

Last-in First-out? Estimating the Effect of Seniority Rules in Sweden^Ω

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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. The seniority rules have received increased attention recently; opponents have argued that these rules induce inefficient adjustment costs on firms’ employment decisions. Several aspects of this principle have often been overlooked in this debate, however, in the sense that there are several ways for firms to circumvent the “last-in-first-out” principle. The break in the policy for firms of size ten provides a natural setting for analysing the impact of the relaxation of the LIFO principle using a regression discontinuity approach. Using population data on all firms with two to twenty employees, we find that the change in the LIFO principle had no effect at all on the size distribution of firms and we do not find any general effects on employment fluctuations or on hirings and separations.

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

JEL Classification: E24; J63; J65

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1. Introduction

This paper empirically investigates how employment protection affects firms' employment behaviour. The main motivation for employment protection is to protect workers from unfair dismissals. In the US, the employment-at-will doctrine was replaced at different points in time across states by wrongful-discharge protections (see e.g., Autor *et al.* 2007). How employment protection affects the labour market functioning is a controversial subject (see e.g., OECD 2004). Opponents against strong employment protection argue that more lenient protection would improve the efficiency of the labour market. On the other hand, employment protection is a welfare arrangement which provides economic security to workers; ultimately increasing the value of work. How employment protection affects firms' employment behaviour is vital to the design of policy and to the understanding of labour market behaviour.

Theory suggests that employment protection imposes worker adjustment costs for employers which reduces incentives for firms to lay off workers and makes them more careful in their hiring decisions. As such, theory predicts that employment protection decreases employment fluctuations through reduced hirings and separations. The net effect on employment (or unemployment) is therefore *a priori* ambiguous. Employment protection may also affect 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 suggests differential effects on different groups of workers. A relaxation of employment protection could increase the demand for workers with lower attachment to the labour market such as young, older, immigrant and female workers.¹

Earlier empirical research often relies on cross-country variation by correlating some strictness index measure of employment protection with the employment level.² Results from these studies are mixed. A few recent studies, most based on U.S. data, exploit changes in employment protection within a country over time and also focus on a wider array of outcome measures. Results from these studies can be summarized in the following way. First, increased strictness of employment protection reduces employment fluctuations and employment. Second, increased strictness affects the composition of workers through decreased employment prospects for female, young and unskilled workers. Some evidence also suggests that increased protection substitutes capital for labour and therefore increases labour productivity.

¹ For example, Skedinger (1995) shows that countries with stricter employment protection have higher teenage unemployment.

² For an overview of previous studies, see Kugler (2007).

One main concern with cross-country studies is to obtain comparable measures of strictness, since legislation and rules vary widely across countries (see *e.g.*, Howell *et al.*, 2007; Kugler 2007; OECD 2004; Lazear 1990; Bertola 1990). We therefore argue that it is also important to evaluate certain aspects of the employment protection legislation. One such aspect is seniority rules which protect workers with long seniority. In Sweden, this is formulated as the “last-in-first-out” (LIFO) principle which is one main cornerstone in the Swedish Employment Protection Act (EPA, *Lagen om anställningsskydd*, SFS 1982:80, 22 §). This principle implies that the worker who was employed last has to go first when a firm downsizes. As such, this principle introduces a cost to the employer since the employer can not necessarily keep the most valuable workers.³ 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 such that employers with ten or fewer employees were allowed to exempt two workers from the seniority rule. Despite this strong focus on the seniority rules, consequences of the LIFO principle on employer behaviour have never been empirically investigated.

Evaluating the consequences of the change in the LIFO principle is also interesting since it points to the importance of considering in detail how legislation is formulated and how it works in practice. There are several aspects of this principle that often are overlooked in this debate. First, the LIFO principle holds within a defined clientele of workers (*turordningskrets*) with comparable work tasks and the employers can circumvent the “last-in first-out” principle and keep a valuable worker through negotiations with the union. Usually the outcome of the negotiation is not very costly for the employer since the redundancy payment to the leaving worker is often financed through a collective insurance organized by collective agreements.⁴

Second, the last-in first-out principle only holds if the worker with long seniority is qualified for other work tasks at the firm. Thus, if the work task to be closed due to shortage of work is performed by a worker with long seniority, this worker still has to leave the firm if he or she is not qualified enough for other works tasks at the firm. It is the employer who

³ A few earlier studies have investigated the effect of the introduction of the Swedish EPA without focusing on a certain aspect of the Act. Results from these studies are mixed (see Holmlund 1978, 1980; Edin and Holmlund 1993). Two studies based on interviews with employers found that for some employers the EPA increased hiring cost and made employers more careful in their hiring decisions (Agell and Lundborg 1995; Dahlqvist and Jonsson 2000). However, these studies are not based on a representative sample of Swedish employers.

⁴ An interesting question is how the union negotiates in situations of down-sizing. 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.

decides whether the worker has required qualifications. Thus, the employer can still keep a valuable worker with short seniority if other workers with longer seniority are not qualified enough.

Third, 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.

The purpose of this paper is to estimate the effect of a relaxation of the LIFO principle on worker flows, hirings and separations. Our hypothesis is that the LIFO principle has no effects on firms' employment behaviour since there are so many other possibilities for the firms to circumvent the principle.⁵ The break in the policy for firms of size ten provides a natural setting for analysing the impact of the relaxation of the LIFO principle 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 2007) under relatively weak conditions.⁶ One reason for this advantage is that we do not study a reform per se, but a discontinuity present in a static policy. In our case this means that we will compare employment behaviour between firms with ten and eleven employees. As such, there is no bundle of other reforms that may contaminate the evaluation of the low benefit policy. Moreover, we do not need to make assumptions about the comparability of the treated group to a control group. The commonly used difference-in-differences estimator has been found to perform poorly with inappropriately chosen control groups (see e.g., Lemieux and Milligan 2007). In our case we would have to choose large firms, i.e., firms with eleven or more employees, as the control group to small firms. Large and small firms are likely to operate in different labour markets (regions, industries etc.) implying that we would have to worry about differential changes in broad economic environment across small and large firms.

Using population data on all firms with two to twenty employees, we find that the change in the LIFO principle had no effect at all on size distribution of firms. Thus, firms did not adjust the size of their workforce in order to benefit from the new LIFO principle. Therefore, the regression discontinuity approach is an appropriate strategy to use in order to

⁵ The 2001 reform of the LIFO principle has previously been used to estimate the effect on individual sickness absence behaviour (see Lindbeck *et al.* 2006; Olsson 2007). Results suggest that sickness absence decreased, at least in the short run. This paper focuses on employer behaviour rather than worker behaviour. This is an important difference since workers might have different information on the real consequences of the change in the LIFO principle.

⁶ The regression discontinuity approach has recently become increasingly popular in economics, mainly because of the perception that the identifying assumptions are quite weak. For an overview of the regression discontinuity design and recent studies, see Imbens and Lemieux (2007). To name a few examples of other studies applying the regression discontinuity approach, see van der Klaauw (2002) who looks at the effect of students aid offers on college enrolments, Black (1999) who studies parents' valuation of elementary schools, and Lee (2007) who looks at the electoral advantage to incumbency.

estimate the effect on firms' employment behaviour. We do not find any general effects on employment fluctuations or on hirings and separations.

The remainder of the paper is organized as follows. In Section 2 we describe the seniority rule and data. Section 3 deals with the consequences of the change in the seniority rule. Discussion and conclusions are given in Section 4.

2. The Seniority Rule Reform and Data measures

The 1982 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). However, as discussed in the Introduction, there are several reasons why the LIFO principle can be circumvented.

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 though 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 an unlikely 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.

Our data come from the IFAU database, which is based on register information from Statistics Sweden on the entire Swedish population aged 16–65 during the period 1985–2004. We use the Employment register which contains information on all employments, matched with the longitudinal database LOUISE which includes individual register information.⁷ From this data we can construct measures of firms’ monthly employment flows, where information such as age, gender and county of birth are available for each individual. One advantage with this unique data is that we avoid measuring firm size based on yearly averages. Firms size on monthly basis distinctly measure the correct size of the firms which is important in this paper since we want to distinguish between firms with ten and eleven employees. We restrict the analysis to the population of firms with 2-20 employees and restrict the information to workers aged 18-64. Around 91 percent of these firms belong to the private sector. The sample period used is 1999-2002.

As theory suggests, a relaxation of employment protection increases job flows through increased hirings and separations, implying an *a priori* ambiguous effect on the employment level. Therefore, the first outcome measure we use is 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 size 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 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 about here

⁷ The Employment register gives the start and end months of all employments, along with identifiers for employers and employees. LOUISE is a register based data set on the total Swedish population which includes information on, among other things, income and education.

3. Consequences of Changes in Employment Protection

The first question to answer is whether firms manipulated the size of their workforce in order to benefit from the relaxation of the LIFO principle. This would happen if firms with more than ten employees downsized in order to benefit from the new LIFO principle; or if firms with ten or fewer employees avoided expanding their workforce. In addition, a relaxation of the LIFO principle might also have increased the entry rate of small firms. The question whether firms manipulated their workforce size is also crucial for using the regression discontinuity approach, which consists in comparing outcomes for subjects close to the cutoff value of the forcing variable (i.e., the variable that determines whether a subject is treated or not). In our case the forcing variable is firm size, which in a deterministic way defines firms as treated if they fall below the cutoff value of eleven employees.

