining self-employment and employment"
- 2008:24** Dahlberg Matz, Kajsa Johansson and Eva Mörk "On mandatory activation of welfare receivers"
- 2008:25** Grönqvist Erik and Jonas Vlachos "One size fits all? The effects of teacher cognitive and non-cognitive abilities on student achievement"
- 2008:26** van den Berg Gerard, Annette Bergemann and Marco Caliendo "The effect of active labor market programs on not-yet treated unemployed individuals"
- 2008:27** von Below David and Peter Skogman Thoursie "Last in, first out? Estimating the effect of seniority rules in Sweden"

Dissertation series

- 2007:1** Lundin Martin "The conditions for multi-level governance: implementation, politics and cooperation in Swedish active labor market policy"
- 2007:2** Edmark Karin "Interactions among Swedish local governments"
- 2008:1** Andersson Christian "Teachers and Student outcomes: evidence using Swedish data"