One main concern in the RD design is that subjects under study may be able to manipulate the forcing variables. McCrary (2007) suggests a formal test of whether subjects under study manipulate the forcing variable determining treatment. The principle of this test is to estimate the distribution of the forcing variable. If there is a discrete jump in the distribution around the cutoff, this indicates that subjects under study manipulate the forcing variable. Since the firm size distribution is a discrete variable, there may be discrete jumps in the distribution even if firms do not manipulate their workforces. Our solution to this problem is to compare with the corresponding distribution before the reform of the LIFO principle. If the firm size distributions are the same before and after the reform, this indicates that firms do not manipulate the size of their workforce.⁸ This would also detect whether there are any changes at the extensive margin due to the reform.

Figure 1 shows the firm size distributions before and after the reform. Firms centred around the treatment cutoff, i.e., firms with ten or eleven workers, constitute around 6 percent each of all firms in the sample. More importantly, there does not seem to be any particularly large discrete change in the shares of firms with ten or eleven workers, which we would have expected if firms manipulated their workforce in order to benefit from the relaxation of LIFO principle. The absence of such firm behaviour is also confirmed when we compare the firm size distribution after 2001 with the pre-2001 distribution. The two distributions are remarkably equal, which strongly supports the hypothesis that the relaxation of employment protection had no impact on firms' propensity to change the size of their workforce.

⁸ The formal version of the test suggested McCrary (2007) is to run a regression of the number of firms and test whether there is a significant jump in the number of firms between ten and eleven. Since the firm size distribution is discrete, significant differences might appear even if firms did not manipulate their workers force. But since the distributions before and after the 2001 reform are virtually identical this is sufficient for concluding that firms did not manipulate their workforce.

Figure 1 about here

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 when the relaxation of the LIFO principle was in place (i.e., during 2001-2002). To further strengthen our case, we also compare with the corresponding employment behaviour during the pre-reform period (i.e., in 1999-2000). Figure 2 shows the absolute month-to-month employment change (as defined in Equation 1), separately before and after the reform and by firm size.⁹

As shown in Figure 2, the absolute employment change after the reform is equal for firms with ten and eleven workers. As such, employment volatility seems to be insensitive to whether firms can exempt two workers or not from the LIFO principle. 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. The only difference is that employment volatility is slightly higher for all firm sizes in the after the reform. This would appear to be due to a general improvement in the business cycle in the post-reform period.

One concern may be that firm size is measured with error, such that firms measured as size ten and eleven employees, respectively, do not distinctly distinguish between firms that can benefit from the exemption of the LIFO principle and firms that can not. As revealed in Figure 2, however, the pattern of absolute employment change is also flat for firm sizes between 9 and 12. Therefore, if we compare absolute employment changes between firms with nine and twelve employees, in order to more distinctly compare treated with non-treated firms, results are basically unaffected. Note that a comparison further away from the cutoff is inappropriate since absolute employment change seems to be negatively correlated with firm size. Taken together, the above analysis indicates that the relaxation of the LIFO principle for “small” firms did not affect employment volatility.

Figure 2 about here

If the exemption rule from the LIFO principle yielded a change in new hires as well as separations of the same magnitude, this would not show up in the absolute employment

⁹ The underlying measures are constructed on a monthly basis, and then averaged on a two-year basis: before (1999 and 2000) and after (2001 and 2002).

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. Figures 3 and 4 show the share of new hires and the share of separations by firm size, respectively. Figure 3 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). As regards the share of separations, Figure 4 shows a slightly lower share of separations for firms with ten employees compared firms with eleven employees. If anything, the effect of a relaxation in the LIFO principle is in the opposite direction of what we would have expected according to economic theory.

Figures 3 and 4 about here

As argued in the introduction, different degrees of employment protection can have consequences for the composition of workers. For example, a relaxation of the LIFO principle may improve employment prospects for young, older, immigrant and female workers. To examine this, we show separate figures by young (aged 18-25) and old workers (aged 50-64) and for female and immigrant¹⁰ workers. Results from such an analysis, reported in Figures 5 and 6, confirm the above results, i.e., firms do not seem to react to a relaxation of the LIFO principle.

Figures 5 and 6 about here

The analysis this far has been graphical. Although such an analysis has provided convincing enough evidence of no effect of the relaxation of the LIFO principle, we now perform a more formal statistical analysis by estimating the difference in the employment measures between firms of size eleven and ten.

3.1 Statistical analysis

When estimating the difference in the employment measures between firms of size eleven and ten, we choose to use aggregated data at size level as given in the figures shown above (see Figures 2-4). Relying on firm-level data involves problems of handling correlated errors within size which could seriously underestimate standard errors (this problem is similar to the

¹⁰ Immigrants are defined as individuals born outside of the Nordic countries.

one analysed by Moulton 1990).¹¹ Using size level data we deal with this issue since the size-error term will be part of the equation on the aggregated level. We therefore perform OLS on the following equation:

$$(2) \quad \bar{y}_s - \bar{y}_{s+1} = \delta I[size = 10] + \varepsilon_s,$$

where \bar{y}_s is the average employment measure for firms of size s , $I[size=10]$ is an indicator variable taking the value 1 if firm size is ten (0, otherwise), and δ captures the immediate jump in employment change between firms of size ten and eleven. 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 eleven and ten employees is likely to be drawn from a different distribution in contrast to all other estimated differences between adjacent firm sizes (this is exact inference as discussed by e.g., Bertrand and Mullainathan 2004). Note that we only use post-reform data (the years 2001 and 2002) in these estimations.

Figures 7-9 show the estimated differences between adjacent firm sizes, with corresponding 95 percent confidence intervals, for the three employment measures (absolute employment changes, share new hires and share separations), respectively. All point estimates of the effects except those between firms of size 2 and 3 are close to zero, with confidence intervals covering the zero. Since firms of sizes 2 and 3 are so far from the treatment cutoff value of 11, this estimated difference is not a problem for the interpretation our results. More importantly, the difference in employment measures between firm sizes ten and eleven are highly insignificant and very close to zero. For the absolute employment changes and the share of new hires, the estimated difference between firms of sizes ten and eleven can hardly be considered as draw from a different distribution than all other estimates between adjacent firm sizes. As regards the share of separations, the estimated difference between firm size eleven and ten is negative and more negative than all other estimated differences between adjacent firm sizes (except for the difference between firm size 2 and 3) – but the 95 percent confidence interval still contains the zero.

Figures 7-9 about here

¹¹ Clustering at the size level would be unreliable in our context due to the small number of firm sizes (see Conley and Taber 2007).

Taken together, the above statistical analysis confirms the picture outlined in Figures 2-4, suggesting that the relaxation of the LIFO principle for firms with less than eleven employees did not affect firms' employment behaviour at all.

3.2 Robustness analysis

Estimations based on establishment level data

We have also estimated differences between firms with ten and eleven employees for the tree employment measures, relying on monthly firm level data for the two years 2001 and 2002. Results from these estimations, presented in columns 1, 3 and 5 of Table 2, support the above conclusions (columns 1 and 3 of Table A1 in the appendix report estimates on the share of hires and separations, separately by groups). No effects are significant except for the share of separations which is negative and highly significantly different from zero (see column 3 of Table 2). Since we do not account for correlated errors within firm size in these estimations, standard errors are likely to be downward biased. The standard errors reported in Figure 9, on the other hand, were conservatively estimated, suggesting that the consistent and efficient standard errors lie somewhere between these two extreme cases. Whether the effect of the share of separations is significant or not is still an open question but since this effect seems to deviate from all the other estimated effects as shown in Figure 9, there seems to be a tendency of a decrease in separations due to the relaxation of LIFO principle.

Table 2 about here

Increasing the bandwidth

In order to investigate the concern that firm size could be measured with error, such that firm sizes ten and eleven do not distinctly separate into treatment and control, we have also estimated the difference in employment behaviour by increasing the firm size bandwidth and comparing firms with 9-10 and 11-12 employees. Estimations are presented in columns 2, 4 and 6 of Table 2 (estimations on the share of hires and separations separately by groups are reported in columns 2 and 4 of table A1). Results must be interpreted with caution, however, since by increasing the bandwidth we might introduce a bias due to a general relationship between firms' employment behaviour and firm size. Results reveal that the effect on the absolute employment change is still insignificant and even smaller in magnitude. The effect on the share of hires becomes significantly negative. This is contrary to what economic theory would suggest about the effect of a relaxation of employment protections. Instead, the

negative effect is more likely to be a result from a general positive relation between firm size and the share of hires, a tendency which could be noticed in Figure 3.¹²

Difference in differences analysis

As an additional robustness analysis we have also adopted a difference-in-differences approach. Here, we compare all firms that always have ten or fewer employees in every month before and after the January 2001 LIFO reform with firms that always have eleven or more employees before and after.¹³ Figures 10-12 show the quarterly development of absolute employment change, share of hires and separations, respectively, for small (ten or smaller) and large firms (eleven or larger). As shown by these figures, the two groups seem to have parallel trends in these employment measures, which is the crucial assumption for the difference-in-differences approach to consistently estimate the treatment effect. Most importantly, as regards all measures, there is no relative increase for small firms in the absolute employment change, new hires nor separations. This is also confirmed by the difference-in-differences estimates reported in Table 3 where all estimates are insignificant. Difference-in-differences estimates separate by group, reported in Table A2, are all insignificant.¹⁴

Figures 10-12 about here

Table 3 about here

Estimations for firms with at least three separations

All effects estimated this far have been based on average changes using all hires and separations, sometimes very few hires and separations per firms and month. For example, around 97 percent of all separations per firm are less than three. In data we can not separate between voluntary separations and separations based on “shortage of work”. If the majority of all separations are voluntary it might be the case that separations due to shortage of work – i.e., where the LIFO principle is relevant – are so few that any effects of the change in LIFO

¹² For the same reason, we have also estimated the difference between firms with 9 employees and 12 employees using aggregated data on size level and found no significant differences for any of the three employment measures. This again supports the conclusion that relaxation of LIFO principle did not increase work job flows.

¹³ Due to this restriction, 25 percent of the observations are deleted.

¹⁴ 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.

principle is disguised by voluntary separations. This possibility can be checked by looking at firms with at least three separations. For these firms, a substantial fraction of the separations is likely due to shortage of work. Figure 13 shows the average share of separations by firm size for firms with at least three separations. Figure 14 shows the estimated differences between adjacent firm sizes using Equation 1, with corresponding 95 percent confidence intervals, based on the share separations given in Figure 13. This analysis suggests that there are no effects of the LIFO principle at all.

Next, we perform the same type of analysis for older workers aged 50-64 (Figures 15 and 16), workers aged 18-25, (Figures 17 and 18), female workers (Figures 19 and 20), and for immigrant workers (Figures 21 and 22). These figures clearly show that it is difficult to find any important effects of the LIFO principle.¹⁵ The only possible effect that can be noticed is the one estimated for younger workers. Figures 17 and 18 reveal that there might be an increase in separations between firm sizes ten and eleven. Even if the 95 percent confidence interval covers the zero, this estimated difference is somewhat unusual compared to all other differences between adjacent firm sizes. It is also the case that the effect is significantly different from zero if we base the estimation on establishment level data. We should keep in mind, though, that standard errors are likely to be underestimated when relying on establishment level data since we can not cluster standard errors on firm size (see previous discussion). This is the only estimated effect which is in accordance to the economic theory of employment protection. The share of young separations for firms with at least three separations is 12 percent. Since the estimated effect is approximately 0.01, this amounts to an increase in separations for young workers in firms with at least three separations of around 8 percent due to the relaxation of the LIFO principle.

Figures 13-22 about here

Heterogeneous effect with respect to industries

Another issue related to heterogeneous effects is whether there are differential effects depending on whether firms have signed collective agreements or not. A majority of Swedish firms have signed collective agreements but the fraction of firms that do so is inversely related to firm size. As such, a non-negligible fraction of the firms present in our population have likely not signed such agreements. For example, it seems to be common knowledge that collective agreements are less common in the hotel and restaurant industry.

¹⁵ Note that 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.

Based on a two-digit industry classification we have estimated effects of the exemption rules, separate for 59 industries, using the regression discontinuity approach relying on monthly establishment-level data. When estimating 59 effects, it can be argued that, even in the absence of a true effect, we could expect to obtain significant effects in 5 percent of the cases (i.e. roughly three cases) by chance. As regards the absolute employment change, there are significant positive effects in four cases. For the share of new hires, there are only two effects that are positive and significant. For these two measures it can therefore not be ruled out that we observe only spurious effects. Consistent with previous findings of a negative effect for share of separations, there are seven industries where there is a negative relationship between the relaxation of the LIFO principle and separations. Four of these industries belong to the manufacturing industry (clothes, paper, mineral and metal products); one industry deals with publishing of papers and books; one is the wholesale and retail industry; one industry deals with data and software consulting. There is no effect on any of the three measures for the hotel and restaurant industry.

4. Discussion and conclusion

The contribution of this paper is to exploit population microdata and investigate whether a relaxation in seniority rules (the “last-in-first-out” principle) for firms with less than eleven employees had any effect on firms’ employment behaviour. This paper is related to a recent literature on US data which uses natural experiments to evaluate employment and productivity effects of changes in employment protection legislation.¹⁶

The seniority rules have received increased attention recently, from a political as well as an academic point of view. Opponents have argued that these rules induce inefficient adjustment costs on firms’ employment decisions. Several aspects of this principle have often been overlooked in this debate, however, in the sense that there are several ways for firms to circumvent the LIFO principle. The hypothesis in this paper is that the seniority rule has played out its role due to the possibility for firms to circumvent the legislation e.g., through negotiations with unions, and because the last-in-first-out principle only holds if the workers with longer seniority have required qualifications for the remaining jobs; the employer decides about required qualifications.

The break in the policy for firms of size ten provides a natural setting for analysing the impact of the relaxation of the LIFO principle using a regression discontinuity approach. Using population data on all firms with two to twenty employees, we find that the change in the LIFO principle had no effect at all on size distribution of firms and we do not find any general effects on employment fluctuations or on hirings and separations. The only effect that can be noticed is an increase in separations for young workers aged 18-25.

The result in this paper is not only interesting because it is the first paper evaluating the consequences of the LIFO principle on firms’ employment behaviour; in fact, very few empirical studies on employment protection and employment behaviour using European micro-data exist. One main concern with cross-country studies is to obtain comparable “strictness” measures across countries. If the Swedish seniority rule contributes to making the Swedish employment protection stricter in these cross-country studies, this paper shows that such a measure would seriously over-estimate strictness. We therefore argue that it is important for further empirical studies to focus on certain aspects of employment protection.

¹⁶ See e.g., Autor (2003); Autor *et al.* (2004, 2006); Autor and Kugler (2007); Kugler (2007); Kugler and Pica (2006); Kugler and Saint-Paul (2004); Kugler *et al.* (2003).

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Table 1. Means

<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	0.065	0.068	0.064	0.062	0.059	0.056	
Share new hires	0.038	0.041	0.042	0.042	0.042	0.042	
Share separations	0.037	0.040	0.040	0.040	0.040	0.040	
<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	0.055	0.052	0.053	0.052	0.052	0.051	
Share new hires	0.041	0.041	0.042	0.042	0.042	0.042	
Share separations	0.040	0.040	0.041	0.041	0.041	0.041	
<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	0.052	0.052	0.051	0.052	0.051	0.049	0.050
Share new hires	0.042	0.042	0.043	0.043	0.043	0.042	0.043
Share separations	0.041	0.041	0.041	0.041	0.041	0.041	0.041

The number of firms are measured in November 2001.

Table 2. Regression discontinuity estimations based on monthly establishment data during the period 2001-2002.

	<i>Employment change</i>		<i>Share hires</i>		<i>Share separations</i>	
	<i>Bandwidth</i>					
	<i>10-11</i>	<i>9-12</i>	<i>10-11</i>	<i>9-12</i>	<i>10-11</i>	<i>9-12</i>
<i>All</i>	0.0005 (0.0007)	0.0002 (0.0005)	-0.0002 (0.0005)	-0.0008* (0.0003)	-0.0010* (0.0004)	-0.0011** (0.0003)
Observations	240,843	491,789	241,405	492,975	241,405	492,975

Estimations include month and year dummies. Standard errors are clustered at the firm level. ** and * indicate significant at the 1 and 5 percent significance level, respectively.

Table 3. Difference-in-differences estimates of the 2001 reform in the LIFO-principle.

	<i>Employment change</i>	<i>Share hires</i>	<i>Share separations</i>
<i>All</i>	0.0007 (0.0010)	0.0001 (0.0006)	-0.0011 (0.0005)
Observations	2,210,326	2,212,057	2,212,057

Estimations include quarter dummies. Moulton standard errors cluster on small and large.

** and * indicate significant at the 1 and 5 percent significance level, respectively.

Figures

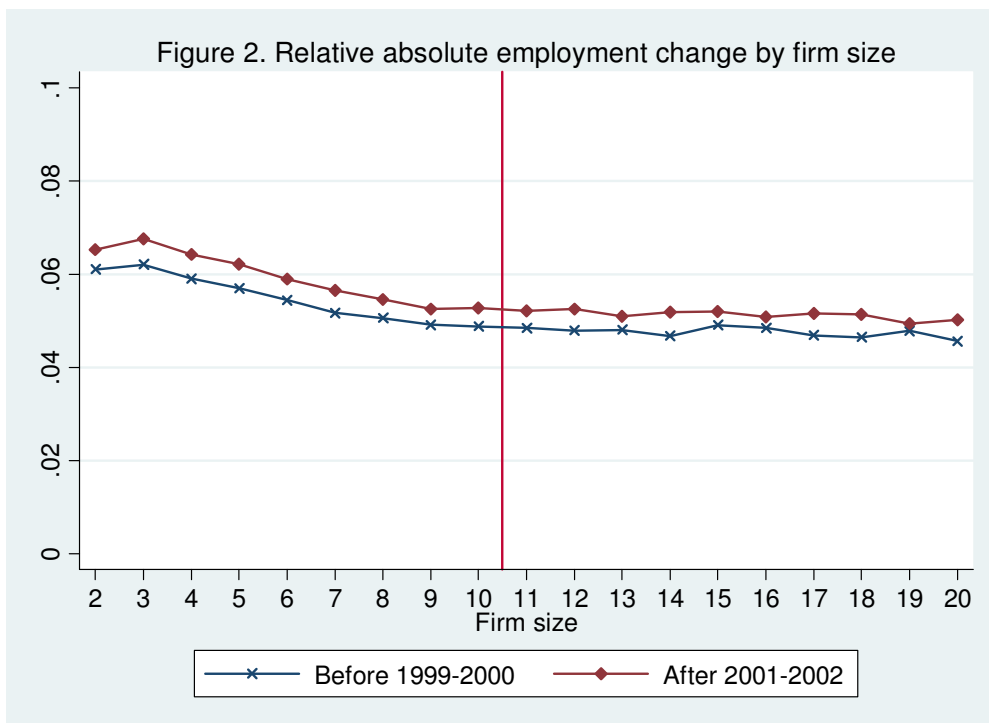
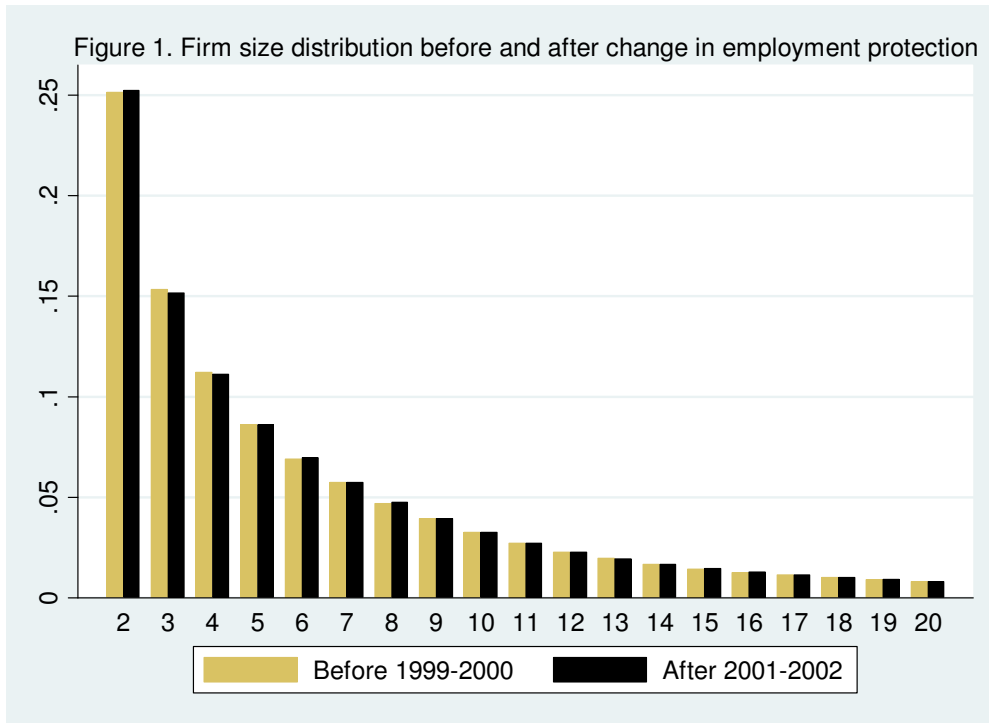


Figure 3. Share new hires by firm size

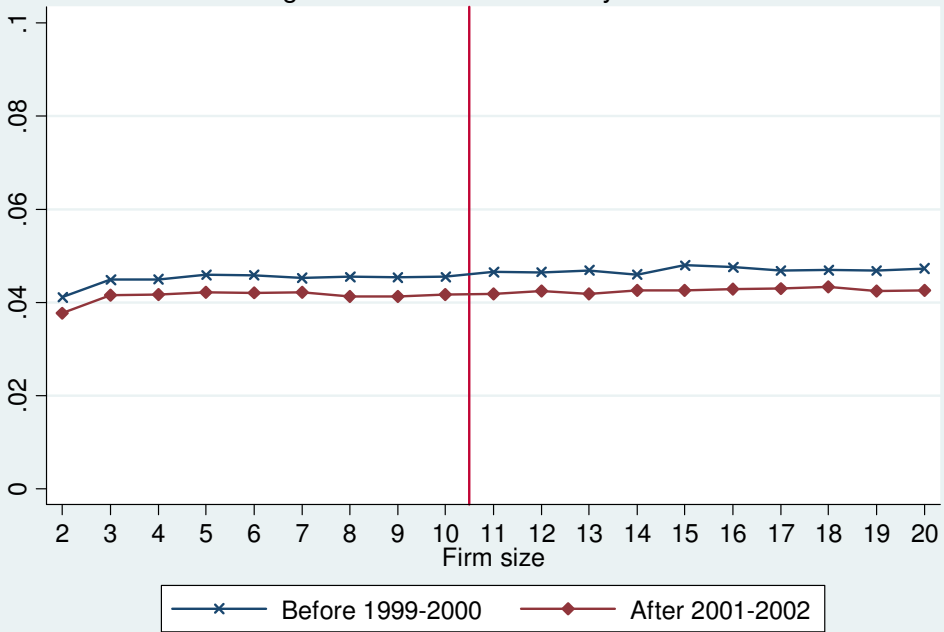


Figure 4. Share separations by firm size

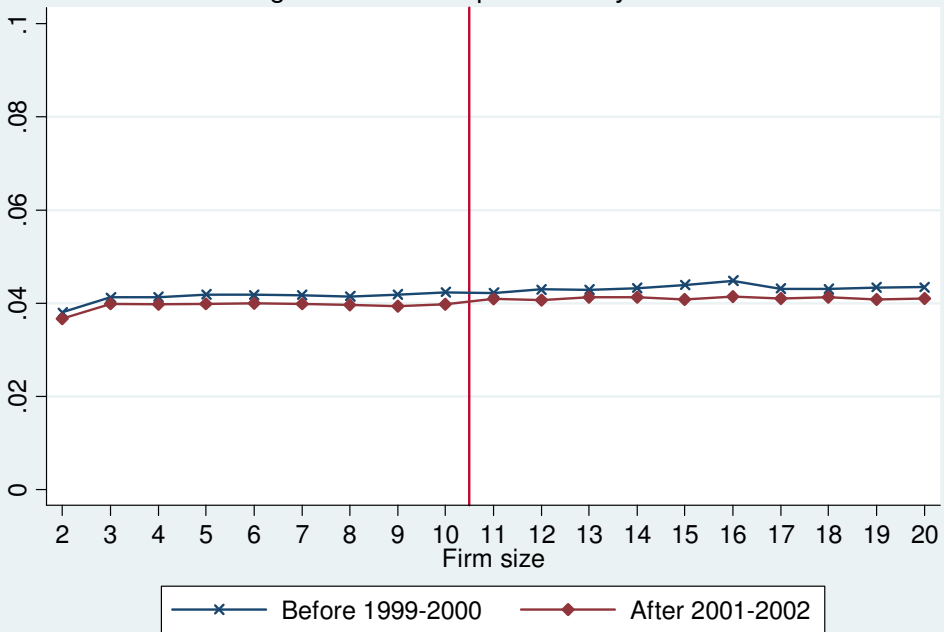


Figure 5. Share new hires and separations for young and old by firm size

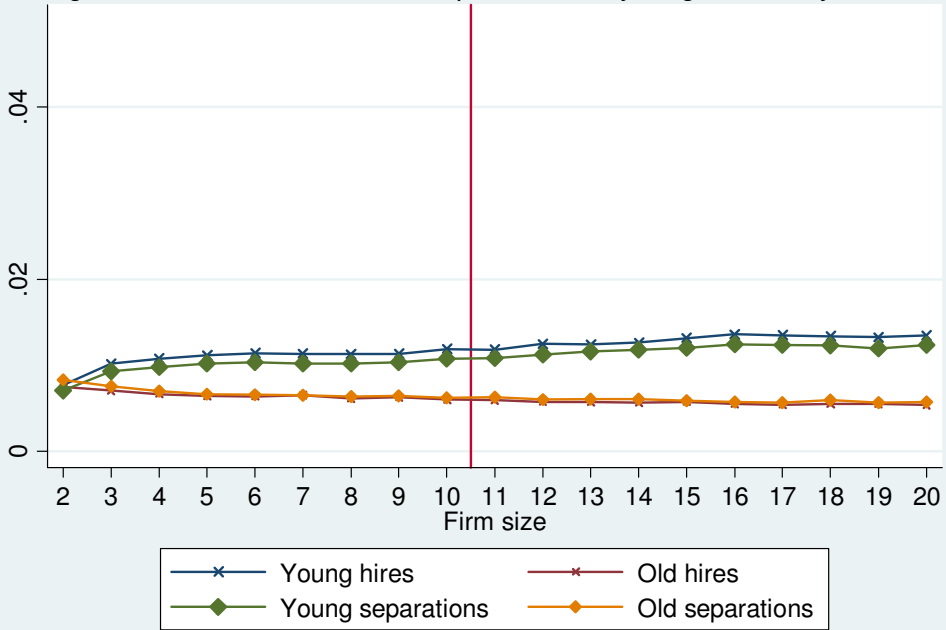
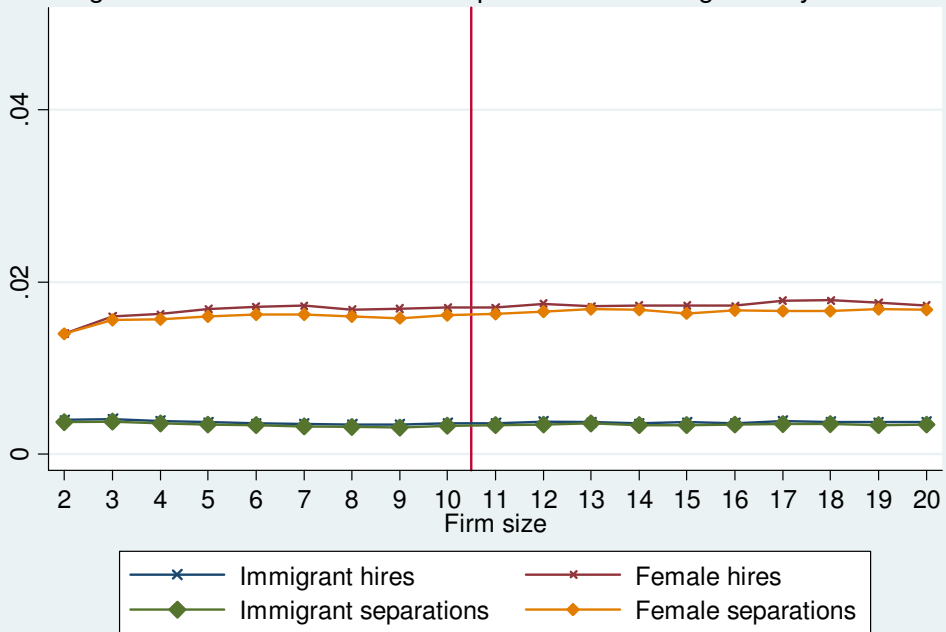
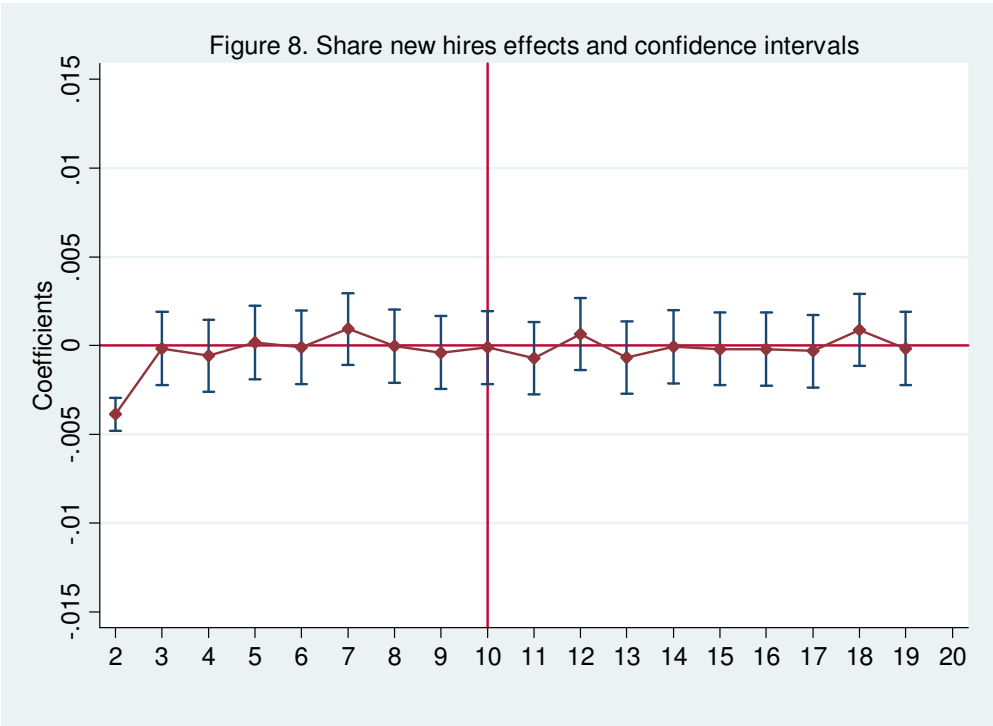
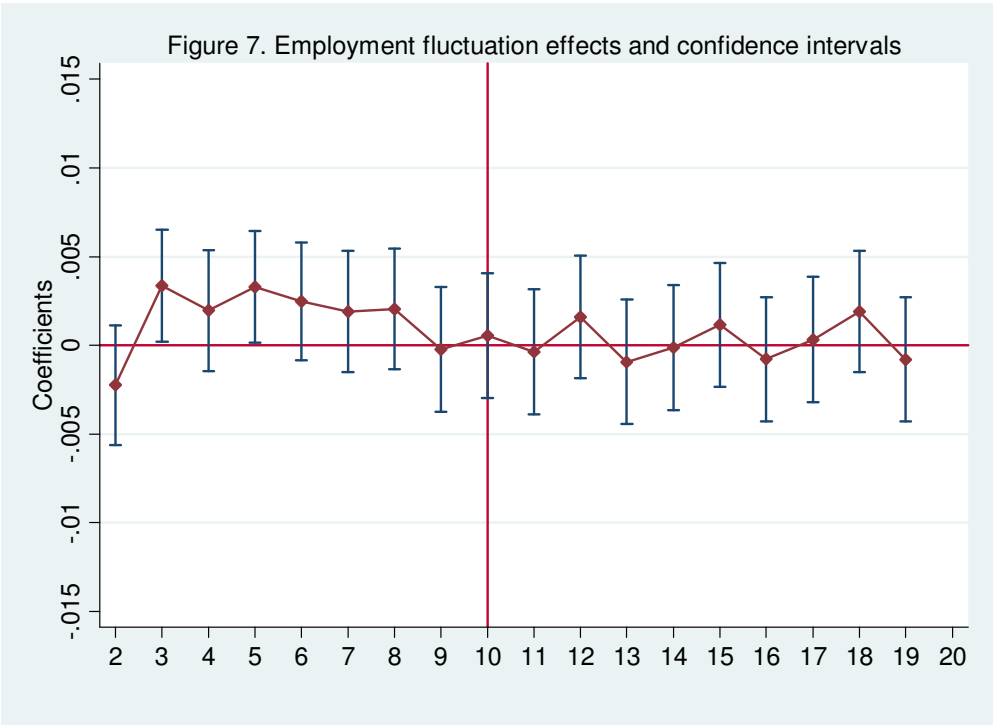


Figure 6. Share new hires and separations for immigrants by firm size





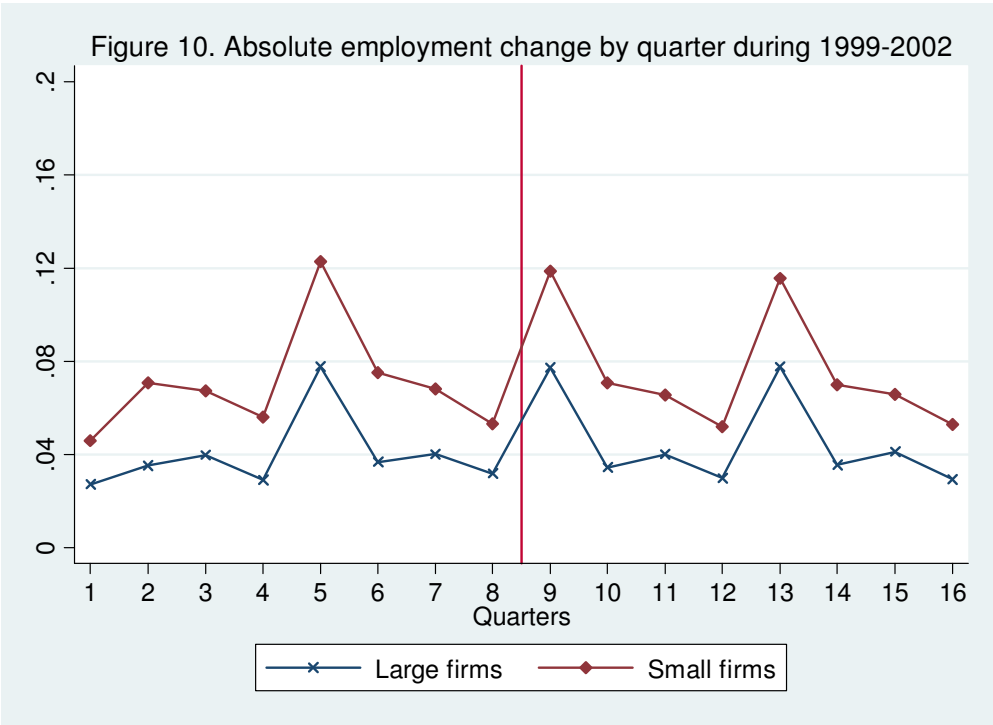
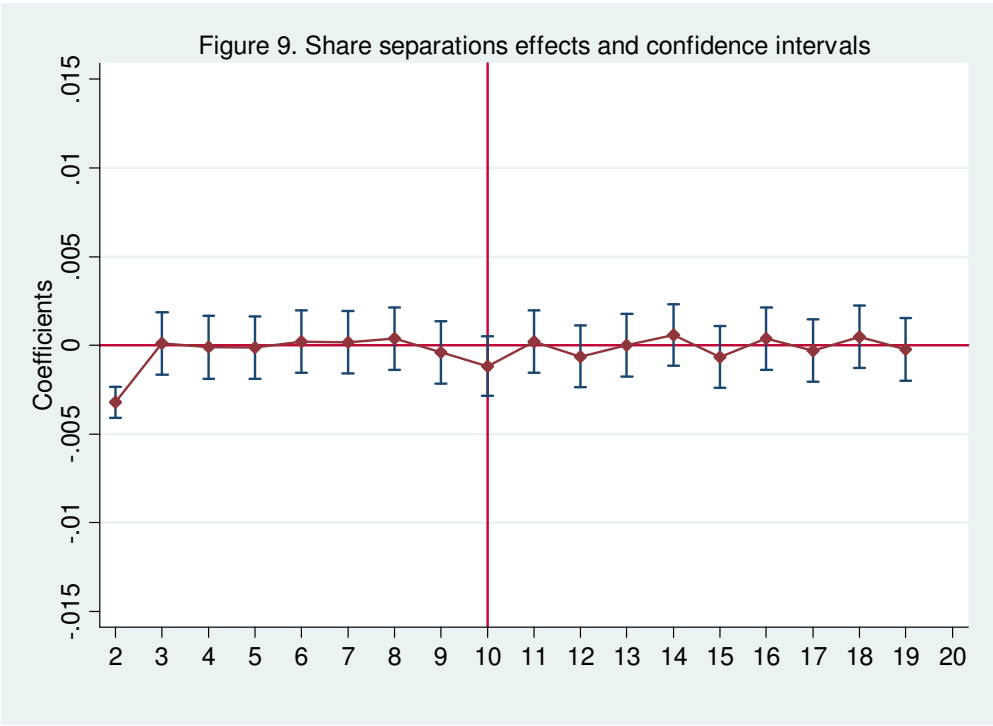


Figure 11. Share new hires by quarter during 1999-2002

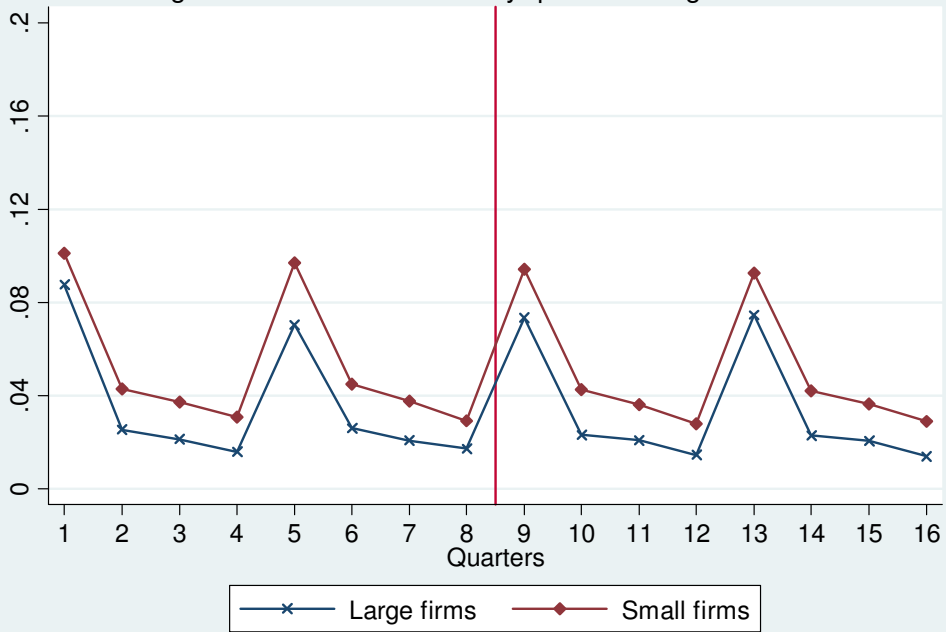


Figure 12. Share separations by quarter during 1999-2002

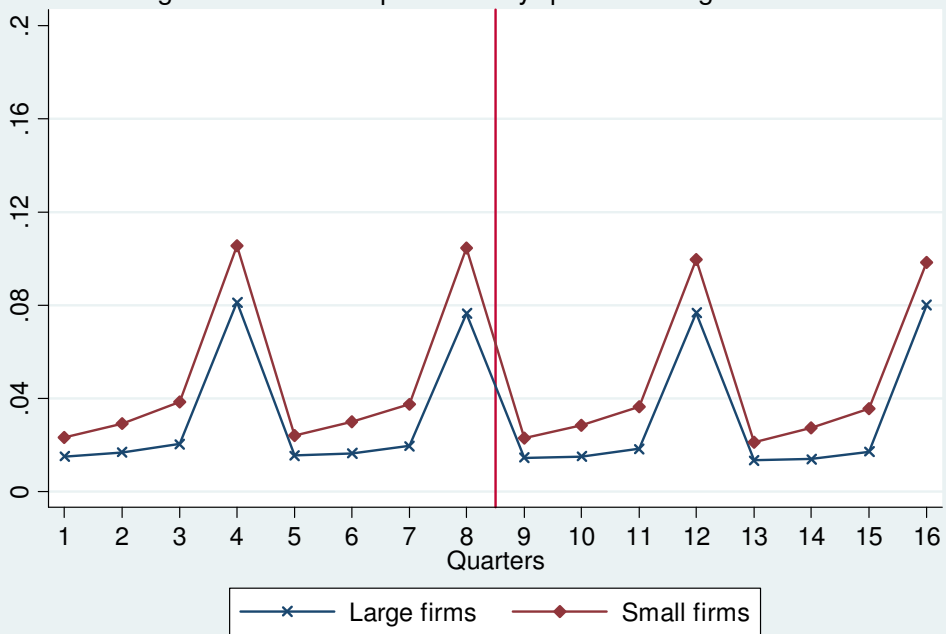


Figure 13. Share separations by firm size. Firms with at least three separations

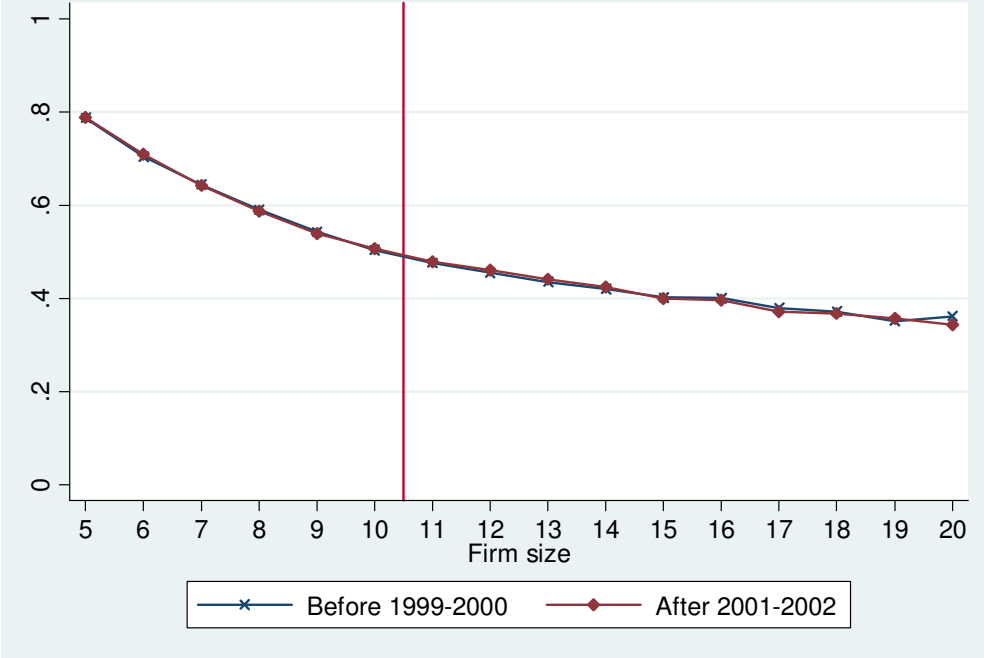


Figure 14. Share separations effects and confidence intervals

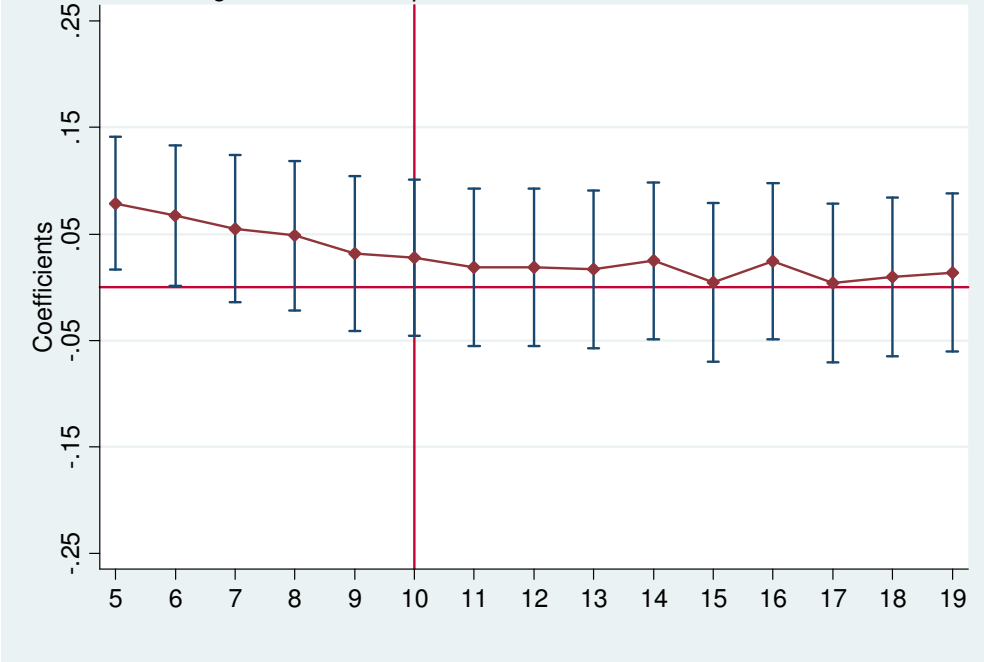


Figure 15. Separations aged 50-64. Firms with at least three separations

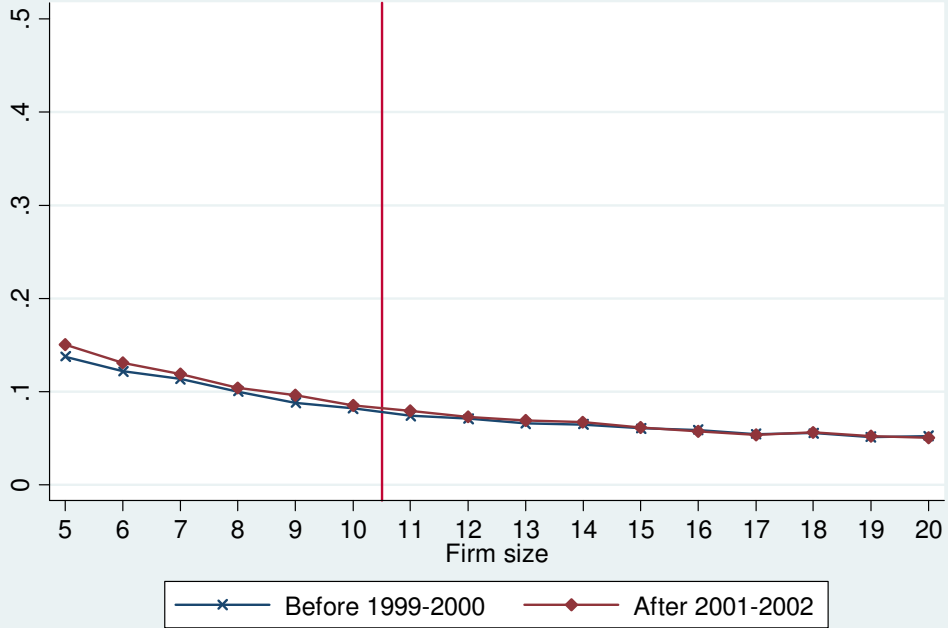
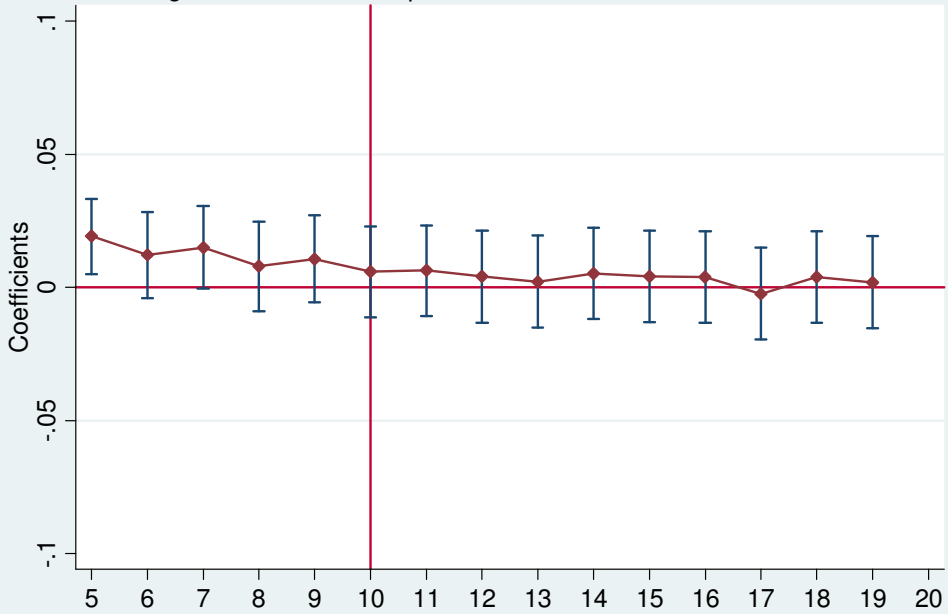
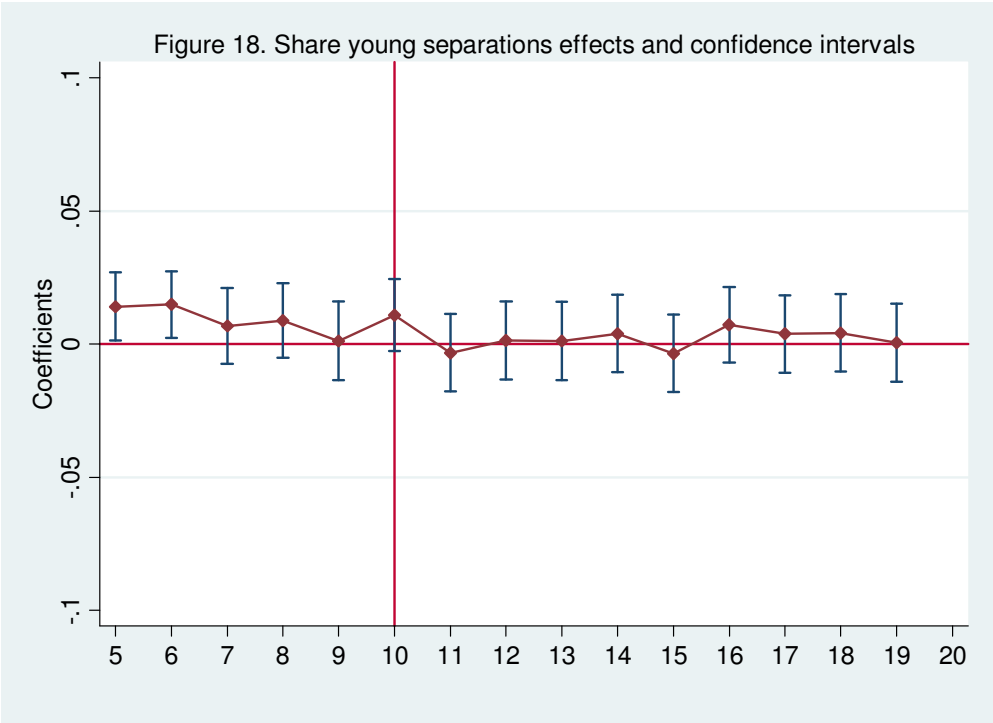
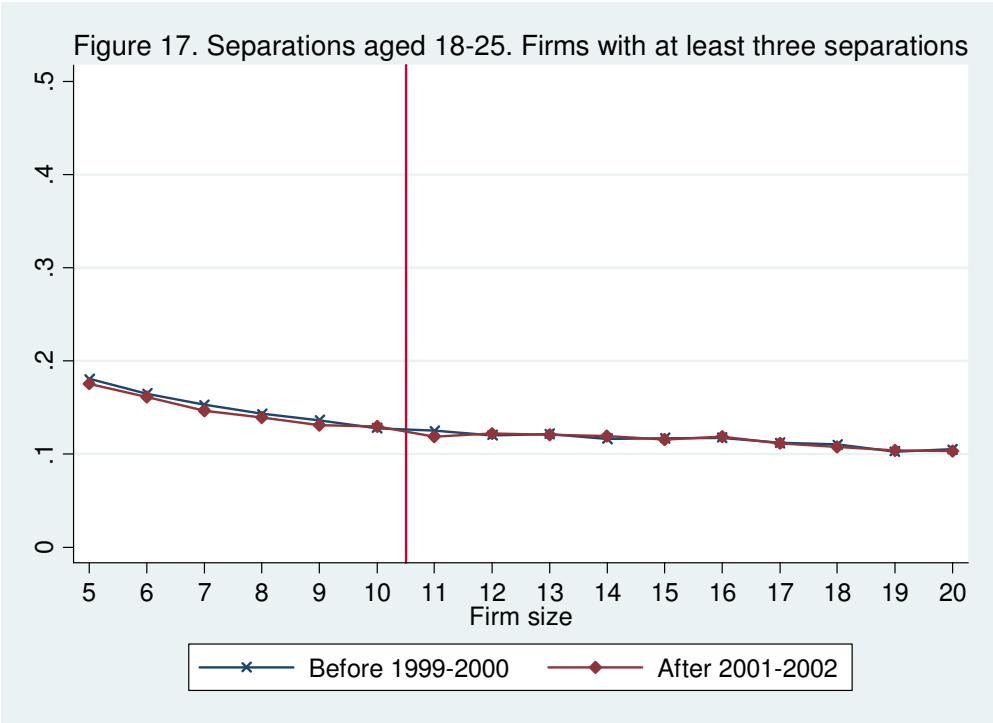


Figure 16. Share old separations effects and confidence intervals





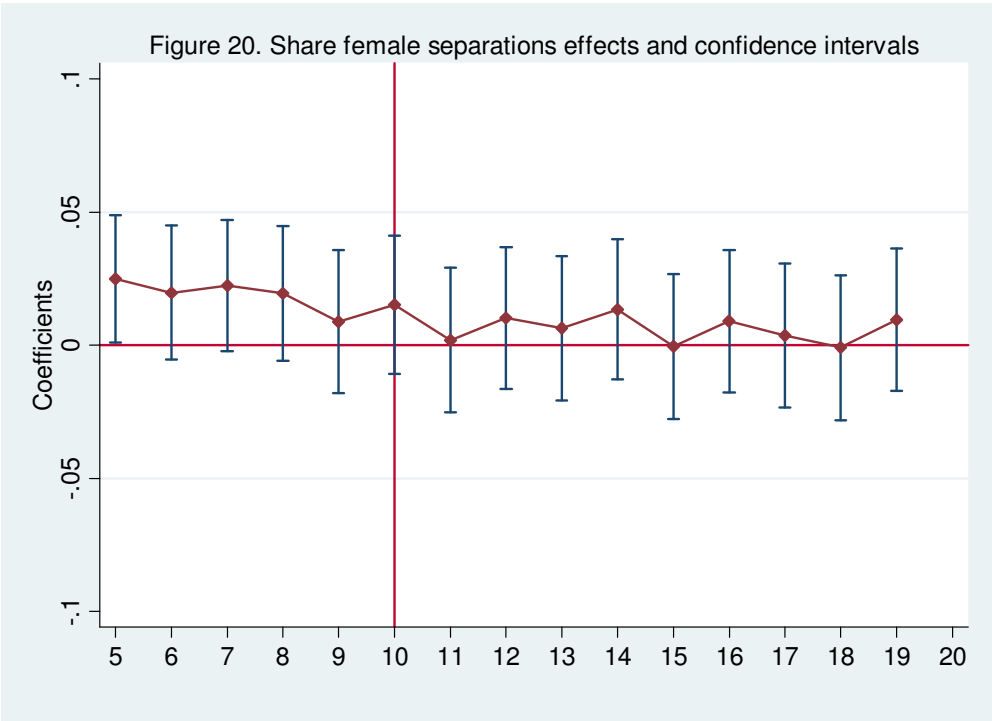
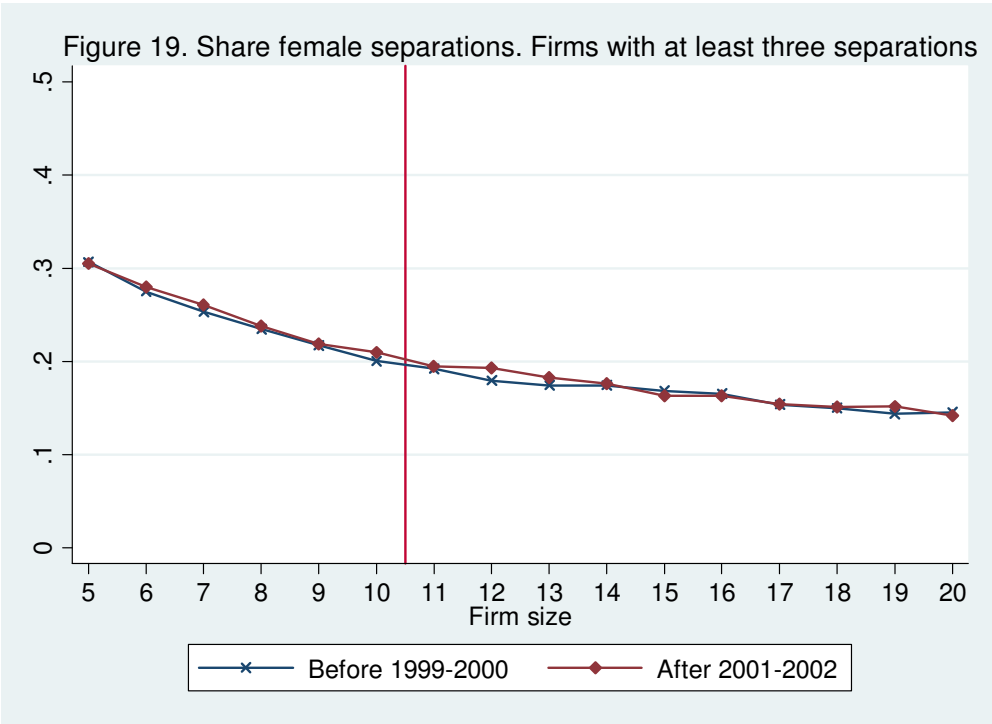


Figure 21. Share immigrant separations. Firms with at least three separations

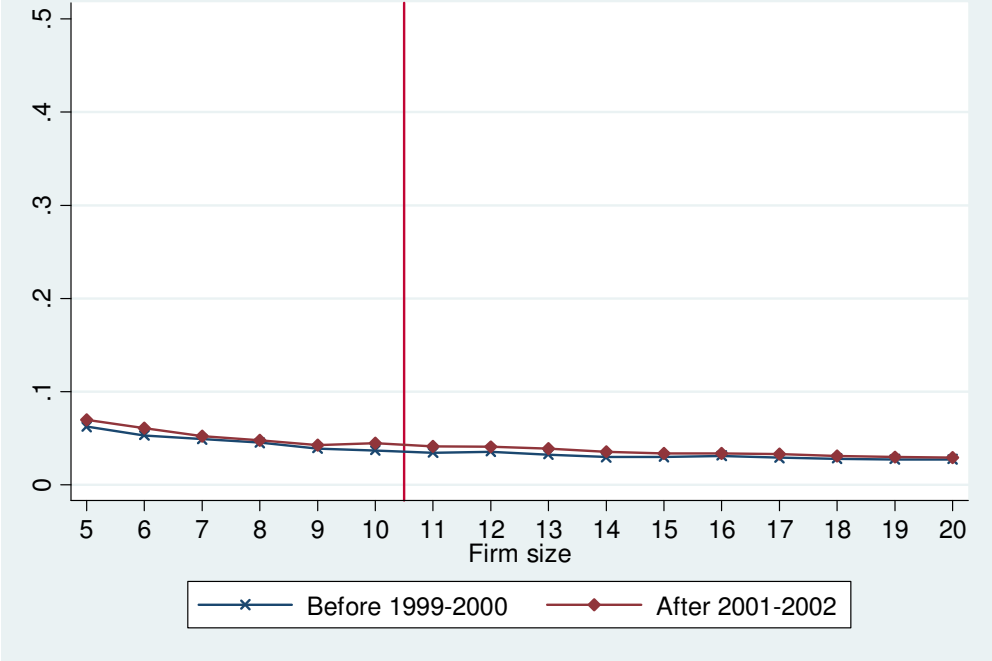
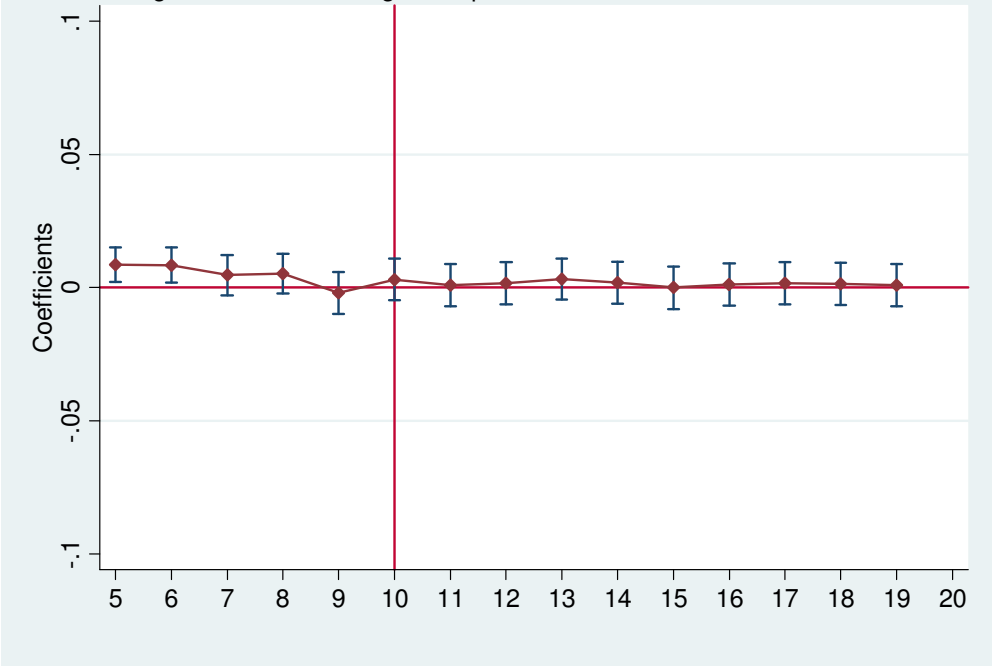


Figure 22. Share immigrant separations effects and confidence intervals



Appendix

Table A1. Regression discontinuity estimations based on monthly establishment data during the period 2001-2002.

	<i>Share hires</i>		<i>Share separations</i>	
	<i>10-11</i>	<i>9-12</i>	<i>10-11</i>	<i>9-12</i>
		<i>Bandwidth</i>		
<i>Young</i>	0.00005 (0.0002)	-0.0006** (0.0001)	-0.00004 (0.0002)	-0.0005** (0.0001)
<i>Old</i>	0.00004 (0.0001)	0.0003** (0.0001)	-0.00007 (0.0001)	0.00002 (0.0001)
<i>Immigrants</i>	-0.00001 (0.0001)	-0.0002* (0.0001)	-0.00008 (0.0001)	-0.0002** (0.0001)
<i>Females</i>	-0.00005 (0.0003)	-0.0003 (0.0002)	-0.0001 (0.0003)	-0.0004* (0.0002)
<i>Observations</i>	241,405	492,975	241,405	492,975

Estimations include month and year dummies. Standard errors are clustered at the firm level. ** and * indicate significant at the 1 and 5 percent significance level, respectively.

Table A2. Difference-in-differences estimates of the 2001 reform in the LIFO-principle.

	<i>Share hires</i>	<i>Share separations</i>
<i>Young</i>	0.0003 (0.0003)	-0.0001 (0.0002)
<i>Old</i>	-0.00003 (0.0002)	-0.0002 (0.0002)
<i>Immigrants</i>	0.0002 (0.0002)	0.00003 (0.0002)
<i>Females</i>	-0.0003 (0.0004)	-0.0003 (0.0003)
<i>Observations</i>	2,212,057	2,212,057

Estimations include quarter dummies. Moulton standard errors cluster on small and large.

** and * indicate significant at the 1 and 5 percent significance level, respectively.